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UNIV. OF MICH.

THE

PHILOSOPHICAL MAGAZINE:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

GEOLOGY, AGRICULTURE,

MANUFACTURES AND COMMERCE.

BY ALEXANDER TILLOCH,

M.R.I.A. F.S.A. EDIN. AND PERTH, &c.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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Johnson
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THE

THE
PHILOSOPHICAL MAGAZINE.

I. *On the Means of counteracting the Effects produced by the Formation of the Gases found in Coal Mines.*

To Mr. Tilloch.

SIR, I AM induced to request you to lay the following pages before the public, from a desire to put a subject of much importance in a train for investigation. Little has hitherto been written on the subject, calculated to do away the frequency of the dreadful accidents to which miners are incident; and what has been written has come from the pens of those who were not every way qualified to do justice to what they have taken in hand.

As the *manner* of working mines varies according to circumstances, many things herein will appear to some as redundant, while to others much will seem to be omitted. But let it be remembered that I lay no claim to general knowledge: what will be advanced is chiefly the result of practical observation in a particular district.

To men of considerable experience in mining, probably, nothing will be found worthy of attention: but if a few individuals should chance to reap any benefit, or should it prove the means of preventing one single accident, my end will, in no small degree, be obtained. With regard to the experimental part, it may be proper to observe, that my situation renders it in many instances impossible to procure apparatus of the best kinds; and hence, if any inaccuracies should chance to be discovered by the scientific reader, it is hoped ample allowances will be made.

In endeavouring to accomplish the important and desirable object before us, we ought to adopt those measures which have a tendency to prevent the accumulation of the noxious gases; for wherever they are suffered to accumulate there is continual danger*.

I do not know of any method, neither do I think it possible to devise one, at all comparable to that with which Nature has wisely provided us, namely, atmospheric air.

This effects itself so abundantly, and requires such simple arrangements to convey and apply it to the purpose, that we may be considered as extremely deficient in our duty, if we do not make the best use of it, before we turn our attention to matters of speculation. In fact, no contrivances can supply its place. A salubrious atmosphere is so necessary to the healthy action of the human system, that in mines where a due proportion of its vital principle in a given space is sometimes wanting, it is the cause of numbers being subject to disease†.

[illegible] which tends materially to increase the expense of working a mine, or which throws considerable obstacles in the way of getting and removing the minerals out of it, [illegible] on these accounts in the present day be [illegible]. In this light we may reckon the schemes for [illegible] or destroying the gases by chemical agency; such, for instance, as the use of oxy-muriatic acid for destroying fire-damp, proposed by Dr. Trotter‡. The expense attending such a process would be enormous, the mode of applying it unmanageable, and the remedy, if it could be effected, [illegible] would be worse than the disease. The quantity of hydrogen gas or fire-damp, evolved during one day in a single colliery, is almost incredible to persons unacquainted with the fact; and as we cannot reasonably expect to procure chemical re-agents at a much cheaper rate than this gas can be obtained artificially, the idea of applying them to this purpose must therefore be entirely abandoned.

There are also other modes which might be adopted, such as procuring light accompanied by a heat below the temperature requisite to fire the inflammable gas; or by forming an insulated atmosphere round the flame of a lamp or a candle; admitting the air of the mine thereto in such proportion, and so diluted with the insulated and incombustible air, as to prevent explosion.

---

* This requires no proof. Accidents can never happen but where there is an accumulation of gas.

The idea of [illegible] is too ridiculous to deserve the least notice.

† Working in mines, where the [illegible] is very prevalent, is the occasion of violent headachs, and [illegible] even [illegible] is considered as producing asthmas, [illegible], &c.

‡ "A Proposal for destroying the Fire- and Choke-damps of Coal Mines, &c. &c. on the Principles of modern Chemistry, by T. Trotter." Newcastle.

This last plan has often been proposed; and I understand that a gentleman of Sunderland has completed a contrivance of this kind. Although these modes of obtaining the end in view are in a great measure free from the difficulties inseparable from reagents, yet there are others which render them also objectionable.

With regard to the first method, the only one with which I am acquainted is the *steel mill* as it is called, which consists of a number of flints, so placed with respect to a circular steel pulley or wheel, to which a handle is fixed, that, when it is turned, a considerable quantity of light is given out. The manual labour attendant on this plan, together with the expense of renewing the apparatus, (to say nothing of the imperfect light procured,) renders it inapplicable to ordinary purposes. Indeed I believe it is seldom or ever used, except in peculiar situations, when a part of the mine is incapable of ventilation, without great expense or difficulty. Moreover, it cannot be considered as entirely free from danger of exploding the gas; and that this has actually happened, I am well informed.

Reflectors are sometimes used to convey light, in *thrilling* (or thurling) between two *shafts* of no great depth, or for other works of a like nature, where the quantity of *fire-damp* is considerable, and incapable of being carried off with facility.

Concerning lamps constructed so as to burn in an insulated atmosphere, nothing deduced from practice can be said. They must necessarily be expensive, and extremely liable to be broken by pieces of coal, &c., flying from the workmen, or from accidental falls; and should this happen in a part of the mine where the gas was accumulated in a quantity sufficient to render the use of such a lamp necessary, what would be the consequence? Besides, complicated machinery is ill calculated for ignorant and careless miners: an instrument of this nature must needs require nice adjustment and frequent trimming, which last could not be done with perfect safety where any danger was to be apprehended. In short, men who have ever been accustomed to the use of a candle, and a piece of clay as the ready means of fixing it where circumstances require, will not willingly be incumbered with a lamp, even of a common construction. There is another serious objection to e urged against all the foregoing modes of obtaining light, amely, the pre-supposed want of a sufficient quantity of xygen in the atmosphere; this, added to the positive bad ualities of the gases present in such situations, would reatly endanger the health and lives of the workmen.

Let us now turn our attention to the more practicable nd effectual plan of ventilation; and in pursuing this sub- ct we will commence with the first opening of a colliery.

It should be understood, that hydrogen and carbonic cid gases are often met with in *sinking shafts* of consider- ble depth, as in many instances such are evolved for a ngth of time in great quantities from fissures in the strata. two shafts are sunk near together, *thrilling* occasionally rough from one to the other is of great utility; but in me instances this is by no means sufficient to produce the sired effect. Should this be the case, *air-pipes* of large ameter must be extended from the bottom of the sinking aft, through the last *thril* into the adjoining *shaft*, in e bottom of which, providing it is not deeper than the *ril* last made, (in which case a scaffold will be necessary,) fire ought to be constantly kept burning: by this means re air will descend the sinking shaft, carrying with it the tricated gases up the *pipes* into the adjoining *shaft*, hose column of air will be rarefied by the fire. I conceive is method to be superior to that of building a cupola, in hich a fire is placed, upon the ascending shaft; because, the chimney be not somewhat near the size or diameter the shaft, the current of air will be impeded thereby. esides, a rarefied column of air in a deep shaft has a eater tendency to produce a quicker current, than the lumn of air in a chimney, beyond doubt.

When no communication can readily be had with an- her shaft, it is sometimes customary to partition the nking shaft with boards down the middle. This, though simple, is an expensive plan; and if the temperature casioned by the workmen is the only means of pro- ucing a circulation, how must it be carried on, when the en are not at work? I am aware that where there are umps working, these would, in some measure, remedy this eming defect, especially by occasionally turning or spill- g the water lifted down from the top to the bottom of e shaft, for a short time.

If wooden air-pipes, of about 15 inches diameter, are t down the sinking shaft, and the upper ends of them are ced to the ash-pit of the engine boiler, (which, for an gine capable of lifting the water of a deep shaft, is of considerable size,) and the ash-pit is purposely constructed, by being closed in front, and provided with a door, through which the ashes may be removed, all the air required for combustion must necessarily be forced up the pipes out of the shaft, owing to the great rarefaction existing in the ash-pit and engine chimney. The fire bars should in this case be far asunder, and be kept very clean, in order to allow a greater quantity of air to pass through their interstices.

Another plan consists in the use of *blast cylinders*, worked by the engine. Though these are generally constructed so as to force air down the pipes, yet I should recommend the reverse mode, because it will frequently occur that the air will be foul in some parts of the shaft and yet be salubrious at the bottom: besides, the downward blast prevents the conveying feeders or jets of gases from particular fissures directly out of the shaft, by turning them into the air-pipes. Of the methods above detailed, the second, by thrilling, appears to me to have a decided advantage, from its constant and extensive powers of ventilation.

We will now suppose the shaft or shafts to be sunk down, and a direct communication formed in the mine, and shall now touch upon such a course of proceedings as ought to be adopted to ensure the working of the mine with safety.

It is generally customary to begin with *driving* two parallel *headings* or levels, pretty near to each other, intersecting the pillar betwixt them by *thrils*, as often as occasion requires, taking care to make up or closely stop the last but one after them in succession. One of these *levels* is called the *air-way*, or wind-gate; and the other, the waggon-way, or passage through which the produce of the mine is conveyed to the shaft; and these are extended to the boundary fixed upon for the workings.

When other headings are made to communicate with these at right angles, or in any other direction, it is necessary to place a close-shutting door, in order to turn the current of air, out of its course, through one branching heading, and back along another. But there is such a variety in the modes of working mines, each of which is adapted to peculiar circumstances, that it would be tedious to detail them particularly; neither is it absolutely necessary.

As a great deal of expense may be spared and many accidents avoided, in situations where *fire-damp* is prevalent, by

having these headings properly constructed, it may not
amiss to state generally what should be observed in
rming them.

The *first* thing to be attended to is, to have the air- and
ıggon-ways of a sufficient width and height, so as to ad-
it an abundant quantity of air to carry off the extricated
ses; and in proportioning them, it would be well to attend
the extent and number of air-ways, &c., or the area
mine required to be got or worked to a shaft; and like-
ise to the quantity of gas likely to be evolved. In thin
ines of coal, this ought to be an important consideration.
o passages or headings, in situations where the workings
e likely to be extensive, ought to be less than four feet
d a half square. There is a lateral friction occasioned
air passing through apertures of every description, and
is is in proportion to the area of their sides, if smooth;
d is increased by the roughness and irregularity of the
rface passed over, as well as by contractions*. There
e other obstructions to a regular circulation, such as the
ening of doors, men or horses, &c. &c., passing in a
rection contrary to the current of air; and these circum-
ınces render capacious air-ways indispensable.

Secondly. Every part of such air- and waggon-ways should
made as secure as possible; for, when they are left in an
ıfinished and temporary state, they must needs, ere long,
repaired, at an additional expense, to say nothing of the
ndrances and accidents to which such slovenly manage-
ent is liable.

Thirdly. Thrills should be made nearly as large as the
-ways; a number of contractions of this kind would ren-
r a capacious air-way of no service. When they are
de up, great attention should be paid to their being air-
ht, otherwise the air will be lost, and the current thereby
nsiderably diminished.

Fourthly. Doors for the purpose of turning the air
ould be well made, very tight, and firmly fixed. They
e sometimes constructed with springs, &c., so as to shut
themselves; but as the most simple contrivances are
und to answer best, these do not generally obtain. There
a method of construction, which appears to me to be
pable of answering very well, namely, swing doors, which
properly framed together, and suspended by double cen-
s, will shut of themselves, and open with little trouble;

---

* Any person may satisfy himself of the truth of this, by noting the re-
ar current of air through the windings of a coal mine, and then causing
air to pass directly between the two shafts.

they should be hung at about two-thirds of their length from their bottom end, and counterbalanced so as to act against the current of air. In mines where horses are used, a man will thrust them open without delay, and they will immediately shut after him. Common doors, such as shut one way, are on many accounts the best; but they are apt to be left open in situations where persons are not expressly appointed to attend them, and indeed blown open by the air acting in a contrary direction, owing to the sudden impulses of blasts, or shots fired, &c. &c.

Lastly. Great care ought to be taken to preserve the air-ways, &c. in good repair; and should any part of them give way, no time must be lost in securing them, and removing the rubbish, that there be no obstruction to free ventilation *.

When all the headings are completed, or as many of them as are deemed necessary, it is customary to proceed in working the mine; but, for reasons already stated, it would be departing from my original intention to say anything upon this head.

Indeed, to an intelligent miner, who has an abundant supply of air at his back, little difficulty will arise in turning it effectually to his purpose.

Ventilating the *waste* or old *workings*, so as to prevent an accumulation of damp, is a matter of importance. In thin mines it is not necessary to do it for any length of time, as the *roof* and *floor* soon gradually close together: in others, where the *roof* is rock, and of such thickness as to stand under any circumstances, it may be practised with ease: it is in mines of four feet thick and upwards, whose superior strata are not capable of supporting themselves for a considerable width, that the greatest difficulties arise. To leave sufficient pillars of coal would frequently be to bury the profits of a colliery, and at the same time destroy an amazing quantity of valuable fuel.

Thin pillars are in general required to ventilate the workings, and these are for the most part left: where they happen to stand until a *general weight* or settlement of the

---

* I have known instances, where the air-ways have been so much neglected in this particular, as to put a complete stop to the circulation of air; and, as might very naturally be expected, several misfortunes happened in consequence. The ill effects attending such unpardonable inattention are almost incalculable:—loss of men's lives, loss of time, expense incurred in repairs rendered more difficult to accomplish from damages sustained by explosions; the invariable advance in men's wages, as some compensation for the great dangers to which they are exposed, may all be placed to this account.

superior

ɔerior strata takes place, and the *waste fills up*, it is ll, as then no damp will accumulate, and thereby en- nger the lives of the neighbouring workmen.

Too much caution cannot be observed, by men who are rking near such *old hollows*, never to introduce their ıdles into any cavities which may happen to be pene- ted, but immediately extinguish them and retire, until pper means can be adopted to ensure safety. What ac- lents have occurred for want of this simple precaution *!

It may be, and in fact is, at times necessary, owing to rease of temperature or levity in the atmosphere, to ob- n a greater supply of air than is wanted in the ordinary ırse of things, or, more properly speaking, to preserve an iform circulation.

Here we have an admirable contrivance, whose powers unlimited; and this consists in rarefying the column of in the ascending shaft by means of a fire. There ap- ırs to be a difference of opinion as to the best mode of omplishing this. Some build a chimney or cupola upon near to the shaft, making the top thereof so close that air must pass up the chimney, at the bottom of which re is placed: others hang, by means of a chain, &c., ircular grate filled with burning fuel, down the shaft; l some fix a grate in the bottom of the shaft, in a ıding, driven a few yards for the purpose; while others a groove in the shaft side, which, when built up in nt, forms a chimney.

If we are to judge of the comparative merit of the fore- ng plans from their respective powers of rarefaction, must give to the last method but one a decided prefe- ıce. By it we are enabled to produce the greatest diffe- ıce in weight between the two columns of air, on which ıtilation solely depends; and if the grate be placed a erable height, and opposite to the current of air passing o the shaft, we shall obtain the quickest draught for the . The chimney not only greatly increases the effects of fire, but is a sort of guard to the ropes, which might damaged by coming too near the naked fire.

I shall conclude this imperfect sketch, with offering a general remarks, which, to those who put them in ıctice, will not be found useless.

In all cases where danger from fire-damp is to be ap- hended, the miners should never work naked. It is not

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This is a very common trick of miners, owing to an idle curiosity; and too many have paid dearly for their temerity and want of considera-so much the *depth* of the *burn* as the *extent* of it, which proves fatal: a covering of thin flannel would be found almost invariably to confine the injury to the face and extremities. Some miners cannot afford to do this, and others will not, considering clothing as an incumbrance; but where men's lives and the interest of the proprietors are so much at stake, it ought to become a matter of compulsion.

Should it so happen, that through some unforeseen accident an explosion takes place, the persons in immediate danger ought to throw themselves down, covering themselves as well as the spur of the occasion will permit, and then with all speed make their nearest way to that part of the mine from whence the air proceeds, taking care to creep as low as possible: by this means, suffocation from the *azote*, which remains after combustion, will be avoided. Should any one be so unfortunate as to fall thus for want of respirable air, or by the choke-damp, every exertion ought to be made to cause the air to circulate through the place, so as to enable the sufferer to be brought out; and when this is effected, resuscitating means should be immediately employed. Instances often occur, of men being stifled by the azotic gas, who were little or none the worse from the effects of the explosion.

In every instance, where a miner penetrates any new or neglected cavity whatever, the precautions before mentioned ought to be strictly observed, whether the mine be much subject to *fire-damp* or not.

The practice of laying scaffolds over or in a shaft, in order to do some necessary repairs, without being cautious to ventilate the underside, should be carefully avoided; terrible explosions have been known to follow this negligent practice.

These and other similar remarks, which will occur to every one conversant with mining business, if properly modified, would form a code of rules, with which every miner should be acquainted.

December 19, 1809.

⁂ We shall at all times be glad to receive papers of the above description from practical men, on the subject of their pursuits, and shall think little of our trouble in rounding a period, or correcting grammatical inaccuracies, for such as may chance to need this assistance, owing to the devotion of their time to other important objects.——EDITOR.

On Expectorated Matter. By GEORGE PEARSON, *M.D., F.R.S. Abridged from the Philosophical Transactions for* 1809.

THE attention of physiologists has been very much
thdrawn, for the last half century, from the consideration
the different states of the circulating and secreted fluids,
consequence of the opinion that the nervous and fibrous
muscular systems can afford satisfactory interpretations
the phænomena of living beings; and on account of the
gust produced by the visionary properties and groundless
potheses, originating in the humoural doctrines of Galen.
t late experiments have manifested, that various things
en into the stomach can be made at pleasure to produce
nsiderable effects, by impregnating sensibly the blood
d urine, as well as the milk, sweat, and perhaps saliva.
rther, the fine experiments of Professor Colman have
own, that the contagious glanders may be excited in the
by the transfusion of the blood of a glandered horse,
d the matter from the nose of the glandered ass can
duce this disease in the horse or the ass*. Hence I ap-
hend it is reasonable to expect, that the further investi-
ion of the properties of the animal fluids will afford
tifying instruction to the researcher in natural science,
d important practical information to the physician.

" On the present occasion, I desire the honour of com-
nicating the knowledge I may have acquired, by inves-
ating the properties of expectorated matter secreted by
bronchial membrane. The appearances of this sub-
nce serve to regulate the judgement of the physician con-
ning several diseases of the lungs; but especially of that
pulmonary tubercles, which yearly destroys 120,000 to
0,000 subjects of the United Kingdom. It is fit that I
nark, that I do not notice in this paper the ingenious
periments of several learned chemists, because by so
ing I should be led into a detail of too great extent for
design.

" The numerous varieties of expectorated matter, ac-
rding to my observation, may be arranged and characte-
ed under the following seven heads:

" I. The jelly-like semi transparent kind of a blueish hue,
creted in the healthy state.

Mr. Colman alleges, that there is not a sufficient quantity of blood, in
ngle glandered ass, to excite the glanders by the transfusion of blood
the horse.

" II. The

" II. The thin mucilage-like transparent matter, so copiously expectorated in bronchial catarrhs.

" III. The thick opaque straw-coloured, or white and very tenacious matter, coughed up in a great variety of bronchial and pulmonary affections; especially in that of tubercles.

" IV. Puriform matter secreted without any division of continuity, or breach of surface of the bronchial membrane, very commonly occurring in pulmonary consumptions.

" V. The matter which consists of opaque viscid masses, together with transparent fluid; or the second sort above stated, with nodules of the third or fourth kind.

" VI. Pus from the vomicæ of tubercles.

" VII. Pus from vomicæ by simple inflammation of the lungs, and without tubercles.

" Other kinds of matter are occasionally coughed up, such as calculi—masses of self-coagulated lymph—serous fluid—blood itself—and perhaps the vascular substance of the lungs; but I do not write on these matters, because they either do not belong to any particular recognized disease; or they are rare occurrences in some well known disease, and are too obvious to require description.

" § I. *Sensible or obvious Properties.*

" 1. *The jelly-like matter*, as already said, is excreted in the best health, as well as sometimes in disease. It is mostly coughed, or hawked up, in a morning soon after a night's repose, during which it seems to accumulate. A few masses, or nodules, then appear of the consistence of jelly, and of the size of a pea to a hazle nut. It is also at any time liable to be excreted, in consequence of various extraneous matters irritating the fauces, to the amount of a few nodules. It is of a gravish colour or inclining to blue, with black specks; and it is rarely whitish in nodules. The consistence is that of jelly, but of much greater tenacity. It has a barely perceivable taste of common salt, or muriate of soda. It commonly floats on water, but by agitation to disengage air bubbles, it sinks. It has no smell. To the naked eye, or assisted by a single magnifier, this matter seldom appears uniform, but consists of a mixture of opaque and transparent masses of irregular figures. With the compound microscope, spherical particles were perceived, though few in number, when duly diluted. The presence of an alkali I could in no instance perceive, by means of the usual tests, namely, turmeric paper, litmus paper slightly reddened by vinegar, and cloth stained with violet juice; nor was an acid denoted by means of litmus paper, except when I had reason to believe it was derived from various acid substances taken with the food, or drink, adhering to the inside of the mouth and fauces.

"2. *The mucilage-like expectorated matter*, according to my observation, occurs much less frequently than the other sorts. It appears suddenly in great abundance in certain bronchial catarrhs. I have seen it to the amount of two or three pints in twenty-four hours. It is also secreted, but less copiously, in paroxysms of spasmodic asthma, and of the hooping-cough; and but rarely in pneumonic or pleuritic inflammations, and in some chronical organic diseases of the heart and lungs.

"This matter is a transparent uniform fluid of the consistence of white of egg; or of a mucilage compounded of about one part of arabic gum, and four or five parts of water. It is colourless—has a fleshy smell—has a brackish taste. After standing eight or ten hours, a deposit takes place of fibrous, leaf-like, or curdy masses, some of which are seen suspended in the clear fluid. In some cases nodules of opaque thick ropy matter, at certain times, accompany this mucilage-like matter. Under the simple magnifier I perceived irregular figured masses partly in motion and partly suspended. With the microscope, globules were seen; but larger considerably than those of the blood, and much less numerous. With the usual tests there were no indications of alkali nor of acid, provided the matter was unmixed with other things. It usually floated, or was suspended in water, when first expectorated; but on standing in the water it fell to the bottom, evidently owing to the disengagement of air-bubbles.

"By standing exposed to the air in warm weather, it sooner grew fœtid than pus of abscesses; without becoming opaque. Neither could I render it opaque or thicker, by exposure to a stream of oxygen gas for an hour; or by exposure of it in a jar of this gas for a month.

"3. *The opaque ropy matter* above mentioned.

"1st. It is secreted most copiously in that very common, and extensively epidemial disease of our climate, the *winter-cough*, occasioned by tubercles, to the amount of half a pint to a pint in twenty-four hours; especially during the winter season for several successive years, and sometimes during the whole of a long life, after the age of forty or fifty years. 2dly. It is often the expectorated matter of the pulmonary consumption of young persons, also occasioned by tubercles, but frequently mistaken for the pus of abscesses or vomicæ. 3dly. It appears, oftentimes, in pneumonic or bronchial inflammation with fever, seemingly being a beneficial discharge; as well as in some instances at the close of a fever without concomitant inflammation of the lungs. 4thly. A severe paroxysm of spasmodic asthma is often terminated in the excretion of this kind of matter. 5thly. A secreted substance of this sort is sometimes expectorated in various chronical organic diseases of the lungs, the heart, aorta, and parts contiguous to the lungs, which occasion difficult transmission of blood through them.

"In all these instances the matter by expectoration is of the consistence of thick cream, or of thin toasted cheese; so tough as to hang in the form of a rope, four or five inches in length, on pouring it from one vessel into another. Its aggregation is such that it is readily detached in large masses from the vitreous surface of vessels. It is not unusual for small black, or reddish spots, and streaks, to appear on the surface of this sort of expectorated substance. A pretty large bulk of it is seldom throughout uniform; but it is frothy, and exhibits opaque masses of various hues with transparent matter interposed. The colour is yellowish, straw-coloured, and white, or gray: it also, though seldom, is greenish and blueish. The taste asserted by patients, is, in their own terms, various, namely, saltish, nasty, faintish, sweetish, luscious, or like that of a sweet oyster,—a sharp or sour taste is the most rare. The only smell which I have perceived is that of flesh, but very frequently there is none. When any offensive or pungent smell was perceived, immediately after expectoration, I have always found that it was owing either to the foulness of the vessel in which it was received; or it was from extraneous matters in the mouth, and from decayed teeth.

"This opaque viscid substance, being duly diluted with distilled water, was examined with microscopes of common as well as of very great powers: by means of any of them crowds of spherical particles were seen passing to and fro, in currents, not unlike those of the blood; except that they were larger. These globules I could not destroy, nor alter in form, by trituration; nor by long boiling in water; nor by exsiccation, and again dissolving in water; nor even by coagulation with mineral and vegetable acids, with alcohol, with sulphuric ether, or with tannin, and alum; nor by mixture with caustic alkalies in a proportion which leaves the liquor turbid; nor for some time after the putrefactive process had appeared. But these globules disappear with such a proportion of sulphuric acid as detaches charcoal; or of nitric acid, and of liquid potash, as produce a clear solution: also by charring by fire. It is perhaps superfluous to remark, that these atomic globules are quite different from the air bubbles usually entangled in this kind of matter, as perceived by the microscope; the latter differ much from the former, in being of far greater magnitude—in being less numerous—in being transparent, and disappearing on agitation, or heating the matter, or even by mere standing.

"For the most part this expectorated substance swims on water; but by agitation or stirring to disengage air bubbles, or by merely standing, it sinks. Some of the lumps suddenly hawked up, immediately fall to the bottom of a vessel of water. No signs of either acid, or alkali, appeared on the trials of this matter with well known reagents, provided it was free from extraneous matter; but it was apt to betray acidity from things taken with the food or drink.

"4. *Puriform matter.* I have seen this matter expectorated in several diseases in the quantity of two or three ounces to half a pint in twenty-four hours, on some rare occasions, without any breach of surface. I believe it would be considered by every one to be *pus*, having the properties commonly admitted to be those of this substance. It will, however, perhaps, only be just to call it *puriform*, for the present, as it appears to me probable, that I shall hereafter be able to show that it possesses properties not belonging to pus of abscesses, although in the obvious, or sensible properties, it is similar to such pus. Accordingly this expectorated matter is not only opaque, white, or yellowish, and thick as the richest cream, but it also has not more tenacity than cream. It is not apt to entangle air, and therefore it immediately mingles with water, rendering it milky; and presently subsides to the bottom, leaving the water clear, or at least whey-coloured. It appears to the naked eye uniform in its texture; and nearly so under the simple lens: but under the microscope thousands of globules similar to those of the blood are seen, which are indestructible as those above related belonging to another kind of expectorated matter.

"The substance, of which I am now speaking, is most frequently excreted in the latter stages of pulmonary phthisis, for many weeks successively. It is taken for granted that this matter is from a breach of surface or ulceration;

but on examination after death, such a state was not found, in many instances, under my observation, although the lungs were as usual full of tubercles and vomicæ. This puriform matter is occasionally expectorated in certain other diseases. The last summer my colleague, Dr. Nevinson, furnished me with several ounces of this sort of substance, but of a greenish hue, and of the consistence of thin cream; which was expectorated by a woman in the third week from the attack of the measles. In a few days she died. On examination of the lungs very carefully, by the excellent house surgeon of St. George's hospital, Mr. Dawes, no ulceration could be discovered in the trachea or in the bronchial tubes; nor were any tubercles or abscesses found in the lungs. The patient, according to my information, had expectorated more than a pint of this fluid every twenty-four hours for a week before death. In another hospital case, a man laboured under a cough with spitting of matter, which all who saw it called pus, and as usual it was considered to arise from an ulceration, or suppurated tubercles; but, on examination after death, the disease was ascertained to be condensation of the lungs, to the consistence of liver, with water in the cavities of the chest, and nothing more.

"5. *Opaque viscid matter of a third, and perhaps fourth sort,* above distinguished, appearing in nodules, and irregular-figured masses, *mixed with transparent slimy matter of the second sort.*

"It is not unusual to see the mixture of these two different kinds, from severe fits of coughing in that constant epidemy of the British islands, the winter chronical pneumonia.

"Different parts of the bronchial membrane being in different states, may account for the secretion of the two different matters. This seems more probable than that these different matters should be secreted from the same part; although it is true that the same part does secrete at one period transparent thin slime, and at another an opaque thick matter. The former is occasioned by great irritation of the membrane, and the latter is the effect of a more gradual secretion with much less irritation.

"For the sake of brevity, I avoid a further description. The practical application of these observations, however important, would not be suitable in this place.

"The sixth and seventh kinds of expectorated substances being secreted after a quite different manner, and being very different in their nature from the preceding five kinds, I shall not give an account of them in this paper."

[The author then describes at large, and with much pre- ion, the effects produced on expectorated matter by the ency of caloric; of alcohol of wine, of water; and of etous acid—also some experiments with different objects m any of these: but we confine the remaining part of present extract to his *Conclusions*, as containing that d of information which will be most acceptable to the nerality of our readers.]

" *Conclusions.*

" 1. From the preceding experiments and observations, d from others which I might have related, it does not ear that the various kinds of expectorated matter, page 12, fer in the ingredients of their composition, but merely the proportion of them to one another.

" 2. It has been shown that expectorated matter consists coagulable, or, as it is also now frequently termed, *albu- nous* animal substance, and of water impregnated with eral saline and earthy bodies;—that the largest proportion the animal substance, which may justly be called an ide, amounts to one-twelfth, and in some very rare cases one-tenth of the expectorated matter, reduced to a brittle te by evaporation; and that the smallest proportion of is oxide, in rare instances, amounts to one forty-fifth of e expectorated matter; but that the usual proportions of vary between one-twentieth and one-sixteenth of this agulable oxide to the evaporable water, that is, between e and six per cent. of the expectorated matter.

" 3. The impregnating substances have been shown to muriate of soda, varying commonly between one and a lf to two and a half per 1000 of the expectorated matter— tash varying between one-half and three-fourths of a rt per 1000—Phosphate of lime about half a part of 1000 Ammonia, united probably to the phosphoric acid; phos- ate, perhaps of magnesia; carbonate of lime; a sulphate; rifiable matter, or perhaps silica; and oxide of iron. But e whole of these last six substances scarcely amounting one part in 1000 of the expectorated matter, it would useless to estimate the proportion of each of them. It is ry probable that the proportions and quantities of these gredients vary much more than now represented in dif- rent states of disease and health [*]. It is very probable so, that some of the ingredients may occasionally be ab-sent, and others of a different kind be present, agreeably to the different states, on different occasions of the other secretions.

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[*] In one case, the opaque expectorated matter in a pulmonary consump- n having been exsiccated to brittleness, became almost liquid after a night's posure to the air.

"4. It is manifest that the different states of consistence of expectorated matter are owing to the proportion of albuminous or coagulable oxide; but I purposely avoid giving an account of the different conditions of health, on which the differences of consistence depend.

"5. The thicker the matter, the smaller I commonly found the quantity of saline impregnation. Hence, in sudden and copious secretions of the bronchial membrane, the matter is asserted to be salt, and to feel hot. In such instances, the proportion of coagulable matter was small; but that of the saline impregnations, particularly of the muriate of soda, and neutralized potash, so great, that the exsiccated expectorated substance tasted very salt, and presently grew moist, or even partially deliquesced; but the opaque ropy or puriform matter afforded a much larger proportion of exsiccated residue, which was but slightly salt, and generally only became soft on exposure to the air. This property of growing moist depends upon the potash.

"6. Each of the human fluids, according to my experiments, contains neutralized potash; at least, this is the fact of the blood, dropsy fluid, pus of abscesses, and pus secreted without breach of surface; the fluid effused by vesicating with cantharides; the urine; and in course in the very abundant secretion from the nose by a catarrh. The alkali being united to oxide of animal matter in these fluids, it is easily demonstrable.

"7. Although I think I have discovered many properties by which expectorated secretion may be distinguished from expectorated pus, I shall not speak of them, on this occasion, further than just to observe that the saline impregnation of pus, particularly that of potash, and muriate of soda, is in very much less proportion than in expectorated secretion; and hence it does not become moist after exsiccation, on exposure to the air.

"8. It has been, I believe, uniformly asserted, that the circulating and secreted fluids are impregnated with soda; that it is especially in the matter secreted by the bronchial membrane. The experiments of others must confirm or disprove mine. It seems, however, much more reasonable, that the human fluids should be found to contain potash than soda, united to some oxide or destructible acid; because the former alkali is daily introduced with the vegetable food, and with the drink of fermented liquor; and it is as little likely to be destroyed, as the muriate of soda also induced in the very same way. But our food and drink do not, commonly at least, contain the soda united to a destructible acid, or an oxide.

"9. It is plain, from the preceding experiments, that expectorated matter belongs to the class of coagulable fluids, and not of gelatinizable, or, as commonly asserted, mucous fluids. It differs from the coagulable fluid, serum of blood, in forming a much thicker fluid with a much larger proportion of water: for serum, and also the water of blisters, is quite liquid, although they afford, on exsiccation, one-twelfth to one-eleventh of their weight of brittle residue, while some kinds of expectorated matter, of the consistence of mucilage, afford only one-fortieth of dry residue, and others of the consistence of thin paste afford only one-fourteenth of residue.

"10. But for the unavoidable extent of this paper, I should trouble the learned Society with various other conclusions and remarks, especially concerning the *globularity* of expectorated matter, which seems to indicate organization. Although Antonius Van Lewenhoeck, above a century ago, discovered the globularity of the blood, and even noticed it in other animal fluids, neither he, nor any other person, as far as I know, investigated the subject in any fluid but the blood, till by Mr. Home's acuteness and industry, at a very early period of life, it was observed in pus. I have in this paper related, that expectorated matter, especially the opaque ropy kind, as well as the puriform, is full of globules, and that, except by such agents as destroy charcoal, they are scarcely destructible. Do these spherical particles consist chiefly of organized carbonaceous matter?"

III. *Memoir on the best Method of decomposing the Chromate of Iron, obtaining Oxide of Chrome, preparing Chromic Acid, and on some Combinations of the latter. By M.* VAUQUELIN *.

WHEN I made my first experiments on chrome, I had such a small quantity at my command that it was impossible to vary them so as to bring all its properties before my view.

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* From *Annales de Chimie*, tome lxx. p. 70.

The importance, however, of the oxide of chrome, on account of its beauty and solidity as a green pigment for earthenware, and in forming imitations of emerald, added to the discovery of large quantities of chromate in the department of the Var, determined me to resume the subject, and to study the properties of chrome at greater length. In the experiments about to be detailed I was assisted by M. Robiquet, an eminent chemist of Paris.

Process for decomposing the Chromate of Iron.

Chromate of iron is generally employed in order to procure a large quantity of the oxide of chrome: this ore has for its matrix a kind of steatite, which, from its colour and some other physical properties, might be confounded to a certain extent with chromate itself, the more easily because these two substances at first sight seem to form one and the same mass: after a little attention, however, we find that the matrix is composed of long and pearl-like laminæ, whereas chromate of iron is very fine grained, shining, and denser than the matrix.

I had formerly employed, in preparing the oxide of chrome, three parts of nitre to one of the ore reduced to fine powder; but I have since found this proportion was by far too large: indeed, as we can isolate the matrix but very imperfectly, it happens that the nitre, by means of the alkali which it sets free, attacks not only the chromate of iron, but also the alumine and silex, which are there in large proportions; and thus the chromate of potash is mixed with an alkaline solution of all these earths, from which two inconveniences result: in the first place, we are under the necessity of employing, in the separation of these earths and the saturation of the excess of alkali, a great quantity of nitric acid, and if it happens that we go beyond the quantity of acid necessary to the exact saturation of the potash, we redissolve a portion of silex, but principally of alumine: in the second place, these earths, on being precipitated, carry down with them chromate of potash, which cannot be freed from them by washing: a third inconvenience occurs, particularly when we operate on a large scale, and when the heat is necessarily long continued, —this excess of alkali attacks the crucible and melts it. Thus the œconomy and success of the operation require that we should employ only one half part of nitre to one of chromate: by these means the mass does not enter into fusion, and the chromate is well attacked. It has often happened

that

at the potash has been entirely saturated with chromic id.

This decomposition being effected, the mass is to be ell lixiviated, the residue is then to be treated, when hot, ith muriatic acid diluted in water, which takes up the on, magnesia, alumine, and silex, divided by the action the potash and the subtraction of the chromic acid.

The solution being terminated, the acid liquor is speedily canted, otherwise it would go into a jelly, and it would en be very difficult to separate the undecomposed chroate: the latter is to be once more treated as at first; but stead of employing the same quantity of nitre, a fourth rt will be sufficient. When the chromate of iron is enely decomposed, we mix the solutions of alkaline chroate, in order to saturate them by the nitric acid, after hich it is proper to crystallize this chromate, as well in der to separate some portions of earth which would have en dissolved by the excess of acid, as to take up a little romate of iron, which is separated in brown dust by the ogress of evaporation. We redissolve in water, filter, d precipitate the liquor by a solution of nitrate of merry at the *minimum*, containing the least possible quantity acid in excess.

Even supposing that the chromate of potash has been rified as we have indicated, *i. e.* that it contains neither rthy substance nor muriate, the chromate of mercury is ecipitated in a more or less intense colour, according to e state of concentration of the solutions, their temperare, and excess of acid. In some circumstances the moleles of this salt, by approaching each other more slowly, sume more aggregation, even crystallize, and thereby quire a deeper red colour. We may also remark that e first portions precipitated are the palest, because, in prortion as we subtract chromic acid, it has the same effect chromate of potash as if we diluted the liquor. To conude, the colour has no influence on the quality of the romate of mercury.

When the mercurial solution is employed at the *minium* of oxidation, and as neutral as possible, there reains nothing in the mother water except nitrate of potash d nitrate of mercury when a superabundance of these s been introduced: but in general these mother waters tain an amethyst colour, and yield with the alkalis a pale een precipitate, which, when heated, leaves oxide of rome.

I have attentively examined this precipitate, in order to become better acquainted with its nature and properties.

When treated cold with caustic alkali it dissolves, and communicates a fine green colour to the liquor: a red powder remained which presented all the characters of oxide of mercury at the *maximum*. The alkaline solution, being filtered and subjected to ebullition, deposited a great quantity of green flakes of oxide of chrome, and preserved a fine golden yellow colour: this was chromate of potash.

Having diluted in water a portion of the precipitate obtained by the alkali of the above mother waters, I remarked that it was composed of two different substances; the first was flaky and light; the second, which always occupied the bottom of the mixture, was formed of small crystals of a violet brown. These last presented the following properties: 1st, When thrown on burning charcoal, they are entirely volatilized and condensed in small purple needles on such cold bodies as are exposed to their fumes. 2d, When heated more slowly in a retort, they furnish mercury, and leave green oxide of chrome as a residue: they are dissolved in weak nitric acid, communicating to it a fine yellow colour: if we pour into this solution nitrate of mercury at the *minimum*, common chromate of mercury is precipitated. 3d, When we treat this substance by a caustic alkali, the latter acquires a yellow colour, and a red powder remains which is oxide of mercury at the *maximum*, while the common chromate of mercury gives black oxide by the same process.

The greenish precipitate obtained by the saturation of the mother waters, by means of an alkali, contains therefore green oxide and chromate of mercury at the *maximum*.

From what is above stated, we may easily explain what takes place when the mother waters, although containing mercury in excess, nevertheless give a precipitate of chromate of mercury by the addition of fresh nitrate. The reason of this is, that although the nitrate of mercury at the *maximum* precipitates the chromate of potash, it requires but a very small quantity of acid in order to be dissolved: this inclined me to think formerly that this precipitation had not taken place, because the mercurial solution, containing always an excess of acid, and being rarely at the perfect *minimum*, the chromate at the *maximum*, on account of an excess of acid, is kept in solution; but when we add a fresh quantity of nitrate of mercury, the portion at the *minimum* takes off the chromic acid from the red oxide which is deposited: if

n the contrary we add alkali, we precipitate this chromate
: the *maximum*.

It now remains to explain how oxide of chrome is found
this precipitate:—the following experiment seems adapted
) give a demonstration of it. When we treat chromate of
ercury at the *minimum,* by the nitric acid, the solution
effected without the extrication of nitrous gas; but if we
duce the quantity of alkali necessary for the saturation of
e acid, we obtain in the first place a brownish-red sedi-
ent formed of chromate of mercury at the *maximum:*
e solution by this subtraction becomes green, and preci-
itates, on the addition of a fresh quantity of alkali, green
xide of chrome, which is easily redissolved in an excess of
ustic alkali. In this case, to all appearance, a portion
f the chromic acid is de-oxygenated in order to hyper-
xidate the mercury, from which result chromate at the
aximum and oxide of chrome.

It is therefore certain that the chromate of mercury at
e *maximum,* found in the mother water, may arise from
vo causes: either it results from the mercurial solution,
it contains oxide at the *maximum,* or it proceeds from
e solution of the nitrate at the *minimum,* on account of
e excess of acid, and then it is found mixed with oxide
f chrome.

The solubility of the oxide of chrome in the alkali fur-
ishes the explanation of what passes, when on lixiviating
old, the product of the decomposition of the chromate of
on by nitre, we obtain a green liquor which becomes yellow
n ebullition; this is because the green oxide is deposited,
hich lays us under the necessity of filtrating these lixi-
iums before heating them, in order to separate this oxide
n the state of purity. This last phænomenon inclines us to
hink, with M. Godon de St. Memin, that the chromate of
on, so called in commerce, contains chrome in the state
f oxide; for it is improbable that the chromic acid is re-
uced at the same time with the nitre. What still confirms
he opinion of M. Godon is, that the acids extract a green
xide only from the chromate of iron.

To return to the subject. We shall observe that it is es-
ential to wash the chromate of mercury with a good deal
f water, in order to free it completely from the nitrate of
otash, which, by the calcination of mercurial chromate,
ould again form chromate of potash, which produces a
ommencement of fusion in the oxide of chrome, conse-
uently gives it a deeper shade, and renders it heavier, which

we may avoid when the chromate of mercury is prepared with proper care.

It is sufficient, in order to obtain the oxide of chrome very pure and of a very fine colour, to heat strongly in a well luted earthen retort the pure chromate of mercury, until no more oxygen is extricated, and to keep up the fire so much the longer in proportion to the depth of the shade we wish to obtain: it seems that there really exist two kinds of oxide of chrome, for by heating it a very long time the green is so weakened, that it passes to a dead-leaf yellow.

Combination of the Chromic Acid with Barytes.

In order to prepare the chromate of barytes, we employ with success chromate of potash well purified and very neutral; we mix with it nitrate of barytes until no more precipitate is produced: we must collect the latter, decant the liquor, and wash several times, until it is entirely freed from all extraneous saline particles.

No harm is done by employing great quantities of water, even warm, in order to wash the salt, for it is not very soluble.

Analysis of the Chromate of Barytes.

Five grammes of this salt, dissolved in the nitric acid and precipitated by sulphuric acid, gave four grammes and four tenths of sulphate of barytes. Admitting with M. Klaproth 68 parts of barytes in 100 parts of sulphate, we shall have, in 100 parts of chromate of barytes, 62·2 of base, and 37·4 of acid.

According to this account, the five grammes of chromate of barytes contain one gramme 87 centiemes of concrete acid, and the latter converted to the state of green oxide by a strong calcination, is reduced to one gramme 56 centiemes, which gives a difference between the quantity of oxygen contained in 100 parts of oxide and 100 parts of acid of 16·6, *i. e.* in order to convert 100 parts of oxide of chrome into acid, we must combine with it 16·6 of oxygen.

In order to effect the analysis of the chromate of barytes, we must dissolve it in weak nitric acid, aided by a little heat, and pour into the solution sulphuric acid in excess; we then wash the sulphate of barytes, and weigh it after having dried and calcined it.

With the same view, we may gently evaporate the liquor to dryness, in order to drive off the nitric acid; redissolve the residue in water, saturate it with ammonia, and ıd calcine strongly in order to evaporate the sulphate of nmonia and completely decompose the chromate of ammonia: lastly, weigh the oxide of chrome. The latter, hen boiled with nitric acid, ought to give no sign of sulıuric acid with the nitrate of barytes.

If the proportions of the chromate of barytes which we ıve obtained are very exact, as there is every reason to :lieve they are, the same analysis may serve for those of .her chromates which are soluble in water.

Process for obtaining the Chromic Acid pure.

Among the various methods which may be employed for 'eparing this acid, the most preferable, in our opinion, ›nsists in decomposing the chromate of barytes by the sulıuric acid : all other processes were attended with more or ss difficulties.

We must therefore dissolve the chromate of barytes in eak nitrous acid; afterwards carefully precipitate it by eans of sulphuric acid, so that all the salt may be decom›sed without the sulphuric acid being in excess. If by ıance we exceed this point, we must separate the superabunınce of the acid by barytes water. We shall then find wheıer we have seized the point at which the precipitate formed ; chromic acid in barytes water is redissolved entirely in tric acid, and at which the sulphuric acid does not disturb ıis chromic acid.

We then filter the liquor and carefully evaporate it, parcularly towards the latter part of the operation, that we ay not decompose the chromic acid : this evaporation ust be repeated several times to dryness, in order to expel l the nitric acid.

When the chromic acid is highly concentrated, irregular *nammelonnées*) masses are formed, in which we see small 'd crystals grouped together, but they are not permanent ı the air, as they attract humidity from it.

The chromic acid thus purified is of a deep red colour, ıs a very acid, but austere and metallic taste; it is solue in alcohol, which speedily decomposes it, for the so.tion becomes green.

'asy Method of determining the Quantity of Chromic Acid combined or mixed with several Saline Substances.

We must put into the solution containing chromic acid slight excess of nitric acid, if the salt be not of itself soble nor acid: we must then pour in a little hydro-sulphut of ammonia, and macerate for some time in a close flask, after which we must boil in order to drive off the superabundant sulphuretted hydrogen, filter in order to separate the sulphur, pour into the filtered liquor some drops of caustic potash, and thus obtain the green oxide of chrome; filter again, wash carefully, dry and calcine carefully. On adding to the quantity of oxide obtained the 16·6 centiemes of its weight, we have the quantity of chromic acid which any given salt contained. We may easily see that in this operation the sulphuretted hydrogen makes the chromic acid pass to the state of green oxide, which the excess of acid takes up again in proportion as it is precipitated: if some portions of it remain, we succeed in separating it from the sulphur by ebullition, which serves at the same time to drive off the sulphuretted hydrogen. The potash afterwards added only serves to decompose the salt of chrome which is formed.

Action of Sulphurous Acid on Chromic Acid.

If into chromic acid we pour a great quantity of sulphurous acid, its yellowish-red colour passes to a dirty brown; and if at this period we pour caustic alkali into the liquor, we obtain a reddish-brown precipitate, which is dissolved in the acids.

A greater quantity of sulphurous acid immediately changes the red colour of the chromic acid to a pale green. Thus it should seem that two kinds of oxide of chrome exist, but which scarcely differ in the quantity of oxygen[*].

Action of the Acids on Oxide of Chrome.

In general the oxide of chrome obtained by the calcination of the chromate of mercury is attacked by the acids with very great difficulty: we at length succeed, however, in dissolving it; but in order to form combinations we made use of the oxide obtained from the decomposition of the chromate of potash by means of the hydro-sulphuret, according to the process above described. We have seen that this oxide, when recently precipitated, is dissolved with the greatest facility even in the weakest acids.

The sulphate of chrome presented nothing remarkable; we merely observed that it is easily decomposed by heat; for, when slightly calcined, it is no longer redissolved in water.

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[*] We obtain an oxide similar to that which is produced by sulphurous acid in the former case, by passing oxygenized muriatic acid into an acid solution of green oxide of chrome, and into which we gradually pour a little potash.

The muriate is so far remarkable, that when evaporated
dryness, it gives a red powder which attracts humidity
om the air; its solution is of a fine green colour. If we
lcine it rather strongly, it gives out a smell of oxygenized
uriatic acid, acquires a great bulk, and is transferred into
nall micaceous brilliant yellow fibres: finally, if we heat
still more, it is totally converted into green oxide.

We boiled several times, and in large quantities, nitric
id over oxide of chrome recently precipitated, and it was
mpletely dissolved; but when we separated the oxide by
eans of caustic potash, the supernatant liquor was co-
urless: the contrary is the case when we evaporate to
yness and slightly calcine: by redissolving in water, the
tter assumes a reddish colour; and after the separation of
e oxide, the liquor remains of a fine golden yellow.

We also dissolved oxide of chrome in phosphoric and in
kalic acids: the first combination was of a splendid eme-
ld green colour, and the other, when viewed in the mass,
resented an amethyst hue. Sulphurous acid also dissolves
xide of chrome with great facility.

Action of the Caustic Alkalis on Oxide of Chrome.

If into a solution of chrome a little diluted we pour
ustic potash in a quantity above what is necessary for the
turation of the acid, the oxide is redissolved in this al-
ali. We also obtain an alkaline solution of oxide of
hrome on taking it recently precipitated, diluting it with
little water, and dissolving in this water some pieces of
ustic potash; on afterwards diluting the combination
ith water and filtering, we obtain a liquor of a fine green,
hich, on ebullition, deposits the oxide it contains, and
e liquor remains colourless.

Chromate of Potash.

There are two kinds of chromate of potash; the one neu-
al is of a citron yellow, and crystallizes in small prisms.
his salt, on the addition of heat, assumes a fine red, but
turns to its natural colour when cooled. The second has
n excess of acid; its colour is orange-red, and it crystal-
zes in beautiful prisms of the same colour.

Chromate of Ammonia.

When we saturate ammonia by the chromic acid, and
bandon the liquor to a spontaneous evaporation, an ar-
orescent salt is formed out of the liquor, composed of tufts
f fine yellow: sometimes it is presented in the form of
pearl-

pearl-like laminæ. To conclude, this salt is easily decomposed by heat; even when it is dissolved, brown flakes are separated, which are oxide of chrome, and which become green by calcination.

Chromate of Lime.

Chromic acid forms a very soluble salt with lime; its solution furnishes by evaporation silky flakes of a yellowish brown, which are easily dissolved in water: this salt is decomposed by the fixed alkalis.

Chromate of Magnesia.

Magnesia easily combines with chromic acid: the salt which results from it is very soluble in water; the solution crystallizes in prisms with six faces perfectly transparent, and of a fine topaz yellow: when they are in considerable quantity the colour is orange yellow.

Magnesia is separated from the chromic acid by the fixed caustic alkalis and the alkaline earths.

Metallic Chromates.

If into a solution of sulphate of iron at the *minimum* we pour chromate of potash, we obtain a fawn-coloured precipitate, which, when treated with caustic alkali, gives no trace of chromic acid: this precipitate is dissolved with great facility in muriatic acid, from which the alkali separates it completely without the least trace of alkaline chromate remaining. The nitric acid dissolves a part of the precipitate, and assumes a fine green colour: this precipitate therefore is not a chromate of iron, but a mixture or combination of oxide of iron and oxide of chrome, which seems to resemble strongly what is presented to us by nature.

It is evident from the result of this experiment, that the chromic acid has been decomposed by the oxide of iron, which, in passing to the *maximum*, has reduced the other to the *minimum*, or to the state of green oxide. If we wished therefore to form chromate of iron, it would be necessary to employ this last metal saturated with oxygen, in order that it may not be able to act upon that of the chrome.

Chromate of Lead.

This combination assumes different shades, according to the manner in which it has been prepared.

If the chromate of potash be neutral, we obtain an orange-yellow colour; if it has an excess of acid, the colour

our is deep citron yellow: if the alkali on the contrary be redominant, the shade is a reddish yellow, and sometimes fine deep red: the shades also vary in proportion as we perate in the hot or cold manner.

Chromate of lead made with a solution slightly acid is at which is most in request by painters, and it is in fact he most solid. We may heighten its colour, either by a ttle alkali, or by precipitating it hot with acetate of lead. n the latter case it should seem that a part of the acetic cid is separated, and that the oxide of lead which it abandons is united to the common chromate and heightens its olour.

We should conceive that the chromates which contain n excess of oxide of lead must be more alterable by sulphuous vapours than these in which this oxide is saturated by e chromic acid.

Chromate of Copper.

The simplest way of forming chromate of copper is to ix a solution of neutral chromate of potash with a soluon of sulphate of copper: a brownish-yellow precipitate formed, which, when well washed and dried, assumes a stre brown colour.

Chromate of Silver.

Chromate of silver is prepared by decomposing nitrate of lver by neutral chromate of potash: a reddish-brown ecipitate is produced when the operation is performed with eat, and of a purple-red when done in the cold: lastly, is of a carmine-red colour if the solution of chromate of tash contains a slight excess of acid: in the latter case e precipitate is not so quickly formed, and is less abunnt, but it is crystallized in small semitransparent grains. This salt becomes brown on exposure to light: it is soble in the nitric acid, and from which the muriatic acid parates the oxide of silver.

Uses to which the Preparations of Chrome are applied.

This oxide is now generally employed in the porcelain manufactories throughout France. It supports better than [illegible] without undergoing any alteration. [illegible] been employed to prepare hard porcelain: it [illegible] an extremely beautiful green, which has not yet been obtained with any of the other metals.

A very fine enamel resembling in colour the emerald of [illegible] is made with the oxide of chrome. Another enamel is also made with it, which, when applied upon copper or silver, furnishes a colour precisely similar to that of fine gold, and imitates this bright metal extremely well when applied in thin leaves to other metals: a colour which, I think, cannot be obtained in the same degree of perfection with any other metal.

I shall not dwell upon the different varieties of chromates of lead used in painting: they are already well known to artists, and are in great request on account of their beautiful colours, the facility with which they may be applied, and their great inalterability.

It is very probable that several other metallic chromates would also furnish beautiful colours if they were properly examined by painters.

IV. *Observations on loaded and unloaded Barges, Boats, Beams, or floating Bodies descending with Streams or Currents, and why the heavier End will go foremost.*

By GEORGE ORR, *Esq.*

IN addition to what I have already said on this subject, I now submit what follows to the consideration of scientific men: and as my object is an endeavour to attain the truth on so interesting a subject, I am ever ready to admit my errors where I am wrong, but hope that reason and good temper, free from peevishness and personality, will ever regulate the discussion of philosophical subjects.

When bodies of the description alluded to, or in fact any bodies that are specifically lighter than water, float in it, there are two powers always opposed to each other, that is, the specific gravities of the fluid and of the floating body; and in proportion as these differ more or less, in the same proportion will the two bodies oppose each other: for the less specifically heavy any floating body is, the less power will it possess to contend against the fluid in its endeavours to sink or descend; and of course it will follow that it will float nearer the surface. When water is perfectly at rest, it has found its level, and its surface presents a horizontal plane; or in other words, all its particles press or gravitate towards the centre of the earth in perpendicular and right lines; but on any change from this state of rest taking place, the particles of water are, by the force of gravitation, put in motion, and will endeavour to find their level again, or continue to move on an inclined plane; and all bodies suspended in the fluid, or floating on the surface more or less deep, being subject to

the same laws of gravitation, take their direction with
e moving fluid, and thus pass down the same inclined
lane, with a motion more or less accelerated as such bo-
ies are heavier or lighter; that is, as they possess a greater
r less power to overcome the resistance that may be op-
osed to them.

I think it is manifest from what is here said, that the
otion which takes place with regard to both fluid and
lid, is owing to the attraction of gravitation; and as the
elocities of bodies arising from this power are greatest in
escending the perpendicular to the plane of the horizon,
it will follow that these velocities will be diminished un-
l the line, along which they may descend, being carried
om the perpendicular round the whole quadrant or right
ngle, arrives at the level or parallel to the horizon, where,
the power of descending be totally opposed, or the centre
f gravity altogether supported, no motion will take place.
ence it will follow that the velocity of a stream, river, or
rrent, and of course of bodies that float in them, will be
eater or less, as the inclination of the plane on which
ey descend departs more or less from the line bounding
e horizontal plane.

Loaded barges, beams of heavy wood, &c., without
wing, and that float with the tide, will make a quicker
ogress than the tide; the same will take place in a river
stream where there is no tide. Captain Burney having
ked the bargemen on the Thames the reason of this, their
ply was, "That loaded barges had more hold of the tide
om their floating deeper than unloaded ones." This was
t a bad way of accounting for it; but some reason must
assigned by those who continually observed this, and
ho were ignorant of the real cause of it.

The reason of this quicker progress seems to me to be
is: any solid floating in a fluid, and descending with it,
ts altogether in one mass; and its particles thus acting
gether conspire to overcome the resistance they meet, and
divide the fluid, which, easily yielding to any pressure,
ill make way for the body in its descent pressing forward.
esides, the particles of the fluid do not act in conjunction,
d being easily separated they will roll about and impede
ch other by their friction, not only against each other,
t also against the sides of the river, and the bed on which
ey descend. To this may be added, that less friction takes
ace between the fluid and solid, as they attract each other
ss, than between the innumerable particles of the fluid;
nsequently the solid will glide on, or slip through the
liquid

liquid body with a greater degree of velocity than the fluid, under the circumstances already enumerated, can attain.

Captain B. says, this greater progressive motion in the floating body is not owing to a more rapid under-current. I think not, but to the causes already assigned. Captain B. says, the surface of the ocean is an inclined plane. I have stated in some letters several months ago in the Philosophical Magazine, that the surface of the ocean, owing to the attraction of gravitation and its laws, and to the impression of the winds, &c., consists of an infinity of inclined planes, or ascents and descents.

No collision could take place between a heavier and lighter barge or body passing London bridge with the ebb of the tide, let them be ever so near, provided the heavier body were foremost, because on its descent it will, from its greater weight, acquire a *momentum* which will carry it on more rapidly than the lighter body; but if the lighter body were foremost and very near, it might be overtaken, and a collision take place.

When the wind blows strong into any bay, or against an embayed coast, there must be an under-current, because the wind prevents the return of the accumulated water along the surface. All pressure on bodies floating with streams must, whether the pressure be perpendicular or oblique, increase their progress:—if the pressure be perpendicular, it adds to the weight, and consequently to their power of overcoming resistance on the part of the fluid: if the pressure be oblique, and in direction of the motion, it will, besides increasing the weight, give impulse.

The reason why ships at sea that are deeply loaded make less progress on a voyage than those that are lighter, and which seems to be in contradiction to what was last stated, seems to me to arise from the following cause: that is, that what they gain by their gravity over a lighter vessel in descending from the top of the wave or inclined plane, they lose in ascending the next; for it is manifest that the surface of the ocean consists of innumerable inclined planes, or ascents and descents; but in a river or running stream the whole progress is on a descent.

Two pieces of wood of the same kind and of the same weight, but of different shapes, the one, for instance, a cylinder with all its transverse diameters equal, the other of a conical form, with its transverse diameters or lines all unequal, would, in my opinion, differ considerably in their progress in the same fluid, the cone making greater way than the cylinder; and to this I presume it may be principally attributed why one ship sails better than another, and which, on some future occasion, I may attempt to demonſtrate.

Timber used at sea to find the direction of any current ought to be in its shape conical, and a good deal so, but the base of the cone to be rounded, or the sharp angles taken off, in order that it may pass more fairly through the water; for wood of this shape will take the direction better, and be more easily observed: in such cases there should always be two conical timbers, and if they both take the same direction, the tendency of the current is certainly proved. No experiments can be made on this subject with South Sea clubs, such as captain B. used, nor with anything that will sink in water, that is, with any body specifically heavier than water.

Supposing a barge loaded at one end and empty at the other, and without a helm, if it floated in a fair and regular stream without currents, it would certainly proceed with the heavy end foremost, for the same reasons that a conical piece of wood, or even a cylindrical one loaded at one end, would go with their heavy end foremost in a fluid, though originally placed in a contrary direction: it would be the same with the cone and cylinder, if placed across a dry inclined plane; that end containing the greatest quantity of matter, from its power of overcoming resistance, would always have a tendency to be foremost. The inclined plane of rivers and streams must, from the number of inequalities at bottom, be a very irregular one—the current will partake of those irregularities, and the motion of bodies that float in them must be affected by them in a certain degree.

Lastly, Does a body floating down a stream or current, and which has a quicker progress than the stream or current, receive any addition to its motion from the motion of the fluid? I think it does not; that it is only indebted to the fluid for its suspension, but that it is to its own gravity, and acting in one mass, that it is indebted for its greater progress. On the contrary, I think it loses in its velocity or progressive motion; for though the floating body be specifically lighter than the water, and of course one would imagine that it would move slower, still, owing to the causes already enumerated, its progress is quicker: but there is a drawback on this progress; because, if the solid overtake the water, part of its force must be wasted against that body which moves slower, but in the same direction. This case is analogous to that of two balls moving in the same direction with different velocities; that with the greater velocity, on overtaking the other, communicates part of its force, and of course loses so much in its velocity.

V. *On Nævi Materni.*

To Mr. Tilloch.

Sir, The last Number of your Philosophical Magazine contains a discussion, by one of your ingenious correspondents, in support of the old prejudice that certain congenital marks or excrescences, commonly called *nævi materni*, originate from the influence of the mind of the parent. As the paper, no doubt, was written with a serious object, I comply with the writer's request in forwarding to you for publication some observations on the subject.

It is agreed that *nævi materni* resemble known objects in nothing more than *form;* although, as Enceps (the writer's signature) observes, " more than one volume has been filled with reputed instances of the effects of the mother's imagination upon her offspring." The *forms*, however, of *nævi materni* in general are so far from having a resemblance to any known object, that the experience of practitioners, who are in the daily habits of operating on some, and seeing many others, proves as much as extensive experience can prove, that the relations of *nævi materni* resembling a bunch of grapes, or a bit of bacon, are to be classed with the idle tales of the nursery. Even Enceps himself " was for a long time prepossessed with the same notion," viz. that such fanciful forms of *nævi materni* were " idle tales," till a case " was related to him by an intelligent friend who had seen a child born with only one leg, as well as its mother, who declared her firm belief that the cause of this imperfection in her child was a violent fright which she experienced from seeing a beggar suddenly uncover the wounded stump of his thigh." This, however, is not an example of *nævus maternus*, but of *monstrosity*. Now I will seriously ask Enceps, at what period of pregnancy this unfortunate mother received such a *violent fright*. In the latter months of pregnancy, *after* the limbs of the fœtus had been formed? The separation of a limb, and its still more extraordinary annihilation, must then be explained as the effects of imagination,—*or* in the early months of pregnancy, *before* the ovum had become organized!

zed! The imagination must then be supposed to act on a art of the fœtus before such a part had any existence. The ommon opinion is, that monstrosity depends on *original onformation* of the ovum; nor is it by any means intelli-ible that a metamorphosis or annihilation of any part of he fœtus can take place from an impression on the imagi-ation of the parent.

I have said that Enceps's object in writing was not evity; but when I came to the *tale* of the cat and the ittens, my suspicions were somewhat roused. "A preg-ant *she* cat" had its tail trodden upon, and lo! when she ittered she had an *even* number of kittens; otherwise the urprising things that did happen, could not have happen-d—that "*half* of her kittens had their tails bent in the niddle!" and that *half* had not. The circumstance which oused my suspicion was the gravity with which Enceps ays "this fact (or rather this *tale*) seems to be very im-ortant, and *to prove nearly to a demonstration*, that the magination of pregnant females has the power of acting on he bodily conformation of their young!!"

As a medical man, I had reason to hope that the old rejudice of the perfection of the fœtus being in anywise ontingent on the imagination of the parent was at an end; nd having long been a reader of your respectable Magazine, I could not observe Enceps's reasoning in favour of uch a prejudice, without presuming that it was subject to nimadversion.

I remain, sir, your humble servant,

CHIRURGICUS.

ecember, 1809.

VI. *Memoir on the Mineralogical Geography of the Environs of Paris. By Messrs.* CUVIER *and* BROGNIART[*].

THE country in which Paris is situated is perhaps one of the most remarkable hitherto observed, from the succession of the various soils which compose it, and from the extraordinary remains of ancient organizations which it contains: myriads of sea shells, regularly alternated with fresh-water shells, form the principal mass: bones of terrestrial animals, entirely unknown even with respect to their genera, fill certain parts: other bones of species remarkable from their size, and the counterparts of which are only

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[*] *Annales du Muséum d'Histoire Naturelle*, tome xi. p. 293.

found in very distant countries, are scattered in the strata nearest the surface: a strongly-marked character of a great eruption proceeding from the south-east is imprinted on the forms of the eminences and the directions of the valleys: in short, there is no district better adapted for making us acquainted with the final revolutions which put a termination to the formation of our continent.

This country has nevertheless been very little studied under this point of view; and although so long inhabited by many enlightened men, whatever has been written on the subject has been confined to some detached fragments, and almost all of them are either exclusively mineralogical, without any regard to organized fossils, or purely geological, without reference to the position of these fossils.

A memoir of Lamanon, on the gypsums and their fossil bones, may perhaps form the only exception to this classification: we are bound, however, to acknowledge that the excellent description of Montmartre by M. Desmarets, the information given by the same author as to the basin of the Seine, in the *Encyclopédie Méthodique*, the mineralogical essay on the department of Paris by M. Gillet-Laumont, the extensive researches on the fossil shells of the environs of Paris by M. Lamarck, and the geological description of the same district by M. Coupé, have been advantageously consulted, and have several times directed our steps.

We presume to think that the task of which we are about to present the class with a sketch, will not be without interest, notwithstanding the works above alluded to.

Four years ago we commenced our labours; and although we have persevered in them by making numerous excursions, collecting specimens and information from every quarter, we are far from thinking we have done enough, and we earnestly desire that our readers may not confound the abridgement which we are about to give, with the full details which we propose to publish. Some circumstances compel us to present this abridgement at this moment, and to assign a date * to such tedious and laborious researches, before the happy period at which we shall think them brought to a conclusion.

From the nature of their object, our sketches were limited according to the nature of the soil, and not according to arbitrary divisions.

We thought it right therefore, in the first place, to de-

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* This Memoir was printed in Jan. 1809.—Edit.

rmine the physical boundaries of the district which it was
r object to study.

The basin of the Seine is separated for a long space
om that of the Loire by an extensive high plain, the
eater part of which vulgarly bears the name of Beauce,
d the middle and driest part of which extends from the
rth-west to the south-east, over an extent of more than
rty leagues, from Courville to Montargis.

This plain is bounded towards the north-west by a higher,
d in particular a more broken district, from which the
vers Eure, Aure, Ilon, Rille, Orne, Mayenne, Sarte,
uine, and Loire, arise. The highest part of this district
between Sees and Mortagnes, and which formerly com-
sed the province of Perche, and a part of Basse-Nor-
andie, and which now belongs to the department of the
rne.

The line of separation between Beauce and Perche passes
ose by the towns of Bonnevalle, Alluye, Iliers, Cour-
lle, Pontgouin, and Verneuil.

On all other sides the plain of Beauce overlooks every
rrounding district.

Its slope towards the Loire is not interesting to our sub-
ct.

The slope towards the Seine is divided into two incli-
tions, one of which on the west looks towards the Eure,
d the other on the east looks towards the Seine.

The first proceeds from Dreux towards Mantes.

The other begins from the neighbourhood of Mantes,
sses by Marly, Meudon, Palaiseau, Marcoussy, the
erté-Alais, Fontainebleau, Nemours, &c.

But it must not be concluded that these two inclined planes
e straight or uniform; on the contrary, they are in all direc-
ons unequal and rugged, to such a degree that, if this vast
ain were surrounded with water, its edges would furnish
lfs, capes, and straits, and would be every where sur-
unded by small islands.

In the same manner in our environs, the long mountain
which are situated the woods of St. Cloud, Ville-
Avray, Marly, and Aluets, and which extends from St.
loud to the confluence of the river Maulde in the Seine,
ould form an island separated from the rest by the strait
which Versailles is situated, the little valley of Sevres,
d the great valley containing the park of Versailles.

The other mountain in the form of a fig leaf, on which
e situated Bellevue, Meudon, the woods of Verriere, and
ose of Chaville, would form a second island separated
from

from the continent by the valley of Bievre and that of the hills of Jouy.

But afterwards, from St. Cyr to Orleans, there is no longer any complete interruption, although the rivers Bievre, Ivette, Orge, Etampes, Essonne, and Loing, cut deeply into the continent on the east coast, while the rivers of Vesgre, Voise, and Eure, do the same towards the west.

The most rugged and uneven part of the surface, and that which would furnish most islands, would be what is vulgarly called the *Gâtinois François*, and particularly that part in which the forest of Fontainebleau is situated.

The slopes of this immense platform are generally very abrupt; and all the ravines which we find in them, as well as those of the valleys, and the wells dug in the high parts, show that its physical nature is the same every where, being formed of one prodigious mass of fine sand which covers the whole surface, passing equally over all the other soils or inferior platforms which this great plain overlooks.

The edge of this platform towards the Seine, from the Maulde to Nemours, will therefore form the natural limit of the basin which we are about to examine.

From below its two extremities, *i. e.* towards the Maulde and a little beyond Nemours, immediately issue two portions of a platform of chalk, which extends in every direction and to a great distance, in order to form the whole of Haute Normandie, Picardy, and Champagne.

The interior edges of this great girdle, which pass from the east by Montereau, Sezanne, and Epernay; from the west by Montfort, Mantes, Gisors, and Chaumont, in order to approach Compiegne, and which form at the north-east a considerable re-entering angle which embraces the whole of the Laonnois, complete, together with the sandy coast now described, the natural limit of our basin.

But there is this great difference, that the sandy platform which comes from Beauce is higher than the others, and is consequently the most modern, and finishes completely the stretch of coast which we have marked; while, on the contrary, the platform of chalk is naturally more ancient and lower than the rest, only ceasing to appear outside of the girdle above mentioned; but so far from being at an end, it visibly sinks under all the other strata: we find it in short wherever we dig sufficiently deep under the latter, and it even rises up in some places, piercing as it were through the other strata.

We may therefore conclude that the materials which compose the basin of Paris, in the directions to which our quiries were limited, have been deposited in a vast hollow gulf, the bottom of which was of chalk.

This gulf perhaps formed a complete circle, or a kind f great lake; but we cannot ascertain this, in consequence f its edges on the south-west having been covered, as well the materials of which they were composed, by the great ndy platform first mentioned.

We may add that this great sandy platform is not the nly one which has covered the chalk; there are several in hampagne and Picardy, which, although smaller, are of similar nature, and may have been formed at the same me. Like it, they are placed immediately over the chalk, the places where the latter was so high as not to admit of s being covered with the materials of the basin of Paris.

We shall in the first place describe the chalk, the most cient of the substances which we have in our environs, d conclude with the sandy platform, the most recent of r geological productions.

In the intermedium between these two extremes we shall eak of less voluminous but more varied substances, which d covered the great cavity of the chalk before the platform sand was deposited on some of them.

These substances may be divided into two soils (*étages*).

The first (which covers the chalk wherever it was not suf- ciently high, and which has filled the whole of the bottom the gulf,) is itself subdivided into two parts of equal vel, and placed not upon one another, but end to end: z.

The platform of siliceous lime containing no shells.

The platform of lime with coarse shells.

We are sufficiently well acquainted with the limits of is soil on the chalky side, because the chalk does not ver it; but these limits are marked in several places by e second soil, and by the great sandy platform which rms the third, and which covers a great part of the two hers.

The second soil will be named *gypso-marley*.

It is not generally spread, but merely scattered from ace to space, and as it were by spots; these spots also e very different from each other in thickness, and in the tails of their composition.

These two intermediate soils as well as the two extreme ils are covered, and all the vacuities which they have ft are partly filled, by a fifth sort of soil, mixed also with arle and silex, and which we call *fresh-water soil*, because abounds in fresh-water shells only.

We have the honour to present the class with the first of a series of mineralogical charts, in which each kind of soil is coloured differently.

The sand is fawn-coloured; gypsum blue; shelly lime yellow; siliceous lime violet; chalk red colour; fresh-water soil green streaked with white. We have here marked in plain green, the worn or alluvial sands which have not been tranquilly deposited, but brought from other quarters by currents; and in dark brown the peaty soils formed along the rivulets and round the pools of water.

This chart, one of the principal results of our travels, is complete in the coloured part, and we have only left uncoloured that with which we are not yet sufficiently acquainted.

Such are the great masses of which our district is composed, and which form the different strata. But on subdividing each stratum we may attain still greater precision, and obtain still more rigorous mineralogical determinations, which will give so many as ten distinct kinds of strata, of which we shall now present a rapid enumeration.

Article I. *Formation of Chalk.*

Chalk forms in the environs of Paris, as in almost all those places where it has been observed, a mass in which the strata are frequently so indistinct that we are almost inclined to doubt whether it was formed by beds, if we did not see these beds interrupted by silex, which, by their perfectly horizontal position, their parallelism, their continuities, and their frequency, indicate successive and almost periodical depositions.

Their respective distance varies according to the place: at Meudon they are about two metres (78¾ Eng. inches) from each other, and the space comprehended between any two beds of silex does not contain any detached pieces of this stone. At Bougival the beds are divided, and the silex is much less abundant.

The chalk which contains flints is not pure carbonated lime: it contains, according to M. Bouillon Lagrange, about 0·11 of magnesia and 0·19 of silex, the greatest part of which is in the state of sand, which we may separate by washing. The fossils found in it are not numerous, in comparison with those which we observe in the strata of coarse limestone (which almost immediately cover the chalk), but they are entirely different from these fossils, not only in the species, but even in the genera.

On adding those which we observed ourselves, to those
ich have been collected by M. Defrance, we extend to
number of fifty the various species of fossils with
ich we are acquainted in the chalk, of the soils which
the objects of our study.

All the species of these fossils have not been as yet de-
mined, and we shall give in our subsequent detailed Me-
irs their precise enumeration and determination: we
ll content ourselves with saying on the present occasion,
t we have found

Two *lituolites.*

Three vermiculites.

Some belemnites, which, according to M. Defrance, are
ferent from those which accompany the ammonites of
npact lime.

Some fragments of shells, which, from their tubular form
d fibrous structure, cannot be referred to any other than
genus *pinna:* but if we were to infer from the thickness
these fragments the size of the individuals to which they
ıst have belonged, we must conclude that these testacea
ıst have been monstrous. We measured pieces 12 milli-
tres (·47 inches) thick, while the thickness of the largest
ıds of pinna known is only two millimetres (·08 inches).

One muscle shell.

Two oyster ditto.

A species of the *pinna* genus.

A cranium.

Three anomites (*térébratules*).

One *spirorbis.*

Some *ananchites,* the crustaceous envelope of which has
ained calcareous and assumed the sparry texture, while
middle part only is changed into silex.

Some *porpytes.*

ive or six different *polypiers:* one of them seems to
ong to the genus *caryophyllæa,* and another to the genus
lepora. This last is generally brown, and in the state
oxidated iron, resulting from the decomposition of
ites.

Lastly, some teeth of squali.

We shall observe with M. Defrance, that there has not
n found in chalk any univalve shell with a simple and
ular spire. This fact is the more remarkable, as we
et with these shells in great abundance, some metres
·38 inches) above the chalk, in calcareous strata also,
of a different structure.

Among the quarries and hills of chalk which we have
visited,

visited, we shall mention Meudon. The chalk is here covered by plastic clay and coarse limestone. The upper part of this mass is as it were broken, and presents a kind of rubble (*brêche*), the fragments of which are chalk, and the intervals clay.

The highest part of the mass of chalk appeared to us to be above the glasswork of Sevres. It is 15 metres (49¼ feet) above the Seine. This disposition raises up all the strata of soil which surmount it, and seems at the same time to diminish their thickness. The mass of stone is sensibly inclined towards the banks of the river.

At Bougival, near Marly, the chalk is almost entirely exposed in some places, being only covered by calcareous stones of a very fine grain, but in fragments more or less large, and scattered in a marley sand, which is almost pure at the top.

Amidst these fragments we find geodites, of a yellowish-white limestone, compact and fine grained, with sparry laminæ, and small cavities strewed with very small crystals of carbonated lime. The paste of these geodites contains a multitude of small spiral univalve shells; which seems to prove that this limestone does not belong to the chalk formation.

Among these geodites we found one, which presented a large cavity fringed with long and acute limpid crystals, upwards of two centimetres (·79 inches) long.

Mechanical division alone informed us, that these crystals belonged to the species of sulphated strontian, and a more attentive examination of their form instructed us that they constituted a new variety. M. Hauy, to whom we communicated the circumstance, called it *apotamous sulphated strontian.*

These crystals present rhomboidal prisms with four panes, the angles of which are the same with those of the prism of the unitary, or blunted (*emoussée*) varieties, &c., *i. e.* 77° 2′ and 102° 58′. They are terminated by pyramids with four faces and very acute. The angle of incidence of the faces of this pyramid on the adjacent panes is 161° 16′. The faces are produced by a decrement by two ranges to the left and right of the angle E of the subtractive molecule. This is a law which had not hitherto been recognized in the varieties of sulphated strontian examined, to this day. Its sign will be $\overset{1}{E}\,E^2\,{}^2E$.

The crystals of strontian hitherto observed in the environs of Paris are extremely small, and fringe the sides of some

me of the geodites of strontian found in the green marles
gypsous formation ; but we have not as yet seen any of
em so large and so well defined.

ARTICLE II. *Formation of Plastic Argil.*

Almost the whole surface of the mass of chalk is covered
th a stratum of plastic clay which has some very remark-
le common characters, although it presents sensible dif-
'ences in various points.

This clay is unctuous, to the touch tenacious, and con-
ns silex, but very little lime; so that it does not effer-
sce with the acids. It is even absolutely infusible in a
rcelain furnace when it does not contain too great a
portion of iron.

It varies considerably in colour :—at Moret in the forest
Dreux, it is very white: at Montereau, Houdan, and
ndé, it is gray : pure slate gray, slate gray mixed with
l, and almost pure red, throughout the whole south of
ris, from Gentilly to Meudon.

This plastic clay is, according to its various qualities,
ployed in making porcelain, stone-ware, crucibles, or
nmon red earthenware. It is never effervescent nor fu-
le. The red colour, the pyritous grains, the portions of
x, the small fragments of chalk, and the crystals of
enite which it sometimes contains, are the only defects
nd in it.

This stratum varies much in thickness: in some parts
s 16 metres (52½ feet) and upwards; in others it forms
ly a thin covering of one or two decimetres (4 or 8 inches).

It seems almost certain, that no marine nor terrestrial
sil is found in this clay, at least we have not seen any,
ner in the different strata we examined in their respective
sitions, or on the large heaps which we repeatedly ex-
ined, in the numerous manufactories in which it is
d ; besides which, the workmen who dig up this article
the south of Paris assured us, that they never found in it
shells, bones, wood, or vegetables.

Dolomieu, who discovered this same bed of clay be-
en the chalk and the coarse limestone in the elbow
med by the Seine in front of Rolleboise, says indeed
it fragments of bituminous wood have been found in it,
l that they have been even taken for coal ; but he re-
rks, that these small woody portions have been found in
ces rolled down from the bank, which may have en-
oped them at a period posterior to the primitive deposit
this clay.

The places mentioned above prove, that this stratum of clay is very extensive, and preserves throughout its whole extent its principal characters of formation and position.

If we compare the descriptions we have given of the beds of chalk and beds of plastic clay, we shall remark; 1st, That not only we do not find in the clay any of the fossils met with in the chalk, but we do not even find any fossil in it. 2d, That there is no insensible passage between the chalk and the clay, since the parts of the bed of clay most adjacent to the chalk do not contain lime more than the other parts.

It appears to us that we may conclude from these observations, in the first place, that the liquid which has deposited the bed of plastic clay was very different from that which deposited the chalk, since it does not contain carbonated lime in any sensible quantity, and since none of the animals live there which inhabited the waters that deposited the chalk. Secondly, that there must necessarily have been a distinctly-marked separation, and perhaps even a long period of time between the deposit of the chalk and that of the clay, since there is no transition between these two kinds of soil. The kind of broken fragments of chalk and clay which we remarked at Meudon * even seems to prove that the chalk was solid when the clay was deposited. This earth is insinuated between the fragments of chalk produced on the surface of the chalky soil by the motion of the waters, or by some other cause.

The two kinds of soil which we have described have been produced, therefore, under completely different and even well-defined circumstances. They are the results of the most distinct and best characterized formations which can be found in geology, since they differ in their chemical nature, in the kind of stratification, and above all in the kind of fossils which we meet with in them.

ARTICLE III. *Formation of Sand and of coarse Limestone.*

The coarse limestone does not always immediately cover the clay, being frequently separated by a stratum of sand of various thickness. We cannot say whether this sand belongs to the formation of the calcareous earth, or to that of the clay. We have not found shells in it, in the few places where we have observed it, which would give it an

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* See page 42.

argillaceous

gillaceous formation; but the lowest calcareous stratum
ing generally sandy and always filled with shells, we do
t as yet know if this sand be different from the first, or
it be the same formation. What would lead us to sup-
se that it was different is, that the sand next the clays
iich we have seen, is generally very pure, although of a
l or blueish gray colour; it is refractory, and frequently
very coarse grains.

The calcareous formation reckoned from this sand is com-
sed of alternate strata of coarse limestone, more or less
rd, of argillaceous marle, and even leafy clay in very
n strata, and of calcareous marle; but it must not be
posed that these various strata are placed there by
ance, and without following some general law: they
iformly follow the same order of superposition in the
siderable extent of soil which we have examined. In
ne places several of them are entirely wanting, or are
y thin; but that which was the lowest in one district
ver becomes the uppermost in any other.

This constancy in the order of superposition of the thin-
st strata, and over an extent of 12 myriametres (74½ miles)
least, is in our opinion one of the most remarkable facts
ich we have established. From this there ought to result
st interesting consequences to the arts and to geology.

The means which we have employed for ascertaining, in
midst of so great a number of calcareous beds, a stra-
n already observed in a very distant district, is taken
m the nature of the fossils contained in each stratum,
se fossils being generally the same in the corresponding
ata, and present differences of species sufficiently re-
rkable between one system of strata and the other. This
mark which has not as yet deceived us.

It must not be thought, however, that the difference
ween one stratum and another is equally well marked
h that between the chalk and the limestone. If this was
case, we should have an equal number of distinct forma-
ns; but the characteristic fossils of one stratum become
s numerous in the next superior stratum, and disappear
npletely in the rest, or are replaced gradually by new fos-
which had not as yet appeared.

We now proceed to point out (following the same course)
principal systems of strata which may be observed in
coarse limestone. In the detailed Memoir about to be
blished will be found the complete description, stratum
stratum, of the numerous quarries which we have ob-
ved in order to procure materials for our publication.

The lowermost strata of calcareous formation are the best characterized: they are very sandy and frequently even, rather sandy than calcareous. When they are solid, they are decomposed and fall into dust on the first exposure: this kind of stone therefore is not fit to be used.

The shelly limestone which composes it, and even the sand which sometimes supplies its place, almost always contain green earth in powder or in small grains. This earth, from our experiments, appears to be analogous to *terra veronica.* It owes its colour to iron; it is found in the lower stràta only: we do not find it in the chalk, in the clay, nor in the middle or upper calcareous strata, and we may regard its presence as a certain indication of the proximity of plastic clay, and consequently of chalk. But what characterizes still more particularly this system of strata, is the prodigious quantity of fossil shells which it contains. In order to give an idea of the number of species which these strata contain, it will be sufficient to say that M. Defrance has found more than six hundred species, all of which have been described by M. Lamarck.

We have to remark that most of the shells of the lowest of these strata are much further removed from the present existing species, than those of the upper strata. We shall mention among the fossils peculiar to these lower strata, periwinkles, oysters, muscles, pinnæ, calyptreæ, pyrulæ, large tellines, terebellæ, porpytes, madrepores, and particularly nummulites and fungites.

Such are the names of the most remarkable shells in this stratum: we ought to remark that it was not in the depôt of Grignon alone that we gathered the specimens we have described; such examples would not have characterized the system of strata which we wish to make our readers acquainted with: we chose them from the quarries of Sevres, Meudon, Issy, Vaugirard, Gentilly; in the strata of Guespelle; in those of Pallery near Chaumont, &c.

It is in this same stratum that we find the camerines. They are either by themselves or mixed with madrepores and the preceding shells. They are always the lowest, and consequently the first which are deposited on the chalky formation; but this is not the case every where. We have found some of them near Villers-Cotteret, in the valley of Vaucienne; and at Chantilly on the declivity of the mountain. They are mixed here with shells in good preservation, and with coarse grains of quartz, which, together, make it a kind of puddingstone, on Mount Ganelon near Compiegne, and Mount Ouin near Gi-ors, &c.

Another character peculiar to the shells of this stratum , that they are mostly very entire and well preserved, be-ig easily detached from their rock, and some of them ave preserved their pearly state. In all the preceding aces, and in others, this is less remarkable; because we certained that the sandy calcareous strata, which contain ese shells, immediately follow the plastic clay which covers e chalk; and it is by these multiplied observations that e recognized the generality of the scale which we have id down.

The other systems of strata are less distinct, and we ive not as yet been able to arrange the numerous obser-itions which we have made, in order to establish with ecision the succession of the different fossils which ought characterize them. We can announce, however, that, om our inspection of the quarries to the south and west ' Paris, from Gentilly to Villepreux and Saint-Germain, e strata above those which we have described succeed ch other in the following order.

1. A soft stratum frequently of a greenish colour, this is lled the *green bank* by the workmen. It frequently ex-bits on its lower surface brownish marks of leaves and alks of vegetables.

2. Gray or yellowish strata, sometimes soft, sometimes ry hard, and containing chiefly roundish venuses, cam-eys, and particularly tuberculated cerites, which last are metimes in prodigious quantity. The upper and middle rt of this stratum, frequently hard, is a very good stone ilding, and is known by the name of *the rock*.

3. Lastly, and towards the upper part, a stratum, not ick but hard, which is remarkable from the prodigious antity of small long and striated tellines which it pre-nts in its seams. These tellines are laid horizontally, d closely wedged against each other. They are generally ite.

Above these last strata of coarse limestone we find hard lcareous marles divided into fragments, the faces of which e generally covered with a yellow coating and with black ndrites. These marles are separated by soft calcareous arles, by argillaceous marles, and by calcareous sand, which sometimes agglutinated, and it contains horny silex with rizontal zones. We refer to this system the stratum of e quarries of Neuilly, in which we find crystals of quartz d rhomboidal crystals of variegated carbonate of lime.

But what characterizes more particularly this last system of strata of calcareous formation, is the absence of every shell and of every other fossil.

It results from the observations which we have related:

1st, That the fossils of coarse limestone have been deposited slowly and in a calm sea, since these fossils are deposited by regular and distinct strata; that they are not indiscriminately mixed, and that most of them are in a state of perfect preservation, however delicate may be their texture; the points of the prickly shells being very often unbroken.

2dly, That these fossils are entirely different from those of the chalk.

3dly, That in proportion as the strata of this formation were deposited, the number of the species of shells diminished until no more are found. The waters which formed these strata either have not contained any, or have lost the property of preserving them.

There can be no doubt that things went on very differently in these seas from what they do at present in the waters of the present day; where no strata are formed, the species of shells found in them are always the same in the same regions; we do not find, for example, since we began to fish for oysters on the shores of Concale, that this kind of shell-fish has been replaced by other kinds.

ART. IV. *Gypsous Formation.*

The soil which we are now about to describe, is one of the clearest examples of what is meant by the word formation. Here we find strata very different from each other in their chemical nature, but evidently formed together.

The soil which we call gypsous is not composed of gypsum alone, it consists of alternate strata of gypsum and of argillaceous and calcareous marle. These strata have pursued an order of superposition, which has been always the same, in the great gypsous girdle which we have studied, and which extends from Meaux to Triel and Grisy. Some strata are wanting in certain districts; but those which remain are always in the same respective position.

The gypsum is placed immediately above the limestone, and it is impossible to doubt this superposition. The position of the gypsum quarries of Clamart, Meudon, and Ville-d'Avray, is above coarse limestone wrought in the same places; that of the quarries of the mountain of Triel, the superposition of which is still more evident; lastly, a well

dug

dug in the garden of M. Lopez at Fontenay-aux-Roses, which first passed through the gypsum, and afterwards the limestone, are ample proofs of the position of the gypsum on the limestone.

The gypsous hillocks have a peculiar appearance, which renders them conspicuous at a great distance: as they are always placed upon the limestone, they form on the higher eminence a kind of second conical or elongated hillock, always distinctly marked.

We shall exhibit the details of this formation, by taking, as an example, the mountain which presents the most complete collection of strata; and although Montmartre has been already well described, it is still the best and the most interesting example that can be selected.

We have recognized, both at Montmartre and on the hillocks which seem to form the continuation of this eminence, three masses of gypsum. The lowermost is composed of alternate and thin strata of gypsum, frequently selenitous, of solid calcareous marles, and of very scaly argillaceous marles. It is in the former that we chiefly see the coarse crystals of lenticular yellowish gypsum, and it is in the latter that we find the menilite silex. We are not acquainted with any fossil in this mass, which is the third, of the quarries.

The second or intermediate mass differs from the foregoing only because the gypsous beds are thicker, and the marley strata are less numerous. We ought to remark that among these masses, that which is argillaceous, compact, and of a marbled gray appearance, serves for building-stone. It is chiefly in this mass, that fossil shell-fish have been seen. No other fossils, however, are found in it; but sulphated strontian has lately been discovered in it, in scattered fragments, in the lower part of the marble-like marle.

The superficial mass, which the workmen call the first, is in every respect the most remarkable, and the most important. It is, besides, much more extensive than the rest, since it is in some places 25 metres (82 feet) thick, interrupted only by a small number of marley strata; and in some places, as at Dammartin and Montmorency, it is situated almost immediately under the vegetable earth.

The lowermost beds of gypsum in this first mass contain silex. The silex and gypsum seem as if mutually dissolved in each other. The intermediate-beds are naturally divided into coarse prisms, with several planes, which M. Desmarets has drawn extremely well. They are called the *high pillars*. Finally, the uppermost strata are interlaid with marle: they are but thin, and are alternated with strata of marle; there are generally five such, which are continued to great distances.

But these facts, which are already known, are not the most important: we mention them only that they may be brought under view at one glance. The fossils which this mass contains, and those contained in the marle that covers it, present observations of a different interest.

It is in this first mass that we daily find the skeletons of unknown birds and of quadrupeds, which have been already described by one of us (M. Cuvier) in a separate memoir *. To the northward of Paris they are in the gypsous mass itself; here they have preserved their solidity, and are only surrounded by a very thin stratum of calcareous marle; but in the quarries to the southward, they are frequently in the marle which separates the gypsous strata: they have then a great degree of friability. We shall not revert to the manner in which they are situated in the mass; upon their state of preservation, species, &c. these objects having been sufficiently developed in the Memoirs which we have mentioned. We have also found in this mass, bones of tortoises and skeletons of fish.

But what is much more remarkable, and much more important from the consequences that result from it, is, that we find, although very rarely, fresh-water shells. Indeed, one only is sufficient to demonstrate the truth of the opinion of Lamanon and some other naturalists, who think that the gypsums of Montmartre, and of the other hillocks of the basin of Paris, have been crystallized in fresh-water lakes. We shall relate new facts in confirmation of this.

In the last place, the superficial mass is essentially characterized by the presence of the skeletons of mammiferæ. These fossil bones serve to point it out where it occurs in isolated masses; for we have never been able to discover that they have been found in the lower masses.

Above the gypsum are placed strong strata of marle, sometimes calcareous, and sometimes argillaceous.

It is in the lower beds, and in a white and friable calcareous mass, that we have at various times met with trunks of palm-trees converted into silex. They were lying flat, and of a large bulk. It is in this same system of strata that we found (but only at Romainville) shells of the genus *lymnea* and *planorbes*, which seem to differ in no respect from the species now existing in our marshes. One of us has already communicated this important fact to the class.

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* Published in *Annales du Muséum.*

t proves that these marles are of fresh-water formation, ike the gypsums which they cover.

Above these white marles are also seen very numerous and frequently thick argillaceous or calcareous marles, in which no fossil has been as yet discovered.

We afterwards met with a small bed six decimetres (24 nches) thick, of a scaly yellowish marle, which contains owards its lower part scraps of earthy sulphated strontian, and a little above a thin bed of small elongated tellines, which are lying flat, packed closely into each other. This bed, which seems to be unimportant, is nevertheless remarkable n the first place from its great extent: we have observed it over a space more than ten leagues (27½ miles) long and more than four (11 miles) broad, always in the same place and of the same thickness. It is so thin, that we ought to know precisely where it lies, in order to find it out. Secondly, because t serves as a limit to the fresh-water formations, and indicates the sudden commencement of a new marine formation. In fact, all the shells which we meet with above this bed of tellines are also marine.

We find at first, and immediately afterwards, a strong and constant stratum of greenish argillaceous earth, which from its thickness, colour, and continuity, may be recognized at a great distance. It serves as a guide to the tellines, since it is beneath it that we find them. It contains no other fossil, but merely argillo-calcareous geodites and scraps of sulphated strontian. This earth is employed in the manufacture of coarse pottery.

The four or five beds of marle which succeed the green earths are not thick, nor do they seem to contain fossils; but these beds are immediately covered by a stratum of yellow argillaceous marle, which is strewed with fragments of sea shells which belong to the genera cerites, trochi, mactres, venus, cardium, &c. We also meet with fragments of the bones of a thornback.

Almost all the beds of marle which succeed the latter present fossil sea shells, but they are bivalves only; and the last strata (those which are immediately below argillaceous sand) contain two very distinct oyster beds. The first and lowermost is composed of very thick large oyster-shells: some of them exceed a decimetre (4 inches) in length. Afterwards comes a stratum of whitish marle, without shells, then a second very strong oyster bed, but subdivided into several beds. These oyster shells are brown, much smaller and thinner than the above. These last beds of oysters are very frequent, and we have not perhaps seen them twice fail, in the numerous hillocks of gypsum which we have examined. The gypsous formation is frequently terminated by a mass, more or less thick, of argillaceous sand, which contains no shells.

Such are the strata which generally compose the gypsous formation. We were at first led to divide it into two, and to separate the history of the marine marles of the top from those of the gypsum, and of the fresh-water marles at the bottom; but the strata are so similar to each other, and accompany each other so constantly, that we have thought it right merely to point out this division without making it in reality.

It now remains to say a few words, as to the principal differences presented by the hillocks which belong to this formation. The gypsous hillocks form a kind of long and broad zone directed from the south-east to the north-east, over a breadth of about six leagues (16½ miles). It seems that in this zone it is only the eminences of the centre which present distinctly the three masses of gypsum. Those of the edges, such as the plaster quarries of Clamart, Bogneux, Antoni, Mont Valerian, Grisy, &c., and those of the extremities, such as the plaster quarries of Chelles and Triel, possess only a single mass. This mass seems to us to be analogous to that which the workmen call the first, *i. e.* the most superficial, since we find in it the fossils of the mammiferæ which characterize it; and since we do not find, in its marles, those coarse and numerous crystals of lenticular gypsum which we observe in the marles of the second and of the third mass.

Sometimes the marles above, are almost entirely wanting; sometimes the gypsum itself is totally wanting, or reduced to a thin bed. In the first case the formation is represented by the green marles, accompanied with strontian. The gypsous formations of the park of Versailles near Saint Cyr, those of Viroflay, are in the former state, and those of Meudon and Ville-d'Avray are in the latter.

We ought to mention here what has been said of another work *, namely, that the gypsous soil of the environs of Paris cannot be referred with accuracy to any of the formations described by M. Werner or his followers. We have on the above occasion assigned our reasons, which it is needless to repeat.

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* Brogniart, Minéralogie, tom. i. p. 177.

ART. V. *Formation of Sea-Sand and of Freestone.*

This soil is not extensive, and seems to form a succession
the formation of the marles of gypsum. We should
en have brought them together, if it had accompanied
em as constantly as the marles accompany the gypsum,
d if it had not been frequently separated by a consider-
le argillaceous mass, stripped of every fossil, and very
ferent in its nature from that which we are now about to
scribe.

What we have said, shows that this formation generally
vers the gypsous formation. It consists in beds of sili-
ous sand, frequently very pure and agglutinated into free-
ne, which contains sea shells of various kinds, and all
them of the same kind as those of Grignon. We have
re recognized the same oysters, the same calyptreæ, the
me tellines, and the same cerites.[*] Sometimes the shells
emselves exist, and are in a calcareous state, while in
her places nothing remains of them but the external im-
essions or moulds.

We found this freestone and sea sand on the top of Mont-
artre, at Romainville, at Saint Prix near Montmorency,
ngjumeau, &c. In these last we also remark fossil
lani.

We cannot help reflecting, when looking at these free-
nes, filled with the same shells as those of Grignon, on the
gular circumstances that must have presided over the
rmation of the strata we have examined. On beginning
th the strata after the chalk, we may represent to our-
lves in the first place a sea which has deposited an im-
ense mass of chalk and mollusci of a particular species.
is precipitation of chalk and the shells which accompany
suddenly ceases. Strata of a totally different nature suc-
ed it, and nothing else is then deposited but clay and
nd without any organized body. Another sea returns:
is last contains a prodigious quantity of testaceous mol-
sci, all of them different from those of the chalk. It
rms at bottom thick beds, in a great measure composed
the testaceous envelopes of these mollusci; but this pro-
ction of shells gradually diminishes, and also suddenly
ases. The soil seems then to have been covered with
sh water; alternate strata of gypsum and of marle are
rmed, which surround both the bones of the animals
hich these waters nourished, and the bones of those which
ed on their shores.

The sea seems to have returned a third time, producing some species of bivalve and turbinated shells; but very soon after the same sea gives birth to oysters only. Lastly, the productions of the lowermost second sea reappear, and we find on the top of Montmartre the same shells which were found at Grignon, and in the bottom of the quarries at Gentilly and Meudon.

Art. VI. *Formation of siliceous Limestone.*

The formation of which we have spoken is parallel, as it were, to that of the siliceous limestone. It is neither situate above nor below, but on one side of it, and seems to retain its place in the immense extent of soil which it covers to the west and south-west of Paris.

This soil is placed immediately above some plastic clays. It is formed of distinct beds of limestone, sometimes soft and white, and sometimes gray and compact, and very fine grained, penetrated with silex which seems to have filtered in every direction. As it is often porous, this silex, by filtering into the cavities, has fringed their sides with tufted stalactites (*mamelonées*) variously coloured, or with very short crystals of quartz, almost without any prism, but clear and limpid. This disposition is very remarkable at Champigny. The compact limestone thus penetrated with silex yields, on being burnt, lime of a very good quality.

But the distinguishing character of this singular formation, which no person had remarked before we did, although it covers a considerable extent of soil, is this, namely, that it contains neither *marine* nor *fluviatile* fossils; at least we have not been able to discover any in the great number of places where we have examined it with the most scrupulous attention.

It is in this soil that we find the stones known by the name of burrstones (*meulieres*): these stones, the origin, formation, and situation of which were unknown to most mineralogists, seem to be the siliceous impression of the siliceous limestone. The silex, stripped of its calcareous parts by an unknown cause, must have left, and does in fact leave, porous but hard masses, the cavities of which still contain argillaceous marle, which present no trace of stratification. We have made true artificial burrstones by throwing this siliceous limestone into nitric acid. We shall explain in our full Memoir the various districts of soil which are formed of this limestone. We shall finish its general history

tory by saying, that it is frequently exposed on the sur-e of the earth, but frequently also it is covered with ar-aceous marles, with freestone without shells, and finally h alluvial soil. Such is the nature of the soil of the ests of Fontainebleau.

Art. VII. *Formation of Freestone without Shells.*

The freestone without shells, in whatever place it is nd, is always either the last or the penultimate formation. constantly covers the rest, and is never covered, except the formation of alluvial soil. Its beds are frequently y thick, and mixed with beds of sand of the same na-e. The sand which supports the upper beds, has been netimes washed out by the water; the beds are then ken, and have rolled over the flanks of the hillocks which y formed: of this kind are the freestones of the forest Fontainebleau, of Palaiseau, &c.

Not only do this freestone and sand contain no fossils, they are frequently very pure, and furnish sands much eemed in the arts, and which are gathered at Etampes, ntainebleau, Aumont, &c.

They are sometimes, however, either altered by a mixture argil, or coloured by oxides of iron, or impregnated h carbonated lime which has penetrated them by infil-ion when they are covered by the calcareous soil of fresh ter: this is still the case with the freestone of several ts of the forest of Fontainebleau.

Art. VIII. *Formation of the Fresh-water Soil.*

This formation constantly covers all the foregoing. The k which has resulted from it resembles, in point of struc-e and other external properties, siliceous limestone, *i. e.* s sometimes compact, sometimes white and soft, but al-st always penetrated with siliceous infiltration. The ne silex, sometimes opaque and yellowish, sometimes wn and translucid like pyromatic silex, in some places npletely fills the place of the limestone: finally, this mation gives, like the sixth, burrstones, the origin of ich has one and the same cause.

What exclusively therefore characterizes this formation is, the one hand the presence of shells, which are evidently fresh-water origin, and similar in every respect to those ich we find in our marshes. These shells are lymnese l three kinds of planorbes. We also find in this formation

tion small round hollow bodies (*canelés*) which M. de Lamarck has called *gyrogonites*. We are not acquainted with any analogous existing species: but their position informs us, that the organized body of which they formed part lived in fresh water.

The second character of this formation is the facility which the limestone composing it has of being dissolved in water, however hard it may seem at the moment of its being taken out of the quarry. Hence it is used as marle, fertilizing the soil at Trappe near Versailles, and other places.

We refer to this formation, but rather with hesitation, the sands of the eminences which contain wood and parts of vegetables changed into silex. We were led to make this junction, by observing the siliceous wood and vegetables, which we find towards the top of the hillocks of Longjumeau. The same sand which contains these vegetables, also contains silex filled with coarse lymneæ and planorbes.

The fresh-water soil, although always superficial, is not found in every situation, but rather towards the summits of the eminences, and on the great platforms as well as in the bottoms of the valleys; if it exists in the latter situations, it has been covered by the soil which constitutes the ninth and last formation. Besides, it is extremely common throughout the whole of the environs of Paris, and probably at distances much further off than we have visited. It seems to us astonishing, that so few naturalists have paid attention to the subject; we know no other person than M. Coupé who has mentioned it.

The presence of this soil pre-supposes, in the fresh water which then existed, properties which we no longer find in those now in existence. The waters in our marshes and lakes deposit nothing but friable slime. We have not remarked in any of them the property possessed by the fresh waters of the old world to form thick depôts of yellowish and hard limestone, of white marles and silex, frequently very homogeneous, enveloping all the ruins of the organized bodies which lived in these waters, and even bringing them to the siliceous and calcareous nature of their envelopes.

ARTICLE IX. *Formation of Alluvium* (*Atterissement*).

Not knowing how to designate this formation, we have given it the name of *alluvium*, which indicates a mixture of matter deposited by fresh water. In fact, the slime of alluviation

/iation is composed of sand of all colours, marle, or even
the mixture of these three substances, impregnated with
·bon, which gives it a brown and even black appear-
ce. It contains rounded flints; but what characterizes
more particularly is, the remains of the huge organized
dies with which it abounds. It is in this formation that
find large trunks of trees, bones of elephants, of oxen,
telopes, and other large mammiferæ.

It is also to this formation that we may ascribe the ac-
mulation of flints at the bottom of valleys; and probably
o those of some plains, such as the Bois de Boulogne, the
.in of Nanterre at Chateu, and certain parts of the forest
St. Germain.

This alluvium is not only found in the bottom of our
:sent valleys, but it has covered valleys or excavations
.ich have been since filled up. We may observe this ar-
igement in the deep cutting made near Seran, for the ca-
of Ourque. This cutting shows an ancient cavity, filled
:h the substances which compose the alluvium, and it is
this kind of marshy bottom that we have found bones of
phants and large trunks of trees.

It is to the existence of these ruins of organized bodies
iich are not yet entirely decomposed, that we ought to
:ribe the dangerous and frequently pestilential emana-
ns which are extricated from these soils, when they are
:red up for the first time since the period of their forma-
n; for it is the same with this formation, which appears
be so modern, as with all those others which we have
imined. Although very modern in comparison with the
ier soils, it is still anterior to any historic æra; and we
iy say, that the alluvium of the old does not in any re-
:ct resemble that of the present world, since the wood
d animals found in them are entirely different, not only
m the animals of the countries where they are found de-
iited, but also from all those hitherto known.*

VII. Ac-

VII. *Account of an extensive Organic Lesion of the Brain, thoracic and abdominal Viscera, unaccompanied by the Symptoms usually observed in similar Affections. By* JOHN TAUNTON, *Esq. Lecturer on Anatomy, Surgeon to the City and Finsbury Dispensaries, and to the City Truss Society, &c.*

To Mr. Tilloch.

SIR, IN the course of my various communications to your Journal, I have more than once had occasion to describe cases of injuries to vital parts, which are generally supposed to produce a suspension if not a total deprivation of the reasoning faculties *.

In detailing the remarkable appearances I am about to lay before the public, it is not my wish to be regarded as espousing a doctrine contrary to the generally received opinions respecting the connexion that subsists between the organic structure and intellectual faculties of mankind:—I am anxious, however, that the anomalies which daily fall under the observation of medical practitioners may excite them to further inquiries. It has been my lot to witness cases in which the bones of the cranium have been so demolished, that large portions of the brain escaped from the wound; and on other occasions besides the one I am about to detail, I have found tumours of an inch and upwards in diameter, formed on the surface and substance of that viscus, and yet no symptoms of mental derangement, stupor, or even loss of sight, hearing, &c., supervened. On the other hand, I am free to admit, that I have seen lesions of this description, nay even the most trifling depressions of the skull, producing all the disagreeable consequences which we are taught to expect from injuries of the head. May there not be some chasm in the theory of physiologists on this point, which is reserved to future observers to fill up?

In November last I was called to a M. De la Roche, a foreigner, who described his age as 67. He detailed the circumstances of his case, with a firmness and precision, which perhaps no patient, labouring under such a complication of disorders, ever before evinced. On examination I found a fistulous sinus in the glutii muscles, which he informed me had existed for many years: he had long complained of great pain in the left cavity of the thorax, often

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* See Philosophical Magazine, vol. xxix. p. 169, and vol. xxx. p. 363.

en placing his hand on the affected part:—in other re-
cts his health had been previously good. He was re-
ded as a most active man: he had a general knowledge
literature and science; spoke several languages; was
tinguished for the facility with which he could converse
on most subjects, and reasoned so closely that his intel-
:ual powers were generally regarded as of a superior kind.
had been repeatedly employed by the late Mr. Pitt on
sions to various parts of the continent, and had invent-
several improvements in mechanics, which had gained
1 considerable notoriety.

About two months previous to my seeing him he had
n seized with what was pronounced by a medical gen-
nan who visited him, to be peritoneal inflammation.
s gentleman having discontinued his attendance, I was
t for by the patient; not so much with a view to admi-
:er any professional aid, as to express my consent to
natomize his body" after his dissolution, which he
er ceased to contemplate as rapidly approaching, with a
mess of mind peculiar to himself. This singular, and
elieve almost unprecedented request, was even com-
ted to writing by the patient, and delivered to a parti-
ır friend.

On further inquiry I learned that he felt considerable
1 also in the abdomen, and on applying my hand ex-
ıally I felt a tumour in the scrobiculus cordis, extending
ıbout midway between that part and the umbilicus; but
tumour was not sensible to the mere casual touch. His
se was good, and did not indicate inflammation; but he
eared to be gradually sinking; he took but little nourish-
ıt, and did not rest well.

'he medicines administered were opiates with aperients,
ich afforded some temporary relief. A few days previous
ıis death he had several convulsive fits: he lost the use
ıis left side before death; but the circulation and heat
hat part of his body were uninterrupted; the lower ex-
nities at this time also became anasarcous. He con-
ıed perfectly sensible to the moment of his dissolution.

Examination of the Body.

On sawing off the upper part of the cranium, the bones
eared perfectly diaphanous;—the membranes and surface
he brain were in their natural state. The general sub-
ıce of the brain was firm.

On separating the hemispheres, there appeared attached
he right hemisphere a dark-coloured tumour of at least
an

an inch in diameter; it was of a granulated, fibrous and grumous consistence. Within the substance of the same hemisphere there appeared several other tumours, which, on being cut into, exhibited nearly the same appearances. The general substance of the brain was firm and sound: the olfactory nerves were uncommonly firm, not having the usual fibrous texture, but rather resembling pieces of narrow tape, of a dead white colour; the other nerves were equally firm.

In the thorax the left lung was entirely obliterated, apparently resulting from long continued inflammation. The right lung and the heart were in their natural state. The coronary arteries were ossified to a considerable extent*.

In the abdomen there was about three quarts of a dark-coloured serous fluid; the viscera were of a very dark colour. The structure of the alimentary canal was not injured: the kidneys, bladder, and spleen, were in the natural state: the liver was completely scirrhous: the pancreas was also scirrhous, which must have formed the tumour felt while the patient was alive. The gall bladder was much distended with bile.

There were no symptoms of a scirrhous pancreas during life.

With respect to the diseased appearances of the trunk in the above case, perhaps little doubt can arise; but it may not be improper, before I conclude, to call the attention of my fellow practitioners to the peculiar state of the brain.

The tumours in this organ were exhibited in various stages of progression to the size of the largest, and that which was first described. The whole of them must have existed for many years, and it will perhaps be difficult to explain—Why these tumours did not produce symptoms of compression, considering the space they occupied? Or are we to suppose that absorption of the surrounding parts took place in unison with the growth of the tumour?

I am, &c.,

JOHN TAUNTON†.

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* Although this ossification was extensive, no symptoms of angina pectoris could be traced as having ever existed. Several preparations in my possession, when considered with reference to the histories of the patients, furnish results perfectly analogous.

† The preparations of the various diseased parts in the above case are preserved in my Museum, where they may be seen.

II. *On Dr.* Pearson's *Proposal for an Institution for obtaining an equal Temperature in Houses.*

To Mr. Tilloch.

R, I WAS much gratified by Dr. Pearson's letter in your t number, announcing the probable erection of a large ilding, capable of having its atmosphere kept at an equable nperature, for the use of consumptive patients. Though medical man myself, I have more than once had my ention directed to this subject; and I have long been of inion, that such a building as Dr. Pearson alludes to, uld be, in most cases, a palliative, if not remedy, superior the removals to a milder foreign climate, which are so quently painfully undertaken. That a combination of architectural and philosophical sciences existing in gland is adequate to the erection of a building, in which air could be constantly maintained of a temperature as ld as that of Montpellier or Madeira, and of which the erior equability would more than compensate for any ght deficiency in freshness and purity, there can be no sonable doubt; and how infinitely more, if this could effected, a quiet residence at home, surrounded by attive friends, must contribute to the care and restoration a sufferer by a consumptive attack, than a harassing yage, succeeded by an uncomfortable sojourn amongst angers, need not be pointed out.

But, though I entertain sanguine hopes that, under the spices of Dr. Pearson and the eminent architect he re- s to, the consumptive rich may be shortly accommodated ongst us with a splendid erection of another *Albany*, which while the air is as mild and balsamic as that of thern France, the extent and arrangements will admit the indulgence of fashionable habits, and combine within elf the luxurious enjoyments and comforts of home, with healthful influence of foreign climes; it would greatly ract from my satisfaction, if I conceived that these bless- gs were to be confined to the rich, and that the con- uction of a splendid and expensive edifice was a *sine quâ* n to their attainment. I am persuaded, however, both m Dr. Pearson's hints and my own observations, that at st the remedy of a mild and equable temperature may be mpassed by persons in the middle station of life, in their n houses, and at an expense not exceeding what they uld of necessity expend in applications far less efficaus. The first essential seems to be merely a suite of two air-tight apartments and an anti-room opening into each other, and having no other communication with the rest of the house than through the anti-room. Of these the first would serve for a sitting-, the innermost as a lodging-room. It is quite obvious, that all attempts at maintaining an equable temperature must be nugatory, if the patient have to pass from his sitting- to his lodging-room through a cold windy passage or staircase; and if the atmosphere of both be not kept at the same degree of warmth. There are few houses of modern construction that could not supply two rooms on the same floor opening into each other: and as the anti-room, which is essential for the purpose of preventing a cold draught of air on opening the door of the outer apartment, and for renewing the air of that apartment gradually, may be very small, there are few houses which would not admit of its erection on the stair-head of the first floor. The next essential is to make the rooms air-tight. This may be readily accomplished by double windows, the outer closely caulked, by walls accurately plastered and papered, and doors tightly fitting and listed if necessary. An equable temperature is the third essential. In effecting this, fires in an open stove seem clearly inadmissible, if from no other cause than this, that in an air-tight apartment it is almost impossible to prevent their smoking; besides which, it is equally difficult to regulate their heat properly. *Stoves* of all kinds are open to the last objection, and frequently cause an unpleasant and unwholesome smell, even when the smoke does not escape. *Steam* would doubtless be the most effectual and elegant, as well as simple and perhaps cheap mode of heating the apartments. All that is wanted is a plain and intelligible description of the mode of its application, which could be practised by any ordinary workman. Here I confess my ignorance: indeed my chief reason for now addressing you is to request of Dr. Pearson, or some other of your intelligent correspondents, the requisite information. We were told long ago, that some eminent cabinet-maker was able to warm a large suite of rooms, even the garrets of his warehouse, from the steam of a single copper; and I understand steam is applied also in warming the Royal Institution: so that the practicability of applying it in this way is undoubted. What is wanted is the manner of its application, on a small scale, in private houses. I should conceive that a boiler, which might be fixed on one side of the kitchen-fire, would heat water sufficient to warm the air of two moderate-sized rooms. From this bo-

iler a tin pipe might be conducted, being wrapped with
ol, or some non-conducting substance, until its en-
ance into the apartment to be warmed, and then unco-
red to give out its heat. If sufficiently long or wide,
d made always to incline towards the boiler, I should
nceive that the greater part of the steam would condense
d run back into the boiler; but of course there ought to
some outlet or valve to prevent the pipe's bursting. The
ief data wanting are the size of the boiler and quantity
water necessary to heat a given space; the proper dia-
eter of the pipe, the length to which it ought to extend
the room to be warmed, and its situation, whether near
e floor or the cieling, &c. &c. It is clear, that as little
the steam ought to escape uncondensed as possible; and
r this purpose, the tube in the room should be either very
ng or very wide, but which I know not. If the appa-
tus could be so contrived that the condensed water would
turn to the boiler, it would be a material point; for the
eat difficulty in adopting such plans is the impossibility
getting servants to attend to any directions which re-
ire frequent and precise observance. It is on this ac-
unt that the steam-boiler should be immoveably fixed,
d constantly heated without any particular care.

A steam apparatus, something on the plan above de-
ribed, would, I conceive, be far more effectual, as well as
s troublesome, than the pots of hot water temporarily
ed by Dr. Pearson: and a simple and cheap mode of
plying it is highly desirable, not merely as a mode of
ating the apartments of consumptive patients, but for
neral adoption in many other cases; in particular, for
ating rooms where collections of plants, &c. are to be
pt dry, but free from the dust and dirt which an open
e never fails to make. In concluding this part of the
bject, I wish to inquire whether the Pennsylvanian stove,
vented by Dr. Franklin, and which, from his discoveries,
ems to combine the warmth and cleanliness of a stove
th a sight of the fire, so essential to an Englishman's
mfort, has ever been adopted in this country, or had a
r trial given to it?

Though there can be little doubt that a suite of rooms
armed in the manner indicated above would be a very
od succedaneum during an English winter for an ex-
nsive voyage to Madeira or Lisbon, and though they who
e impressed with a proper sense of the horrors of con-
mption would deem the consequent confinement as a
fling price for the advantage derived; it is not to be de-

nied that it would be found very difficult to induce consumptively-inclined patients to forgo the pleasure of out-door exercise, and submit to such a confining regimen. But it has long struck me, that means might be devised to enable this numerous class, who purchase every sunny walk in winter with the risk of their existence, to enjoy their present liberty with far less danger. It is well known, that the great source of harm to persons with tender lungs is the sudden and great changes of temperature, especially from *cold* to *heat*, which no one who stirs out in an English winter can well avoid. The succession of a cold frosty air to the hot temperature induced by a large fire and tea; then perhaps a walk on the sunny side of a street, exchanged for the piercing cold of the shaded and exposed side; and the whole ended by entering into a hot room, and rushing to a large fire, where the irritability, or whatever we call it, of the body is extraordinarily condensed:—such is the succession which thousands are every winter repeatedly exposing themselves to: And can we wonder at the result to those who are of a consumptive habit? But how to avoid these changes and yet stir from home is the question. In one way this may be certainly effected:—by the application of a handkerchief to the mouth and nose, so as to prevent the air from ever passing into the lungs when in its coldest state, much of the danger of short excursions in the open air may be prevented. Of this fact I have had ocular demonstration in the case of a friend, who, before his adoption of this plan, had constant and severe colds through every winter, but, since he pursued it, has been nearly if not altogether free from them. And this gentleman is now able comfortably to accept winter invitations, which formerly he was under the necessity of declining, or of looking forward to with horror.

But would it not be practicable to invent a substitute for the pocket-handkerchief, which it is tiresome to hold, and too closely confines the breath? Might not a kind of mask consisting of a frame, which should closely apply to the lower part of the face, covered with three or four thicknesses of gauze, be very advantageously employed for the same end? Such a veil would, I conceive, constantly keep the lungs in an atmosphere never much below sixty, while the interstices of the gauze would readily admit of the requisite quantity of air, which, thus gradually mixed with the interior warmer mass, could never, even in the coldest weather, cool the membrane of the lungs so as to make it dangerous to come into a warm room, or approach a fire.

The

he only objection that I can see, is the ridiculous figure ıat persons furnished with such masks would at first be ought to cut. But this would soon have an end. The doption of such veils by a few of the great would be suffiient to induce even the robust to wear them; and in relity there would be much less to laugh at than there was t the first use of umbrellas, inasmuch as a man's lungs re somewhat better worth protecting than his coat.

I shall be glad if these hasty and indigested hints prove f any value in furthering the laudable views of Dr. Pearson.

I am, sir, your most humble servant,

nuary 15, 1810. CRNEPS.

IX. *Notices respecting New Books.*

The Rudiments of Chemistry; illustrated by Experiments, and eight Copper-plate Engravings of Chemical Apparatus." *By* SAMUEL PARKES. Price 5*s*. in Boards.

THERE is much to commend in this little volume, which ontains the principal chemical facts, detailed with preision and perspicuity, and illustrated with apposite expements: but after the luminous discoveries of Mr. Davy, —discoveries which have entirely changed the relation and ependence of the various facts which constitute chemical cience,—we cannot but express our surprise that Mr. Parkes hould have adopted such an arrangement as the following:— ntroduction; Atmospheric Air; Caloric; Water; Earths; lkalies; Acids; Salts; Simple Combustibles; Metals; xides; Combustion; Chemical Affinity. In typography nd mechanical structure the present work is an exact picure of Blair's *Grammar of Chemistry*, nor could a better nodel have been followed for an elementary work of this ind; but justice demands that we should add, it is far more orrect in its detail of facts.

An Essay on the Effects of Carbonate, and other Preparations of Iron, upon Cancer: with an Inquiry into the Nature of that and other Diseases to which it bears a Relation." *By* RICHARD CARMICHAEL, *Surgeon. Second Edition, considerably enlarged and improved.* 8vo, *Dublin printed, and sold by* MURRAY, *London.*

Though subjects of medicine may find their way to the ublic by publications confined to that science, yet, as a ranch of natural philosophy, we have always been ready o receive a selection. The disease to which this work refers is too well known in its fatality to require any introductory remarks, but the remedies have for the most part been concealed from the public. Hence, solitary successful cases only having been published with care, the faculty have been unable to judge of the comparative value of a remedy they could know but imperfectly. Nothing therefore could be more desirable than a performance like Mr. Carmichael's, nor could any thing be ushered into the world with more modesty or propriety. "When I first published," says the author, "my Essay on the Effects of Carbonate of Iron upon Cancer, there was nothing I so much dreaded as the too sanguine expectation of the public, and that a remedy that succeeded in one instance would be required in every other to overcome this disease in all its stages, or be rejected as useless because it could not perform impossibilities. My own hopes were but moderate, and I was careful that they should not wander far beyond the certainty of my experience; but my experience was so circumscribed, that I could merely guess at the virtues of the medicine rather than appreciate its value. This is a misfortune I have not now to complain of;—many and various are the cases a short interval has brought within my care or observation—alike in their symptoms, however different their circumstances—and variable the event of success or disappointment. But if experience has taught me, that in particular instances the medicine may prove inefficacious, and *must*, where the ravages of the malady are great and extensive, yet I had almost universally the satisfaction of discovering its efficacy, wherever the cancerous mass was not very much enlarged: and even when this was the case, instances were not wanting of a perfect recovery, and seldom indeed did it happen that the disease was unalleviated by the medicine."

It is no small compliment to the author, that his proposed remedy has been pretty generally adopted by many of the most eminent of the London faculty, and that this adoption is becoming daily more general.

The plan pursued in the arrangement of the work is as follows:

1st. A detail of the most remarkable cases within the author's knowledge.—These are subdivided into such as were cured by iron—such as were alleviated, and such as were neither cured nor alleviated.

2d. The opinions of the ancients and moderns concerning cancer.

3d. Consideration of the nature of cancer.

4th. Treatment of cancer.

These are followed by some miscellaneous remarks on
e predisposition to cancer, and its connexion with other
seases; and an attempt to answer the queries of the so-
ety formed in London for the cure of cancer.

The cases of cancer cured by preparations of iron amount
30, all well authenticated; and many of them com-
unicated to the author from various respectable sources.

The number alleviated is eleven, and those in which the
medy produced no good effects are seven.

We shall pass over the account of the ancient opinions
ncerning cancer, and even of the moderns. Of the lat-
r our author remarks:

"Theories founded on such uncertain arguments scarcely
serve attention, only that they prove how frequently the
mptoms of this disorder have given the notion of its pro-
iction by the action of living animals. They seem to have
arly dropped into oblivion, till revived by Dr. Adams,
ho supports with much ingenuity and appearance of truth,
inions peculiar to himself on this subject. As the foun-
ition of this theory, he premises, that hydatids possess
e simplest form in which animal life can well be sup-
osed to exist; and as the experiments of Doctor Hunter
ly prove in them a contractile force, which is allowed to
sufficient evidence of their life; so if a similar property
in be proved in the contents of a cancerous tumour, *their*
parate vitality is equally deducible.

"In cancerous breasts, he remarks, there is always found
quantity of *yellow greenish fat*, which is contained in
sts, these together he denominates Carcinomatous Hy-
tids; and to prove their contractile power, he directs the
llowing experiment to be made. 'Immediately after the
peration, take the amputated part, and cut it in a trans-
erse, or indeed in any direction, and wherever you dis-
over this fatty appearance, you will see the surface at first
nooth under your knife. In an instant after you will find
papillary appearance all over this yellow green surface.
ach of these papillæ you will find the contents of a cap-
le, the contraction of which has produced this conical
gure.'

"The other observations in support of his theory chiefly
nd to prove, that those parts usually affected with cancer
e endued with but little powers of life; and which there-
re we would, *à priori*, suppose to be best adapted for the
idus and support of beings possessing a separate existence.
mong those are the organs subservient to the preservation
of

of the species and not necessary to the existence of the individual, as the breasts and uterus in women and the testicles in men, which are most suceptible of the disease at that period of life when they become useless, and consequently possess but a small portion of vitality; however, that the cancerous predisposition may be anticipated by injuries, which render them entirely or in part incapable of performing their usual functions.

" As to the scirrhous structure which forms the most considerable portion of cancer, and possesses somewhat the appearance of softened cartilage, he argues, that ' if carcinomata pass through the same stages as Dr. J. Hunter has remarked of the common or lymphatic hydatid, is it not probable that on the death of any of them suppuration will follow, and that this suppuration may expose the living hydatids in such a manner that many of them may die from not being surrounded by living animal matter? To prevent this, he conceives a fungus is formed; which incloses individuals or clusters of them in separate compartments, so that the death of one set produces no effect on the rest.' However, the Doctor does not inform us whether this fungus is produced by the hydatids for their own preservation, or by the surrounding parts for the purpose of preventing the departure of those troublesome visitors; if the latter is the case, it at least proves, that we do not always profit by the assistance of the vis medicatrix naturæ.

" But when the mind rests a length of time on any favourite theory, it is too apt to seize only the evidence that may confirm, and to neglect altogether that which may subvert the opinions it has formed: thus the ingenious author, who has the merit of opening a new field of inquiry, overlooks some obvious circumstances that are strongly adverse to his theory, which appears to me to have its foundation in the following circumstances; viz. the colour, the quantity, and the consistence of the yellow greenish fat above taken notice of, together with its accumulation in capsules, and the papillary appearance expressive of motion observable on dividing it by a transverse section.

" But these appearances seem to be merely the effects of the deranged actions of the animal œconomy; except the one evincing a contractile power in what he terms capsules, but which, notwithstanding repeated investigation, I never could perceive. The colour of the fat in cancerous breasts, that Doctor Adams seems to lay so much stress on, may be produced by animal hepatic air, which Doctor Crawford

s proved to be 'capable of imparting to the fat of animals
cently killed a green colour,' and that this very air united
ith ammonia escapes in great abundance from cancerous
well as other malignant ulcers*."

"I have thus," says our author, "extensively consi-
ered the opinions of Dr. Adams, which, notwithstanding
e objections that occur, appear to explain the phænomena
the disease more satisfactorily than any that have yet
een offered; and I confess I cannot but agree with him
the fundamental part of his theory, *the independent life*
cancer; but my sentiments are somewhat different con-
rning the part in which that life is resident."

Mr. Carmichael then proceeds to offer his opinion of
ncer as a parasytic animal (a term, we believe, first used
Doctor Adams). This leads to a very interesting history
parasytic plants and animals, which he traces through all
e writings of Darwin, Willdenow, Hunter, and several
hers.

In the succeeding chapter the author enters more parti-
larly into the evidence of the vitality of cancer. The first
these is, that the cancerous substance has no communi-
ating vessels with the parts in which it grows, and the in-
nsibility of the person in whom it finds a nidus to any
jury confined to the cancerous mass. 2d. That carci-
oma arises in parts naturally endued with little life, or
hich, from their nature, are more inclined to run into
ecomposition, particularly the organs in each sex subser-
ient to generation, after the period is passed in which they
an be used for such purposes. 3d. From the fair presump-
on that when suppuration takes place, it is not of the can-
erous substance itself, but of the neighbouring parts, which
re stimulated to suppuration by the previous death of one
art of the carcinomatous mass, according to a law first
iscovered by Mr. Hunter, that a living animal confined
ithin the substance of another animal does not stimulate
o suppuration; but that the same animal when dead sti-
nulates like any other extraneous substance. 4th. "The
rigin of carcinoma first commencing in a point—the forma-
ion of cysts in its texture, containing a fluid—those cysts
vincing a contractile power, by a forcible expulsion of their
ontents on being punctured, are all circumstances which
trongly impress the idea that carcinoma is possessed of in-
lividual life." In this division the author enters much

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* Experiments and Observations on the Matter of Cancer, &c. by
. Crawford, M.D.

into the general relation between hydatids and cancer. 5th. The locality of the disease. 6th. The peculiar kind of pain, which is sometimes compared by the patient to the gnawing of an animal.

"Such are the facts," concludes Mr. C., "that lead me to adopt the unnatural hypothesis that cancer enjoys an independent animal existence in the body on which it preys; and when another theory offers itself that so precisely meets every circumstance, and so perspicuously disentangles every difficulty of this obscure and intricate disease, I shall without hesitation relinquish opinions that I confess are difficult to digest, and most difficult to him whose reading has been most extensive. Vain indeed would be any effort to render them palatable to minds versed in systems of every branch of philosophy, if they had not previously learned the vanity of all human knowledge, and the futility of attempting to embrace within systems the infinite variety of nature."

We cannot help remarking, that most if not all these arguments in favour of the vitality of cancer are urged by Dr. Adams. The second indeed—that carcinoma arises in parts naturally endued with little life, is somewhat differently stated by that author. In accounting for the breasts, ovaries, uterus, and testicles becoming the nidus for hydatids in general, as well as for what he calls carcinomatous hydatids, Dr. Adams assumes as a cause, that such parts are not necessary for the existence of the supporting animal; and that after a certain time they become useless for those offices for which only they are destined.—That by injury that uselessness may be anticipated, and that in some females who are barren, the parts *ab initio* being useless, the breast becomes the nidus for cancer at a very early period.—The last argument produced by Mr. Carmichael we think might be omitted. Gnawing pain is a very common term for rheumatism—darting is a much more usual description for cancer among the unhappy sufferers. We mean not by these remarks to detract from the candour or merits of the author. He seems to have omitted scarcely any other opportunity of introducing Dr. Adams's name, and always with respect: and as to his own opinions, it must be admitted that they stand on the fairest ground—the success of a generally adopted practice.

Mr. C. next enters on the "treatment of cancers." In this most important part, and which, notwithstanding the envy of some of our contemporaries, we think we may truly call his own, as far as any philosophical view of the subject

ject extends, we are ready to give every credit to the
genious author. Our limits will not permit us to follow
n through the whole, but we cannot help remarking,
it every part shows equal industry and genius.—After
narking the various unsuccessful attempts hitherto made,
engages in an inquiry concerning the uses of iron in the
item—The diseases arising from an excess of the oxide
iron—Those arising from a deficiency of the same From
these, many valuable inductions follow on cancer and
me other equally deplorable diseases.

The work concludes with an attempt at answering the
eries of the medical committee of the society for investi-
ting the nature and cure of cancer, and the plan of
iospital in Dublin for the reception of cancerous pa-
nts. Of the first, we cannot help lamenting that we hear
little of a society which seemed at one time to promise
much. The author's answers of course respect princi-
lly passages referred to in his work. Of the second, we
nnot help regretting that the inconvenience remarked by
:. Adams, in his Treatise on the Cancerous Breast, should
so soon forgotten. Can there be a more dreary prospect
r cancerous subjects, than to be constant witnesses of
frequent fatality, and the agonies of their fellow-suffer-
s? Would not the funds of such an institution be better
iployed in supporting the patients among their friends,
d administering remedies, than in erecting expensive
ildings? We cannot, by these few remarks, be sus-
cted of a wish to detract from the merit of a work which
es equal honour to the head and heart of the author.

ractical and descriptive Essays on the Economy of Fuel and Management of Heat. Essay First. By ROBERTSON BUCHANNAN, *Civil Engineer. 8vo. With 2 Engravings.*

The public having given a very favourable reception to a
ort *Essay on the Warming of Mills and other Buildings*
Steam, published by Mr. Buchannan in 1807, and of
iich we took notice in our xxixth vol. p. 272, instead of
erely reprinting that Essay, he has been induced to ex-
nd his plan to a series of Essays under the above title, of
hich, the first, consisting of 280 pages, has just made its
pearance. It is divided into three parts. PART I.—Effects
Heat—Means of Measuring it—Fuel, &c.—Section 1,
eat, Thermometers, Tables; 2, Expansion of Solids and
quids, Tables; 3, Specific Heat of Gases, Liquids and
Solids,

Solids, Tables; 4, Combustion, various Kinds of Fuel; 5, Motion of Heat on the conducting Power of Bodies, Refrigeration; 6, Ebullition, Steam, Tables of Expansion of Air, Water, and other Liquids; 7, Ignition. PART II. —Heating Mills and other Buildings by Steam:—Section 1, Proportionate Size of Boilers; 2, Proportion of Steam-Pipes for Heating a given Space; 3, Substance and Surface of the Pipes; 4. General Observations respecting Arrangement, &c.; 5, Of the Method of connecting the Pipes; 6, Description of the Boiler with its Apparatus; 7, Syphons; 8, Arrangements in actual Use in Mills, Dwellings, and Baths. PART III.—Drying and Heating by Steam applied to Manufactures, &c.

The volume before us exhibits in a condensed yet perspicuous manner the principal laws which regulate the phænomena of heat, including all the recent discoveries of modern philosophers, and the best practical application of them which we have yet met with to the various purposes coming within the plan of the author. To civil engineers, and others whose professional avocations embrace such objects as Mr. Buchannan has illustrated, we cannot recommend it in terms beyond its merits.

Mr. T. Woodfall, assistant secretary to the Society for the Encouragement of Arts, Manufactures, and Commerce, has announced his intention to publish, by subscription, in 2 volumes, 8vo, the whole of the very valuable papers on Agriculture which have been brought before the Society. The approbation given by the Society to these documents, which embrace every subject connected with agriculture, and extensive details of valuable experiments and observations, cannot fail to recommend the present undertaking to the notice of the public.

Mr. Ayshford, member of the Royal College of Surgeons, and assistant surgeon in the Royal Artillery, has in the press An Epitome of Anatomy, comprised in a series of tables. The work will form a thin quarto volume; and as its object is to furnish a copious vocabulary for the student of anatomy, perspicuity and simplicity of arrangement have been chiefly aimed at by the author.

X. Proceedings of Learned Societies.

ROYAL SOCIETY.

ANUARY 11,—The president in the chair. This society aving assembled after the holidays, a summary of Mr. ome's observations on dissecting the man who lately died the hospital in consequence of the bite of a rattle-snake as read. Mr. Home related all the symptoms from the time e patient was bitten till his death, which corresponded ith those observed by Dr. Russel at Aleppo, and were; rpor in the part, swelled arm, pain, fever, delirium; low pid pulse, generally 100, mortification and death after 18 ys' suffering. On opening the body, the blood in the ricardium had a frothy appearance, and the skin of the m separated entirely from the muscles, as observed by r. R. Mr. H. related two or three other cases of the tes of snakes terminating in the same manner and with milar effects.

The evenings of the 18th and 25th were occupied in ading a very long and important paper on the comparave qualities and strength of British and foreign culinary lt, by Dr. Henry, of Manchester. After stating the pular prejudice in favour of the supposed superior strength foreign salt, compared with British, Dr. H. proceeded detail the general process of preparing salt in different rts of England and Scotland, compared with the sea or y-salt from St. Ubes, which is vulgarly believed to prerve meat better than English bay-salt. To ascertain there could be any reason for this belief, Dr. H. anased specimens from all the mines and manufactories of e common salt of commerce, and found their results so alogous, as to render any great difference in their curative wers almost impossible. The results of his experiments drew up in a tabular form, exhibiting the constituent prinples of the salt of various places; in 1000 parts he found e quantity of muriate of soda to vary only from 935 to 38, the lowest and highest in any culinary salt. In the reign salt he generally found two or three parts more of uriate of soda than in the British; but this advantage was ore than counterbalanced by the extra quantity of sulates of lime and magnesia in foreign salt: the latter subances also exist in British salt, but generally in much ss proportions, particularly the sulphate of lime. Hence r. H. concluded that our native salt is in every respect ual to foreign salt for the preservation of provisions, and that the vulgar prejudice against it should be instantly removed by every rational and practicable means, as injurious to the commerce and prosperity of the kingdom.

In the process of manufacturing, he observed that in the North of Scotland, where the fire to the pans is allowed to become low by neglect on Sundays, a species of very strong salt has been produced in consequence, which has obtained the name of "Sunday salt," which he thinks even more powerful than any foreign salt. The cause of this superiority he attributes to the slowness of evaporation, which makes the grain of the salt larger. Large-grained salt is best for curing dry meat, as it dissolves more gradually, and always affording fresh supplies of saline moisture; small-grained, on the contrary, is best for making brine. The decrepitation of all salt Dr. H. found nearly alike; the water of crystallization being in very small quantities, not exceeding two or three per cent. in salt dried at the usual temperature of 212. The specific gravity is also very little different; that of St. Ubes was 19·68, while the British varied from 20·23 to 20·88.

In concluding, the author related some of the tedious and complex operations which he adopted in these researches, in order to ascertain the relative and constituent principles of the various kinds of common salt. Luna cornea, or muriate of silver, was one of his principal tests; but the experiments to detect the sulphates of magnesia and of lime were tedious and complex, especially in ascertaining the presence of an ammoniacal sulphate of magnesia. Among many other curious experiments by this able operator, he ascertained the *compatibility* of sulphate of soda and sulphate of magnesia in the same liquid, contrary to the chemical axiom laid down by Mr. Kirwan. It was not, however, till after two days digesting that a very small quantity of these salts was found to be partially united, and from this experiment the author does not seem disposed to question the truth or utility of Mr. Kirwan's position in regard to salts in their natural state.

SOCIETY OF ANTIQUARIES.

Some curious particulars respecting the former perquisites at the Board of Green Cloth, and the conduct of Sir Gilbert Talbot, keeper of the king's plate during the reign of William III., were read. The only useful facts which this paper established were, that in former as well as the present times, avarice, intrigue, violent passions, and love of places and perquisites prevailed.

Mr. Douce exhibited to the society a French marriage
ken, never used as a coin, and consequently not noticed
any work on coins or medals. On the one side it bore
e circular inscription "Pour Epouse," round fleurs de
, with a D at the bottom; and on the other "Denirs de
y," with united hands. These tokens were formerly
en in betrothing brides. Mr. D. quoted several decrees
d ceremonies relative to the performance of marriage,
d among others a decree of the council of Toledo, pro-
iting the queens of Spain to marry a second time.

The Right Hon. Sir J. Banks, Bart. President of the
yal Society, communicated a curious parchment roll, ex-
iting the marks made on the beaks of swans and cygnets
all the rivers and lakes in Lincolnshire, accompanied with
account of the privileges of certain persons keeping
ans in those waters, and the duties of the king's swanherd
guarding these fowls from depredation, and preventing
y two persons from adopting the same figures or marks
the bills of their swans. The number of marks con-
ned in the parchment roll amounted to 219, all of which
re different, and confined to the small extent of the bill
the swan. The outlines were an oblong square, circular
one end, and containing dots, notches, arrows, or such
e figures, to constitute the difference in the marks of each
rson's swans. Laws were enacted so late as the 12th of
izabeth, for the preservation of the swans in Lincoln-
ire.

LINNÆAN SOCIETY.

January 16.—Dr. Maton, vice-president, in the chair.
ad a description of some new species of plants from New
olland, by Edward Rudge, esq., F.L.S. Part of a paper
William Spence, esq., F.L.S., was also read, on a ge-
s of insects named Choleva by Latreille, with a descrip-
n of eighteen British species, which was prefaced by
me remarks on the comparative merits of the different
stems of entomology.

XI. *Intelligence and Miscellaneous Articles.*

FLINT GLASS.

HE French artists still continue their exertions to manu-
ture flint glass to rival if possible that of this country.
our 33d volume we gave a report of a committee ap-
inted by the French Institute to examine some attempts
of

of this kind made by a M. Doufourgerais. A similar report has been recently published by the Institute on a specimen of flint glass presented by Messrs. Kraines and Lançon. It is described as of great purity, and totally devoid of striæ: its specific gravity is to that of the English flint glass as 37 to 33: dispersive powers very great, being as 5 to 2 with common glass, while the proportion of common glass to flint glass is 2 to 3. Its refraction being described as very strong, the foci of the glasses made with this glass are one-fourth shorter than common glass. M. Delambre informs the Institute that he has made experiments with an achromatic glass of the above materials, and pronounces it to be far superior to a telescope of equal size made by Dollond.

HERNIA.

In a recent foreign journal the following new remedy for hernia has been proposed by a M. G. Tarenne:—

"The author of this proposal has made a number of experiments on snails, and on the singular properties of their slimy juice, which has for a long time been used with success in disorders of the breast. The viscidity of this juice, its astringent virtue, and its reproductive faculty, induced him to suppose, that when applied externally in certain infirmities it would easily penetrate the skin, and spread itself throughout the part affected. He presumed that this juice would in some way close hernial openings; and to assure himself of this property he undertook the care of several persons afflicted with hernia. He had the happiness to succeed in curing them radically in the space of three months.

"This discovery appearing to him too important to be kept secret, he did not hesitate to publish it. We are obliged here to abridge his manner of proceeding, and refer our readers to the work published by M. Tarenne, entitled 'Cochliopérie; Recueil d'Expériences très-curieuses sur les Hélices terrestres, ou Escargot,' &c. 1 vol. 8vo. Paris, 1808.

"The first thing necessary is to be assured of the nature of the hernia by consulting some experienced surgeon. If the ruptured part cannot be returned by any means, or if it is dangerous to confine it in the body, this specific must not be made use of, as it would in this case only augment the evil.

"A truss is then to be made, having the ball at the end concave instead of convex, as is usual, to receive a kind of cup

ıp of an equal diameter with that of the orifice of the
:rnia. This cup may be of porcelain, earthenware, or
ass, in order that the liquor which it is to contain may
ıt penetrate it, nor lose any of its virtue, nor undergo
ıy alteration. The edges of this cup are widened a little,
at they may not incommode the patient when it is placed
the truss. It is to be filled with wool, which must be
ıanged every other day.

" About two, three, or four hundred snails, according to
e size, are then to be procured, and kept in a place where
ey may derive nutriment, because only two or three are
ed every day, or six or eight if they are small. They are
ore easily procured, and of a better quality, in spring,
ıich is the most favourable season for this business.

" The patient every day before he rises, and after he is
bed, takes away the cup from the truss, and with a pin
ounds the snail at intervals in different places. From
ch wound the snail gives out, through the opening in his
ell, sometimes a blueish sometimes a gray-coloured water,
ıich must be caught on the wool in the cup. If the snail
ıly gives out a thick froth, it must be thrown aside and
ıother taken instead.

" The cup being sufficiently filled with liquor is to be
aced on the part affected, always very exactly in the same
uation; it is then to be covered with a white linen cloth,
d apply on it the ball of the truss. This truss, without
ing too tight, must be sufficiently so to prevent the fluid
om escaping between the edge of the cup and the skin in
y posture.

" During this treatment, which will last three or four
onths or more, the patient need not be kept to any par-
:ular regimen. The only precautions necessary, are to
ave the part once every four days, and never to leave the
:rnia long uncovered, in order to avoid cold. To press
ore or less with the hand on the truss whenever the pa-
:nt is going to cough or sneeze, or make any effort what-
er. If the cup rubs off the skin on account of being
dly made, or on account of the hair being suffered to
ow too long, this treatment must be suspended until the
in is well again. In this case the patient will take away
e truss altogether, if it can be done without the intestines
caping through the opening; this will depend on the
ısition in which he is accustomed to lie in his bed. Du-
ıg the day he will wear the truss dry, by filling the conca-
ty with wool and putting a bit of cloth on the hernia.

" By this kind of treatment a common hernia may be
cured

cured in three or, at most, four months, unless something else ails the patient which tends to prevent the aperture from closing. The cure will be found complete when, by applying the finger to the place, we find it closed or almost closed.

"Although the aperture be closed, it will be proper for the patient to continue wearing his truss six weeks or two months longer, because it is indispensably requisite that the wound should be left to close before the muscles which encompass the place are permitted to enjoy their natural action. An unforeseen effort may be productive of more mischief than had occurred heretofore, if the opening be abandoned too early to itself whilst moistened with this antiherniacal liquor." S.

LECTURES.

St. Thomas's and Guy's Hospitals.

The Spring Courses of Lectures at these adjoining hospitals commence the beginning of February, viz.

At St. Thomas's.—Anatomy and the Operations of Surgery, by Mr. Cline and Mr. Cooper. Principles and Practice of Surgery, by Mr. Cooper.

At Guy's,—Practice of Medicine, by Dr. Babington and Dr. Curry.—Chemistry, by Dr. Babington, Dr. Marcet, and Mr. Allen.—Experimental Philosophy, by Mr. Allen.—Theory of Medicine, and Materia Medica, by Dr. Curry and Dr. Cholmeley.—Midwifery, and Diseases of Women and Children, by Dr. Haighton.—Physiology, or Laws of the Animal Œconomy, by Dr. Haighton.—Structure and Diseases of the Teeth, by Mr. Fox.

N. B. These several lectures are so arranged, that no two of them interfere in the hours of attendance; and the whole is calculated to form a Complete Course of Medical and Chirurgical Instruction. Terms and other particulars may be learnt at the respective hospitals.

LIST OF PATENTS FOR NEW INVENTIONS.

To William Cotton of Limehouse, manufacturer, for a new and improved method of regulating the texture of all kinds of cloth in the process of weaving.—Jan. 15, 1810.

To William Murdock of Soho Foundry, in the county of Stafford, engineer, for a process for boring and forming pipes, cylinders, columns, and circular disks out of solid blocks and slabs of stone of any kind or description.—January 15.

METEOROLOGICAL TABLE,

By Mr. Carey, of the Strand,

For January 1810.

Days of the Month.	Thermometer.			Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
	8 o'Clock, Morning.	Noon.	11 o'Clock, Night.			
Dec. 27	43°	39°	38°	29·82	0	Rain
28	35	38	38	30·05	10	Fair
29	43	47	43	29·53	0	Rain
30	43	46	44	·70	15	Cloudy
31	47	50	46	·90	12	Cloudy
Jan. 1	47	49	48	30·10	10	Cloudy
2	47	47	44	·15	0	Small rain
3	44	49	45	·14	15	Fair
4	45	47	44	·29	0	Small rain
5	46	45	42	·30	9	Cloudy
6	43	47	41	·31	10	Cloudy
7	40	43	40	·20	14	Cloudy
8	39	40	40	29·95	12	Cloudy
9	46	47	41	·80	4	Rain
10	40	44	40	·90	10	Fair
11	45	46	43	·85	9	Cloudy
12	43	44	40	·86	10	Cloudy
13	34	34	26	·91	12	Cloudy
14	27	29	24	·92	14	Fair
15	27	28	23	·85	10	Fair
16	20	26	19	·70	0	Snow
17	18	30	26	·96	7	Fair
18	28	34	25	30·20	0	Snow
19	25	29	24	·21	2	Cloudy
20	20	22	29	·05	0	Foggy
21	30	31	30	29·96	7	Fair
22	31	34	34	·90	0	Cloudy
23	34	38	35	30·08	0	Cloudy
24	35	36	35	·19	0	Cloudy
25	35	35	34	·23	4	Cloudy
26	32	35	33	·20	5	Cloudy

N. B. The Barometer's height is taken at one o'clock.

XII. *Description of a new Cupping Instrument.* *By* Robert Healy, *M.B., Dublin.*

No. 1, Clarendon Street,
Dublin.

To Mr. Tilloch.

Sir, I take the liberty of communicating to you a new method of performing the operation of cupping, without the assistance of the syringe, which I have tried, and find to succeed. If you think it worthy of holding a place in your very instructive Magazine, your inserting it will oblige your obedient servant, Robert Healy, M.B.

Description of the Instrument.

The instrument consists of two parts, namely, a hollow vessel A, (Pl. II.) which may contain about half-a-pint of water, made of very thin sheet-copper, or tin, with a stop-cock soldered into it. The end of the stopcock should extend a quarter or half an inch within the vessel A, for a reason that will be assigned. The other part of the instrument is the cupping glass B, made in the usual way, which is to be adapted to the stopcock, with a screw of a coarse thread.—The instrument is to be used in the following manner: the glass is to be unscrewed from A, a little air is to be drawn from the latter by the assistance of the mouth, the cock is then immediately to be turned to prevent the external air from rushing in. Ether or spirits is to be placed in a wine-glass. The mouth of the stopcock is then to be inverted into the liquid so deep, that by turning the cock a sufficient quantity may be drawn up: a drachm of either liquid will be found sufficient. The vessel A is next to be heated to convert the liquid into vapour: as soon as the vapour has filled the vessel, this must be refrigerated in a vessel of cold water, previously turning the cock to prevent the influx of air. A vacuum is produced in proportion to the size of the vessel, and in a very few minutes. The cupping glass is then to be screwed to the stopcock, and placed over the wound made by the lancet or leech. We may adjust the suction by turning the cock, and have a gradual or sudden flow of blood. If the vessel A be of large dimensions, and the patient complains of the suction, we must turn the cock, and either admit the blood to flow into the glass, or unscrew one turn of the ball from the glass, and admit the air to pass through the thread of the screw into the glass. The reason of extending the stop-ck so far within the ball is to prevent the liquid in the ll from passing into the cupping glass when the stop- ck is opened.

November 14, 1809.

III. *On that Power of the Eye, by which it is adjusted to see Objects distinctly at different Distances.* By Ez. Walker, *Esq.*

[Continued from vol. xxix. p. 340.]

FROM observations made on the human eye at different ,es, it appears to undergo a gradual change from infancy old age. For when a child of three or four years old at- ntively views a very small object it is held close to the 'e, not further off than two or three inches. The reason this: in the early part of life the iris, being flexible, has e power of contracting the pupil to a smaller dimension an at any future perriod; consequently a child can see a nall object at a less distance than a grown person: but by :grees this power of the iris decreases, as the rest of the)dy becomes less flexible; and the eye, if a good one, 'adually becomes longer-sighted.

It is generally supposed, however, that the pupil of a iild's eye is larger than that of a grown person's: but this true only when the iris is in a state of relaxation; for it well known, that the pupil always contracts in viewing near object. To suppose that a child can see a small ob- ct at two or three inches distance with a large pupil is)ntrary to the laws of dioptrics, because the rays of light ould then enter the eye in a diverging state, and conse- iently form an indistinct picture of the object upon the tina.

Dr. Jurin observes, that "children read much nearer ian grown persons, for their eyes are smaller, and the ast distance any person can see distinctly at, is propor- onal to the length of the eye*."

But to show that this rule is erroneous, let us suppose iat the nearest distance of distinct vision of an eye is 18 iches; then this eye, according to the Doctor's rule, must : six times as large as the eye of a child that can see di- inctly at the distance of three inches; which is contrary experience. It also appears from the eyes of birds that the

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* Essay on distinct and indistinct Vision, p. 147.

Doctor's rule is erroneous; for it is well known that large birds of prey can see objects at a distance, far beyond the limits of distinct vision of the human eye; and there are small birds that can see minute objects at a very great distance.

Now, as the eyes of birds are much smaller than those of men, it is evident that the utmost distance of distinct vision does not depend upon the length of the axis of the eye.

The nearest distance at which a grown person with a good eye can see distinctly, may in general be about six or seven inches; but this distance increases with age, until near objects appear indistinct.

This increase in the focal distance of the eye is generally supposed to be owing to the humours of the eye becoming too flat through age, so that the rays which fall upon the eye, from an object at a short distance, converge to a point behind the retina, and thus cause indistinct vision. But this theory is, I believe, unsupported by a single clear and decisive experiment.

That the humours of the eye may waste and the eye grow flatter, when every other part of the human frame is upon the decline, may be supposed with some appearance of reason; but that the humours of the eye of a child should decay and the eye grow flatter while every other part of its body is advancing towards perfection, is a theory which cannot be so readily admitted. For, as the eye undergoes a gradual change through life, this effect must be produced in every stage of it, by the same cause. Hence this theory requires further investigation.

II. Those who have written on the properties of the human eye inform us, that "if a good eye views an object at the least distance it can be seen distinctly, and then at twice that distance, and then at an infinite distance, there is about the same alteration made in the figure of the eye between the two last cases, as there is between the two first.

"For let B C D E (Pl. II.) be the axis of the eye infinitely produced; B C, B D, B E, the three distances of the object from the cornea A B; and C A, D A, E A, three rays falling upon any given point of the cornea; whereof E A is parallel to the axis.

"Now to produce distinct vision of the points C, D, E; it is plain that every one of the rays C A, D A, E A, must be successively refracted to the same point F, upon the retina, where it is cut by the eye's axis. At first let us sup-

pose

ɪse the point F to be given, or the length of the axis B F be immutable, and then the quantity of the refraction of ch ray must be varied. And because the distance C D supposed equal to C B, or C A, the angle C A D is equal C D A, and consequently to D A E. Therefore, conriving each ray to come back again from the fixed point to the points C, D, E, successively; the whole quantity its refractions must first be lessened by the angle C A D, d then by the equal angle D A E; and so the changes of e figures of the refracting surfaces must be much the me when the object is removed from C to D, as when it removed from D to E*."

Hence it may be demonstrated, that objects at various stances, as C, D, and E, may be seen distinctly withıt any *alteration in the humours* of the *eye*, or in its *outard form.*

1. It has been proved in a former paper †, that when a od eye views an object at the least distance it can be seen stinctly, the rays which enter the pupil are parallel, or ch as differ very little from being so; whence it is evint, that the rays from the object C falling upon the eye B are parallel.

2. Rays issuing from the object E, a planet or a star, at infinite distance are parallel, consequently the angle A E is infinitely small: and

3. As the angle C D A is equal to the angle D A E, the gle C D A is infinitely small; therefore the line D A cocides with the line D B, and the rays falling upon the e from D must also be parallel; consequently the rays ɔm the object D, and also those from E and C, are rected to the same point F upon the retina: for parallel ys falling upon the eye near the axis of vision have the me focus, whether they come from objects that are near remote. Whence it is evident that vision perfectly dinct is produced only by parallel rays; but the means by nich the eye admits such rays only as are parallel, or arly so, and rejects the rest, come next to be considered.

III.—If a small circular object be viewed with too large pupil, which may be done by placing the object much thin the limits of distinct vision, it will appear larger an perfect vision would represent it, by a penumbra of ;ht, which is called the *circle of dissipation.*

Now if the pupil be contracted, by means of a perforation made in a card of such magnitude as to prevent the lateral rays from entering the eye, the object will appear distinct. But if the aperture in the card be made as large as the pupil, the *circle of dissipation* will appear as large as before.

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* Smith's Optics, Remarks, p. 2.

† Philosophical Magazine, vol. xxix. p. 342.

This circle of dissipation is formed by those rays which enter the eye remote from the axis of the crystalline lens. Thus, when the pupil is too large for distinct vision, the most refrangible of those side rays will cross one another in the vitreous humour, and, by falling upon the retina in a diverging state, will be dispersed over a larger space than the true image, and consequently form a penumbra round it; and the least refrangible rays of the same pencil will be dispersed over the interior parts of the circle; whence that indistinctness of vision which is experienced by people advanced in years.

No writer has paid more attention to the theory of distinct and indistinct vision than Dr. Jurin. This philosopher says, that "the radius of dissipation is, *cæteris paribus,* always proportional to the radius of the pupil. Consequently, when the pupil is narrow, the *radius of dissipation* and the *penumbra* arising from the dissipation will be smaller, that is, vision will be rendered either distinct, or at least less indistinct than it would otherwise be [*]."

IV.—Many philosophers have maintained that we have the power of viewing objects at different distances, by a conformation of the eye for this purpose, independent of a variation in the pupil; but they vary much in their opinions respecting the means by which this effect is produced.

Dr. Matthew Young says, that "the power of seeing distinctly at different distances does not depend on the crystalline.

"This is evident," he says, "from the experiments made on a person who had been couched for a cataract, and by the assistance of the same convex lens, applied to that eye, could see distinctly at different distances [†]."

But Dr, Porterfield is of opinion, that the change made in the eye must be in the crystalline; for a person who had been couched of a cataract was under the necessity of using glasses of different degrees of convexity, for seeing objects distinctly at different distances [‡].

These two alleged facts may both be true, but the conusion drawn by Dr. Porterfield seems to be erroneous lthough it has been used, not only by himself, but by her writers, as an unanswerable argument to prove that e eye is adjusted by means of the crystalline lens,) ; r if the iris of the person's eye mentioned by Dr. Portereld received any injury from the operation of couching, e adjustment of that eye would be imperfect, and require lasses of different degrees of convexity for viewing objects different distances ; as it is well known, that persons who ad with spectacles of very convex lenses require glasses f a less degree of convexity to view remote objects ; still e less convex, the more remote the object.

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[*] Jurin's Essay on distinct and indistinct Vision, p. 145.

[†] Dr. M. Young's Analysis of Nat. Phil. p. 375.

[‡] See Porterfield on the Eye, vol. i. p. 434, 435.

But if the operation mentioned by Dr. Young was perrmed without doing any injury to the iris, that person ould still be able to see objects distinctly at different diances, by the assistance of the same convex lens applied that eye ; because the iris is the only organ by which the e is adjusted to distinct vision. It is a physical truth, hich any one may convince himself of by trial, that no ental exertion can change the adjustment of the eye, hen every part of the iris is covered. This may be tried viewing objects through a hole in a card, made rather ss than the pupil in its most contracted state.

V.—Sir Isaac Newton was of opinion, that the humours the eye decay or shrink by old age, which causes the eye the short-sighted to grow flatter till it comes to a due ure. For short-sighted men see remote objects best in d age[*]. And this opinion has been adopted by some of r best writers on the theory of vision. But let opinions ve place to facts.

Mr. Adams says, "It is generally supposed, that the ort-sighted become less so as they advance in years, as e natural shrinking and decay in the humours of the eye ssen its convexity, and thus adapt it better for viewing stant objects: but among the great number of shortghted that I have accommodated with glasses, I have ever und the reverse of this theory to be true, and the eyes of e myopes never required glasses less concave, but genelly more concave, as they grew older, to enable them to e at the same distance[†]."

Hence it is evident, that the humours of the eyes of the ort-sighted undergo no change as they grow older, and at vision generally becomes less perfect. For, as the iris ses some part of its contracting power with age, the pupil becomes larger, the *circle of dissipation* increases, and consequently remote objects appear less distinct than they did in the early part of life.

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[*] See Newton's Optics, p. 13.

[†] Adams on Vision, p. 126.

VI.—From this investigation it manifestly appears, that the eye is in reality no more than a machine of a fixed and determinate form, without any power to alter its outward dimensions, or to move any of its internal humours; and that the only adjustment necessary to form a distinct picture of an object upon the retina (whether the object be near or remote) is to prevent diverging rays from entering the eye, and to admit such only as are nearly parallel.

This office is performed by the iris, which contracts the pupil to exclude the side rays when we view near objects, and enlarges this aperture to give us a distinct view of such remote objects as are but faintly illuminated: it is also well known, that whenever the eye is exposed to a strong light, the pupil contracts, but it expands as the light decreases.

These contractions and dilatations of the pupil, according to the distances of objects and strength of light in which they are seen, are directed by that volition of the mind which presides over and regulates all the other motions of the eye.

EZ. WALKER.

Lynn, January 3, 1810,

XIV. *On a native Arseniate of Lead. By the Reverend* WILLIAM GREGOR *Communicated by* CHARLES HATCHET, *Esq., F.R.S.*[*]

I.

THAT the oxide of lead and the arsenic acid might be found in the state of natural combination, is a supposition highly probable, from the strong affinity which subsists between these two substances. But the existence of such a compound has not, as I conceive, hitherto been established by such proofs, as entitle it to be ranked amongst the decided cases of mineralogical science. I trust, therefore, that the observations, which I have the honour of submitting to the Society, on a new[†] ore of lead lately discovered in the county of Cornwall, so justly celebrated as well for the variety as for the richness of its mineral productions, will not be deemed superfluous.

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[*] From Philosophical Transactions for 1809, Part II.

[†] It is new at least to the miners in Cornwall; nor was there, previously to this discovery, any ore resembling it to be found in that splendid collection of minerals, which my valuable friend Philip Rashleigh, esq., has so liberally formed, and as liberally employed in the promotion of science.

This mineral was raised in the mine called Huel-Unity, a very rich copper mine, in the parish of Gwennap. According to the information with which I have been favoured by Mr. William Davy, a very intelligent and experienced miner in that district, it was found in a lode south of Huel-Unity principal lode, at the depth of fifty fathoms below the surface, which lode underlay about two feet in the fathom south: at the depth above mentioned, this lode fell in or formed a junction with another small lode or vein to the south, and when the junction took place this lead ore was found. The veins of it are, in general, from six to ten inches wide, and they diverge on going west. Some particles of this lead ore have been found in the southern part, after the separation of the lodes; but the northern lode does not contain any until the junction takes place. This ore is intermixed with some native copper, very rich gray copper, and black copper ore, and some is mixed with quartz. The walls of both veins are killas.

II. *Description.*

This mineral is regularly crystallized. The form of its most perfect crystals is an hexahedral prism; they are of different sizes, from one-tenth of an inch in diameter to the size of a hair. The longest which I have seen do not exceed three-tenths of an inch in length: these terminate in a plane, at right angles, with the axis of the prism; but the crystals of a smaller size are frequently drawn out into a very taper acumination, which appears to be a six-sided pyramid. A number of smaller crystals are often closely packed together in bundles, which are bent in different directions, and terminate in a point. The larger crystals either stand alone, or adhere, on their lateral planes, to the gangue, or are confusedly matted together in a mass.

Some of them are hollow, as if an internal nucleus had been destroyed; and sometimes this internal nucleus overtops the external laminæ. The gangue is a white quartz, which frequently exhibits on its surface the appearance of a partial decomposition.

The red octahedral copper ore, and the copper into which that ore passes, are often intermingled with the crystals of this lead ore and imbedded in them.

The colour of these crystals consists of a variety of tints of yellow. Some are of a beautiful wine yellow resembling the Brazilian topaz: this, in the greater number of specimens, passes into a delicate Isabella-colour: whilst, in other cases, we have the honey-yellow mingled with brown hues of different intensities: so that we meet with crystals resembling dark brown sugar-candy, or common resin.

Some of the crystals are beautifully transparent, whilst others possess this quality in part only, at their extremities, or in inferior degrees throughout their whole lengths.

The external lustre, in some specimens, is vitreous; in others, resinous: but in some instances their surface is partially covered by tender and delicate filaments of a silky lustre. These filaments are sometimes found in a separate state loosely adhering to quartz; and they form a variety of this fossil.

The crystals vary as to hardness. The angular fragments of the most transparent are sufficiently hard to scratch glass.

This mineral is easily reduced to powder, which has the appearance of pounded resin; it contracts a yellower tint by long exposure to the air.

The specific gravity of the purest crystals, taken at the temperature of 50° Fahrenheit, was 6·41.

III.

A fragment of crystal, exposed to the flame of the blowpipe in a gold spoon, melted into a brownish-yellow mass, which on cooling did not assume any angular figure. It remained in a state of ignition apparently unaltered; but when a piece of it was exposed to the flame on charcoal, a rapid decomposition took place, arsenical vapours were extricated, and globules of a metal, possessing the common properties of lead, were left behind.

This mineral, in a state of fine powder, is soluble in nitric acid, even without the aid of heat. Care, however, must be taken, that it does not concrete into lumps. The vessel therefore which contains it must be frequently shaken, and the nitrate of lead produced must be, from time to time, dissolved in water, and poured off from the residuum. The process of solution is, however, accelerated by a digesting heat. Some silica remains, which, as the quantity of it is variable according to circumstances, appears not to be an essential ingredient of this fossil.

The nitric solution is colourless; its transparency is not disturbed by nitrate of barytes. Nitrate of silver renders it turbid, and a small quantity of white curdly matter is deposited. Sulphuric acid and the liquid sulphates produce copious precipitates of a white heavy matter. If the fluid poured off from this subsided matter, and it be freed ım the superfluous sulphuric acid, by the means of nitrate barytes, it will yield, on the affusion of liquid nitrate of .d, an abundant white precipitate, which, urged by the me of the blow-pipe on a support of charcoal, resolves elf into reduced lead and arsenical vapours.

These preliminary experiments led me to the probable nclusion, that this fossil chiefly consisted of oxide of lead, enic acid, and a small quantity of the muriatic acid.

IV. *Analysis.*

A.

1. Fifty grains, carefully selected from crystals of a pale ıbella-colour, were reduced to a fine powder, and exposed a low red heat for about an hour. Their weight was dinished by 0·15 of a grain.

2. The yellowish powder was now transferred to a vessel pure silver, and mixed with a lixivium containing fifty ıins of potash, prepared by the means of alcohol; a quan-/, which I had previously ascertained to be sufficient to :ct a complete decomposition of this mineral. The ley s gradually evaporated to dryness in a sand-bath. The uble part was extracted by distilled water, and poured off m a yellowish white matter, which was sufficiently edul-·ated (*a*).

3. Liquid nitrate of ammonia was now dropped into the aline fluid, as long as it produced any cloudiness: the ar fluid was now decanted from a small quantity of white .tter, which had subsided, and rendered acid by nitric d; ammonia, added to excess, produced a slight turbid-ıs. These precipitates, after sufficient edulcoration, were led to the yellowish white residuum (*a*).

ɫ. The liquid was now rendered slightly acid by nitric d, and a solution of nitrate* of lead in distilled water s dropped into it, as long as it separated any precipitate. e clear fluid was poured off, and evaporated nearly to 'ness, and a small quantity of white matter, thus ob-ıed, was added to the former precipitate, which dried,

and

If the colourless liquid oxy-nitrate of lead be dropped into a dilute so-ɔn of arsenic acid, or of arseniate of potash acidulated by nitric acid, no ıediate precipitation of an arseniate of lead is produced; but crystalline ns are, after a time, gradually deposited at the bottom of the vessel. But id nitrate of lead causes an immediate and abundant precipitate from e same dilute solutions. These two combinations therefore must be :rent.

and exposed to a low red heat, weighed, whilst still warm, 40·8, which, according to the proportion of 33 : 100, established by Mr. Chenevix, implies 13·46 of arsenic acid.

5. The superfluous lead was now separated from the fluid by sulphate of soda, and filtered off. Ammonia precipitated a minute portion of flaky matter; it weighed, after ignition, 0·2 of a grain; it consisted of silica and oxide of lead, and must be attributed to the nitrate of lead employed.

B.

1. The yellowish white residuum (*a*) (A, § 2.) was dissolved without effervescence in nitric acid, except a minute portion of silica, which, after ignition, = 0·28. A white heavy matter was thrown down from this solution, by liquid sulphate of soda. The clear decanted fluid was evaporated to a small volume, and sulphate of soda produced a further separation of white matter; it was sulphate of lead, which, after exposure to a low red heat, and weighed, whilst warm, = 47·5, which, upon the supposition that one hundred parts of sulphate of lead contain 69·74 of lead + 3·48 of oxygen, is equivalent to 34·77 of oxide of lead.

2. The fluid, now freed from lead, deposited, on the affusion of ammonia, a greenish matter, which, after ignition, became red, and = 0·033 of a grain. It was oxide of iron.

C.

1. One hundred grains of larger crystals, some of which were hollow, and the surfaces of which were slightly and partially covered with silky filaments, treated in the same way yielded 95·283 of sulphate of lead, equivalent to 69·76 of oxide, and 80 of arseniate of lead, which indicates 26·40 of arsenic acid. The oxide of iron, in this case, amounted to only ·05 of a grain, and the residuary silica was in too small a quantity to be weighed.

2. I have endeavoured to decompose this fossil by boiling it to dryness in a solution of four times its weight of the purest subcarbonate of potash, and exposing the dry mass, for a very short time, to a low red heat; but I found, that only a part of the arsenic acid had united to the alkali; the larger portion of it was detected in the nitric solution of the residuum; but the relative proportions of the oxide and the acid, were found to correspond almost exactly with the foregoing statement of them.

3. I found also, that carbonate of ammonia precipitated this mineral, in an unaltered state, from its solution in nitric

ric acid : as no arsenic acid had united with the precipi-
ıt. The solution of the nitrate of ammonia was evapo-
ed to dryness, and exposed to a red heat in a platina
ıcible ; but nothing was left, except a slight trace of
ıde of lead. We may infer from hence, the absence of
ıh the fixed alkalies.

4. I found in one specimen only of this fossil any no-
ıle difference in the relative proportions of the oxide of
d and of the acid to which it is united. It consisted of
'stals confusedly matted together in a more compact mass
ın this fossil generally assumes. One hundred grains
re dissolved in nitric acid ; the marine acid was separated
nitrate of silver, and any redundant silver by muriate of
ımonia. The lead was separated by sulphuric acid, and
: superfluous portion of that acid by nitrate of barytes,
d the arsenic acid was combined with the oxide of lead
the affusion of nitrate of lead. The muriate of silver
9·8 ; the sulphate of lead = 97·6, and the arseniate of
d = 72, equivalent to 1·63 of muriatic acid, 71·46 of
de of lead, and 23·88 of arsenic acid, respectively. The
ıartz = 0·35, and the oxide of iron 0·2, nearly.

Another portion taken from the same specimen, treated
:h an alkali, gave very nearly a similar result.

D.

It will now be necessary for me to speak concerning an
;redient of this fossil, which I may have seemed to over-
ık. I mean the muriatic acid : I have found some diffi-
ty in ascertaining the proportion which it bears to the
ıer constituent parts, and from a cause which I did not
ıpect. I considered that the only sure mode of deter-
ning this point, was to have recourse to nitrate of silver,
ıich might effect a direct separation of the marine acid
m the nitric solution of this fossil. But I found, in
ıny experiments upon given quantities of this mineral,
ıt the results, which I derived from this most valuable
ımical test, were variable and uncertain.

At last, I was enabled to trace the error and uncertainty
to two sources. In the first place, I found that the mu-
te of silver was more abundant in the cases where I
ployed a vessel with a long neck for the solution, and
not expose it to heat.

I concluded, therefore, that when the process was con-
:ted under different circumstances, the predominating
mass

mass of nitric acid produced its effect, and volatilized a portion of the muriatic.

Another source of error I found in the following anomalous circumstance, viz. a simultaneous precipitation of a portion of arseniate of lead takes place with that of the muriate of silver. Whatever combination this may be, it is a weak one, and may be severed by nitric acid, which dissolves the arseniate and leaves the muriate; or by ammonia, which takes up the muriate, to the exclusion of the arseniate.

The conclusion to which many experiments have led me is this, that the muriate of silver produced in the nitric solution of one hundred grains of arseniate of lead by nitrate of silver, amounts to about 9·5.

E.

In order to prove that the acid, which is combined with the oxide of lead in this mineral, is the arsenic acid, and that it is not combined with phosphoric, I decomposed some of its acid, which had been combined with lead in the foregoing experiments, by means of sulphuric acid, and filtered off the sulphate of lead. The fluid which passed through the filter was evaporated nearly to dryness, and it assumed the appearance of crystalline grains. Some of it was exposed to the flame of the blow-pipe in a gold spoon; at first it became like a white dry powder, which melted before an increased heat: placed on charcoal and ignited, it was totally dissipated in arsenical fumes.

Some of it was dissolved in water, and, dropped into liquid sulphate of titanium, a white precipitate was produced: combined with soda, it precipitated silver from the nitrate of silver, of a brick colour. It precipitated mercury from its nitrate, of a yellowish colour, which afterwards became reddish. This precipitate, exposed to the flame of the blow-pipe on charcoal, exhibited the same phænomena as arseniate of mercury.

I precipitated magnesia from its muriate, and redissolved it by carbonate of ammonia, perfectly saturated with carbonic acid. I divided this liquid into two portions, and dropped into both a solution of the combination of the acid of this mineral and soda. No precipitate was produced. I dropped into one of the vessels some liquid phosphate of soda, and a separation of saline matter was instantly produced. I soon, however, found, that this mode of distinguishing the phosphoric from the arsenic acid could not be depended upon. For in the other vessel, in which no phosphate of soda had been dropped, in a short time, saline fts made their appearance, and an abundant deposition of line matter was formed. I found also, that if the solu- n had been more concentrated, the precipitation would ve immediately taken place.

On making a comparative experiment with arsenic acid, ound that it forms a triple salt with ammonia and mag- sia, analogous to the phosphoric salt described by Dr. ollaston. The figure of the arsenical salt, as far as I could termine it from a confused crystallisation, is a trihedral sm.

We are therefore, I think, authorized from the experi- nts herein detailed, to conclude, that the fossil which is subject of this paper is an arseniate of lead, and that, we state that the relative proportion of the constituent rts of it is in one hundred, as follows, we shall not be from the truth:

Oxide of lead -	69·76
Arsenic acid -	26·40
Muriatic acid -	1·58

The silica and the oxide of iron, which account for a rtion of the loss, and the alumina and copper which are netimes found in an analysis of this fossil, I do not con- ve to be essential to it.

The existence of a minute portion of muriatic acid as a nstant ingredient of it, is a curious fact: and it is still re curious, when we consider it in connexion with the alogy that, in this particular, it maintains with the natural osphates of lead.

/. Description of a reflective Goniometer. By William Hyde Wollaston, M.D., Sec. R.S.*

ROM the advances that have been made of late years in stallography, a very large proportion of mineral sub- nces may now be recognized, if we can ascertain the gular dimensions of their external forms, or the relative sition of those surfaces that are exposed by fracture. But ugh the modifications of tetrahedrons, of cubes, and of se other regular solids, to which the adventitious aid of metry could be correctly applied, have been determined h the utmost precision, yet it has been often a subject regret, that our instruments for measuring the angles of stals are not possessed of equal accuracy, and that in lying the goniometer to small crystals, where the radius in contact with the surface is necessarily very short, the measures, even when taken with a steady hand, will often deviate too much from the truth to aid us in determining the species to which a substance belongs.

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* From Philosophical Transactions for 1809, Part II.

A means of remedying this defect has lately occurred to me, by which in most cases the inclination of surfaces may be measured as exactly as is wanted for common purposes; and when the surfaces are sufficiently smooth to reflect a distinct image of distant objects, the position of faces only $\frac{1}{50}$th of an inch in breadth may be determined with as much precision as those of any larger crystals.

For this purpose, the ray of light reflected from the surface is employed as radius, instead of the surface itself, and accordingly for a radius of $\frac{1}{50}$th of an inch, we may substitute either the distance of the eye from the crystal, which would naturally be about twelve or fifteen inches; or for greater accuracy we may, by a second mode, substitute the distance of objects seen at a hundred or more yards from us.

The instrument which I use, consists of a circle graduated on its edge, and mounted on a horizontal axle, supported by an upright pillar (Plate II). This axle being perforated, admits the passage of a smaller axle through it, to which any crystal of moderate size may be attached by a piece of wax, with its edge, or intersection of the surfaces, horizontal and parallel to the axis of motion.

This position of the crystal is first adjusted, so that by turning the smaller axle, each of the two surfaces, whose inclination is to be measured, will reflect the same light to the eye.

The circle is then set to zero, or 180°, by an index attached to the pillar that supports it.

The small axle is then turned till the further surface reflects the light of a candle, or other definite object, to the eye; and lastly, (the eye being kept steadily in the same place) the circle is turned by its larger axle, till the second surface reflects the same light. This second surface is thus ascertained to be in the same position as the former surface had been. The angle through which the circle has moved, is in fact the supplement to the inclination of the surfaces; but as the graduations on its margin are numbered accordingly in an inverted order, the angle is correctly shown by the index, without need of any computation.

It may here be observed, that it is by no means necessary to have a clean uniform fracture for this application of the instrument to the structure of laminated substances; for since

ince all those small portions of a shattered surface, that re parallel to one another (though not in the same plane), glisten at once with the same light, the angle of an irregu-ar fracture may be determined nearly as well, as when the eflecting fragments are actually in the same plane.

In this method of taking the measure of an angle, when he eye and candle are only ten or twelve inches distant, a mall error may arise from parallax, if the intersection of he planes or edge of the crystal be not accurately in a line vith the axis of motion*; but such an error may be rendered insensible, even in that mode of using the instrument, by due care in placing the crystal; and when the urfaces are sufficiently smooth to reflect a distinct image f objects, all error from the same source may be entirely bviated by another method of using it.

For this purpose, if the eye be brought within about an nch of the reflecting surface, the reflected image of some istant chimney may be seen inverted beneath its true place, nd by turning the small axle may be brought to correspond pparently with the bottom of the house (or with some ther distant horizontal line). In this position the surface ccurately bisects the angle, which the height of that house ubtends at the eye (or rather at the reflecting surface); then, y turning the whole circle and crystal together, the other urface, however small, may be brought exactly into the ame position; and the angle of the surfaces may thus be measured, with a degree of precision which has not hitherto een expected in goniometry.

The accuracy, indeed, of this instrument is such, that a ircle of moderate dimensions, with a vernier adapted to it, vill probably afford corrections to many former observa-ons. I have already remarked one instance of a mistake hat prevails respecting the common carbonate of lime, nd I am induced to mention it, because this substance is ery likely to be employed as a test of the correctness of uch a goniometer, by any one who is not convinced of its ccuracy from a distinct conception of the principles of its onstruction.

The inclination of the surfaces of a primitive crystal of arbonate of lime is stated, with great appearance of precision, to be 104° 28′ 40″: a result deduced from the supposed position of its axis at an angle of 45° with each of the surfaces, and from other seducing circumstances of apparent harmony by simple ratios. But however strong the presumption might be that this angle, which by measurement approaches to 45°, is actually so, it must nevertheless be in fact about 45° 20′; for I find the inclination of the surfaces to each other is very nearly, if not accurately, 105°, as it was formerly determined to be by Huygens[*]; and since the measure of the superficial angle given by Sir Isaac Newton[†] corresponds with this determination of Huygens, his evidence may be considered as a further confirmation of the same result; for it may be presumed, that he would not adopt the measures of others, without a careful examination.

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* I cannot omit mentioning, that Mr. Sowerby had thought of employing eflection for this purpose, nearly at the same time as myself; but did not ucceed to his satisfaction, in consequence of an attempt to fix the position the eye. For when the line of sight is determined by a point connected ith the apparatus, the radius employed is thereby limited to the extent of e instrument, and the error from parallax is manifestly increased.

IN THE ANNEXED PLATE,

a b. Is the principal circle of the goniometer graduated on its edge.

c c. The axle of the circle.

d. A milled head by which the circle is turned.

e e. The small axle for turning the crystal, without moving the circle.

f. A milled head on the small axle.

g. A brass plate supported by the pillar, and graduated as a vernier to every five minutes.

h. The extremity of a small spring, by which the circle is stopped at 180°, without the trouble of reading off.

i i and *k k*. Are two centres of motion, the one horizontal, the other vertical for adjusting the position of a crystal: one turned by the handle *l*, the other by the milled head *m*.

The crystal being attached to a screw-head at the point *n* (in the centre of all the motions), with one of its surfaces as nearly parallel as may be to the milled head *m*, is next rendered truly parallel to the axis by turning the handle *l* till the reflected image of a horizontal line is seen to be horizontal.

By means of the milled head *f*, the second surface is then brought into the position of the first; and if the reflected image from this surface is found not to be horizontal, it is rendered so by turning the milled head *m*; and since this motion is parallel to the first surface, it does not derange the preceding adjustment.

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[*] Huygenii *Opera Reliqua*, tom. i. p. 73.—*Tract. de Lumine.*

[†] Newton's Optics, 8vo. p. 329. Qu. 25, concerning Iceland Crystal.

XVI. *Che-*

XVI. *Chemical Analysis of a Black Sand, from the River Dee, in Aberdeenshire. By* THOMAS THOMSON, *M.D., Lecturer on Chemistry, Edinburgh**.

THE specimen which formed the subject of the first of the following analyses was brought from the banks of the river Dorr, about seven years ago, by my friend Mr. James Mill, who at that time resided in Aberdeenshire. By him I was informed that considerable quantities of it are found n different parts of the bed of that river,—that it is called by the inhabitants *iron-sand,*—and that they use it for sanding newly written paper. I tried some experiments in the year 1800, in order to ascertain its nature; but was too little skilled at that time, both in mineralogy and practical chemistry, to manage an analysis of any considerable difficulty.

The black powder is mixed with a good many small whitish, reddish, and brownish grains, which, when examined by means of a glass, prove to be pieces of quartz, felspar, and mica. From this it would appear, that the sand of the river Dee consists chiefly of the detritus of granite or gneiss.

When a magnet is passed over the sand, some of the black grains adhere to it, and are by this means easily obtained separate. But after all that can be attracted by the magnet is removed, the greater part of the black powder still remains. This residue is indeed attracted by a powerful magnet, but so very feebly, that it is not possible by means of it to separate it from the grains of sand with which it is mixed. Thus we learn, that the black matter consists of two distinct substances; one of which is powerfully attracted by the magnet, the other not. As this second substance was obviously specifically heavier than the grains of sand with which it was mixed, I placed a quantity of the powder on an inclined plane, and by exposing it cautiously, and repeatedly, to a jet of water, I succeeded in washing away most of the grains of sand, and thus obtained it in a state of tolerable purity.

The first of these minerals we may call *iron-sand,* and the second *iserine,* as they belong to mineral species which oryctognosts have distinguished by these names.

I. *Iron-Sand.*

The iron-sand is much smaller in quantity than the isetine, and does not exceed one-fourth of the mixture at most. Its colour is iron-black. It is in very small angular grains, commonly pretty sharp-edged, and sometimes having the shape of imperfect octahedrons. The surface is rough; the lustre is feebly glimmering and metallic; the fracture, from the smallness of the grains, could not be accurately ascertained, but it seemed to be conchoidal. Opake, semi-hard, brittle, easily reduced to powder. Powder has a grayish-black colour; powerfully attracted by the magnet; specific gravity 4·765.

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* From Transactions of Royal Society, Edinburgh, 1807.

1. As acids were not found to act upon this mineral, 100 grains of it were reduced to a fine powder, mixed with twice its weight of carbonate of potash, and exposed for two hours to a red heat in a porcelain crucible. The mass, being softened in water, was digested in muriatic acid. By repeating this process twice, the whole was dissolved in muriatic acid, except a brownish-white matter, which being dried in the open air weighed 19½ grains.

2. The muriatic acid solution, which had a deep yellowish-brown colour, was concentrated almost to dryness, and then diluted with water. It assumed a milky appearance; but nothing was precipitated. Being boiled for some time, and then set aside, a curdy-like matter fell. It was of a milk-white colour, weighed, when dry, seven grains, and possessed the properties of oxide of titanium.

3. The residual liquid being supersaturated with ammonia, a dark reddish-brown matter precipitated, which being separated by the filter, dried, drenched in oil, and heated to redness, assumed the appearance of a black matter, strongly attracted by the magnet. It weighed 93·7 grains, and was oxide of iron.

4. The 19·5 grains of residual powder, being mixed with four times its weight of carbonate of soda, and exposed for two hours to a red heat, in a platinum crucible, and afterwards heated with muriatic acid, was all dissolved, except about a grain of blackish matter, which was set aside.

5. The muriatic solution being concentrated by evaporation, a little white matter was separated. It weighed one-fourth of a grain, and possessed the characters of oxide of titanium.

6. When evaporated to dryness, and redissolved in water, a white powder remained, which proved to be silica, and which, after being heated to redness, weighed one grain.

7. The watery solution being supersaturated with potash, and boiled for a few minutes, was thrown upon a filter, to

parate a reddish-brown matter, which had been precipi-
ted. The clear liquid which passed through the filter
as mixed with a solution of sal ammoniac. A soft white
atter slowly subsided. It was alumina, and, after being
eated to redness, weighed half a grain.

8. The brown-coloured matter which had been precipi-
ted by the potash, when dried upon the steam-bath,
eighed 20·2 grains. It dissolved with effervescence in
uriatic acid. The solution had the appearance of the yolk
f an egg. When boiled for some time, and then diluted
ith water, it became white, and let fall a curdy precipi-
te, which weighed, when dry, 4·6 grains, and possessed
e properties of oxide of titanium.

9. The residual liquor being mixed with an excess of am-
onia, let fall a brown matter, which, after being dried,
enched in oil, and heated to redness, weighed six grains.
was strongly attracted by the magnet, but was of too
ght a colour to be pure oxide of iron. I therefore dis-
lved it in muriatic acid, and placed it on the sand-bath,
a porcelain capsule. When very much concentrated by
aporation, small white needles began to make their ap-
earance in it. The addition of hot-water made them dis-
pear; but they were again formed when the liquor be-
me sufficiently concentrated. These crystals, when se-
rated, weighed 1·3 grains, and proved, on examination,
be white oxide of arsenic. During the solution of the
x grains in muriatic acid, a portion of black matter sepa-
ted. It weighed 0·2 grains, and was totally dissipated
efore the blow-pipe in a white smoke. Hence, it must
ave been arsenic. These 1·5 grains are equivalant to ra-
er more than one grain of metallic arsenic. Thus, it ap-
ears, that the six grains contained one grain of arsenic,
hich explains the whiteness of their colour. The rest was
on. It can scarcely be doubted, that the proportion of
rsenic present was originally greater. Some of it must
ave been driven off when the iron oxide was heated with
il.

10. The insoluble residue (No. 4.) was with great diffi-
ulty dissolved in sulphuric acid. When the solution was
ixed with ammonia, a white powder fell, which weighed
·8 grains. It was accidentally lost, before I examined its
roperties. But I have no doubt, from its appearance, that
was oxide of titanium.

11. Thus, from the 100 grains of iron-sand, the follow-
g constituents have been extracted by analysis:

Black

Black oxide of iron, -	98·70
White oxide of titanium,	12·65
Arsenic, - - -	1·00
Silica and alumina, - -	1·50
Total,	113·85

Here there is an excess of nearly 14 grains, owing, without doubt, to the combination of oxygen with the iron and the titanium during the analysis.

Had the iron in the ore been in the metallic state, the excess of weight, instead of 14, could not have been less than 30. For the black oxide is known to be a compound of 100 metal and 37 oxygen. Hence, I think, it follows, that the iron in our ore must have been in the state of an oxide, and that it must have contained less oxygen than black oxide of iron. A good many trials, both on iron-sand, and on some of the other magnetic ores of iron, induce me to conclude, that the iron in most of them is combined with between 17 and 18 per cent. of oxygen. This compound, hitherto almost overlooked by chemists, I consider as the real protoxide of iron. Thenard has lately demonstrated the existence of an oxide intermediate between the black and the red; so that we are now acquainted with four oxides of this metal. But the protoxide, I presume, does not combine with acids like the others. Analogy leads us to presume the existence of a fifth oxide, between the green and the red.

As to the titanium, it is impossible to know what increase of weight it has sustained, because we are neither acquainted with it in the metallic state, nor know how much oxygen its different oxides contain. It is highly improbable, that, in iron-sand, the titanium is in the metallic state, if it be made out that the iron is in that of an oxide. The experiments of Vauquelin and Hecht, compared with those of Klaproth, have taught us that there are three oxides of titanium, namely, the blue, the red, and the white. From an experiment of Vauquelin and Hecht, and from some of my own, I am disposed to consider these oxides as composed of the following proportions of metal and oxygen:

	Metal.	Oxygen.
1. Blue,	100	16
2. Red,	100	33
3. White,	100	49

I find, that when the white oxide of titanium is reduced to

ıe state of red oxide, it loses one-fourth of its weight;
ıd that red oxide, when raised to the state of white oxide,
ıcreases exactly one-third of its weight. It was the know-
dge of these facts, that led me to the preceding numbers,
nd I think they may be used, till some more direct expe-
ment lead us to precise conclusions.

Red oxide being the only state in which this metal has
t occurred separate, we may conclude that it combines,
this state, with metallic oxides, and that the titanium in
on-sand is most probably in this state. But white oxide,
minished by one-fourth, gives us the equivalent quantity
f red oxide. On that supposition, the titanium present,
fore the analysis, in the 100 grains of ore, weighed 9·5
rains.

The appearance of the arsenic surprised me a good deal,
it was altogether unexpected. I am disposed to ascribe
to some particles of arsenic pyrites which might have
en accidentally present. This conjecture will appear the
ore probable, when we reflect, that arsenic pyrites very
equently accompanies iron-sand. Before the microscope,
ıe iron-sand appears to contain some white shining par-
cles, which, probably, are arsenic pyrites.

The small quantity of silica and alumina, I ascribe,
ithout hesitation, to grains of quartz and felspar, which
d adhered to the iron-sand, and been analysed along with
. Some such grains were actually observed and separated.
ut others, probably, escaped detection.

12. If these suppositions be admitted as well founded,
e iron-sand was composed of

Protoxide of iron,	85·3
Red oxide of titanium,	9·5
Arsenic, -	1·0
Silica and alumina,	1·5
Loss, - -	2·7
	100·0

The loss will not appear excessive, if we consider, that a
rtion of the arsenic must have been sublimed, before the
esence of that metal was suspected.

Upon the whole, I think we may consider the specimen
iron-sand examined, as composed of nine parts prot-
ide of iron, and one of red oxide of titanium. The pre-
nce of titanium in this ore had been already detected by
mpadius, though, as I have not seen his analysis, I
nnot say in what proportion.

II. *Iserine.*

II. *Iserine.*

The colour of this ore is iron-black, with a shade of brown. It consists of small angular grains, rather larger than those of the iron-sand, but very similar to them in their appearance. Their edges are blunt; they are smoother, and have a stronger glimmering lustre than those of the iron-sand. Lustre semi-metallic, inclining to metallic. The fracture could not be distinctly observed, but it seemed to be conchoidal; at least nothing resembling a foliated fracture could be perceived. Opake, semi-hard, brittle, easily reduced to powder; colour of the powder unaltered; specific gravity 4·491*; scarcely attracted by the magnet.

1. A hundred grains of the powdered ore were mixed with six times their weight of carbonate of soda, and exposed for two hours to a red heat, in a platinum crucible. The mass obtained, being softened with water, dissolved completely in muriatic acid. When the solution was concentrated, it assumed the appearance of the yolk of an egg. It was boiled, diluted with water, and set aside for some time. A white matter gradually deposited, which, when dried on the steam-bath, weighed 53 grains, and possessed the properties of oxide of titanium.

2. The liquid thus freed from titanium was evaporated to dryness, and the residue redissolved in water, acidulated with muriatic acid. A white powder remained, which, after being heated to redness, weighed 16·8 grains, and possessed the properties of silica.

3. The solution was precipitated by ammonia, and the brown matter which had separated, boiled for some time in liquid potash. The whole was then thrown on a filter, to separate the undissolved part, and the liquid which came through was mixed with a solution of sal-ammoniac. A white powder fell, which, after being heated to redness, weighed 3·2 grains. It was alumina.

4. The brown substance collected on the filter was dried, drenched in oil, and heated to redness. It was strongly attracted by the magnet, and weighed 52 grains.

5. It was digested in diluted sulphuric acid; but not being rapidly acted upon, a quantity of muriatic acid was added, and the digestion continued. The whole slowly dissolved, except a blackish matter, which became white when exposed to a red heat, and, as far as I could judge

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* If, as the following analysis would lead us to expect, the specimen examined was a mixture of four parts iserine, and one part quartz and felspar, the specific gravity of pure iserine should be 4·964.

m its properties, was oxide of titanium, slightly con-
ninated with iron. It weighed 1·8 grains.

6. The acid solution being concentrated by gentle eva-
ration, a number of small yellowish-coloured needles
de their appearance in it. By repeated evaporations, all
crystals that would form were separated. They weighed
grains. I redissolved them in water, and added some
monia to the solution. A fine yellow powder fell, which
oon recognized to be oxide of uranium. It weighed 4·2
ains.

7. Thus it appears, that the 52 grains (No. 4.) attracted
the magnet contained 46 grains of iron, and six grains
uranium and titanium.

8. The following are the substances separated from 100
ains of iserine, by the preceding analysis:

Oxide of titanium,	54·8
Oxide of iron, -	46·0
Oxide of uranium,	4·2
Silica, - -	16·8
Alumina, -	3·2
Total,	125·0

Here is an excess of no less than 25 grains, to be ac-
unted for by oxygen, which must have united to the
ree metals during the process. As to the silica and alu-
ina, there can be little hesitation in ascribing them to
ains of sand, which had been mixed with the ore. The
re iserine, in all probability, was composed of iron, ti-
nium, and uranium. If we suppose that each of these
etals existed in the state of protoxide, we must diminish
e titanium by one-fourth, the iron by one-seventh nearly,
d the uranium, according to Bucholz's experiments, by
e-fifth. This would give us,

Titanium, - -	41·1
Iron, - - -	39·4
Uranium, - -	3·4
Silica and alumina -	20·0
	103·9

Here, then, is still an excess of nearly four per cent.
t this I am disposed to ascribe to the oxides of titanium
d uranium, having been only dried upon the steam-bath.
pon the whole, it appears, that in the specimens of iserine
analysed,

analysed, the proportions of titanium and iron were nearly equal, and that the uranium did not exceed four per cent. The appearance of uranium surprised me a good deal. I perceive, however, that it has already been detected in this ore, from an analysis published by Professor Jameson, in the second volume of his Mineralogy, which, I understand, was made by Lampadius. The specimen examined by Lampadius yielded very nearly 60 parts of titanium, 30 of iron, and 10 of uranium. Whereas, in mine, if the foreign matter be removed, there was obtained, very nearly,

48	titanium,
48	iron,
4	uranium,
100	

But there can be no doubt, that the iserine which I analysed was still contaminated with a good deal of iron-sand; for it was impossible to remove the whole.

XVII. *Analysis of the Gray Copper Ore of Airthrey, in Stirlingshire.* By Thomas Thomson, *M.D., Edinburgh*[*].

The copper mine of Airthrey, near Stirling, consists of a thin vein, which runs through the west corner of the Ochils. It has been twice wrought, by two different companies; but, in both cases, was abandoned, after a few years' trial. I went to it some years ago, and examined the ore, at the request of one of the proprietors. The specimens which were employed for the subsequent analysis, were the purest that I could select, out of a considerable quantity. I was told, however, that from the lower level, which was at that time full of water, much richer ore had been extracted. But, afterwards, when the lower level was free from its water, I went down to it myself, and found the ore precisely of the same kind as in the upper, with this difference, that it was more mixed with calcareous spar, and perhaps, on that account, more easily smelted.

The veinstones in the Airthrey mine are sulphate of barytes and carbonate of lime, and with these the ore is almost always more or less mixed.

The colour is at first light steel-gray; but the surface soon tarnishes, and becomes of a dark dull leaden-gray, and

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[*] From Transactions of Royal Society, Edinburgh, 1807.

nd in some places assumes a beautiful tempered steel tar-
ish. Massive and disseminated. In some specimens, it
xhibits the appearance of imperfect crystals. Internal
urface shining and metallic; but, by exposure, it soon
ecomes dull. Fracture small-grained, inclining to even.
ragments indeterminate, and rather blunt-edged. Semi-
ard, the degree being almost the same as that of calcareous
par; for these two minerals reciprocally scratch each other.
treak similar, opake, brittle, easily frangible; specific
ravity 4·878.

1. To free the ore as completely as possible from foreign
atter, it was reduced to a coarse powder, and carefully
cked. It was then digested in diluted muriatic acid,
hich dissolved a quantity of carbonate of lime, amounting
13 per cent. of the original weight of the ore.

2. Thus purified, it was dried on the steam-bath, and
00 grains of it were reduced to a fine powder, and digested
diluted nitric acid, till every thing soluble in that men-
ruum was taken up. The residue was digested in the same
anner, in muriatic acid; and when that acid ceased to
t, the residue was treated with nitro-muriatic acid till no
rther solution could be produced. The insoluble matter
as of a white colour; it weighed 6·9 grains, and was al-
ost entirely sulphate of barytes. No traces of sulphate of
ad, nor of oxide of antimony, could be detected in it by
e blow-pipe.

3. The three acid solutions being mixed together, no
oudiness appeared, nor was any change produced; a proof
at the ore contained no silver.

4. The solution being evaporated nearly to dryness, was
luted with water, and precipitated by muriate of barytes,
y this means, the sulphuric and arsenic acids, which had
een formed during the long-continued action of the nitric
id on the ore, and the presence of which had been indi-
ted by re-agents, were thrown down; for nitrate of lead,
dded to the residual liquid, occasioned no precipitate; a
roof that no arsenic acid was present.

5. The liquid, thus freed from arsenic acid, was mixed
ith an excess of ammonia. It assumed a deep blue co-
ur, while a brown matter precipitated. It was separated
y the filter, and being dried, drenched in oil, and heated
redness, it was totally attracted by the magnet. It
eighed 45·5 grains, and was iron.

6. The ammoniacal liquid was neutralized by sulphuric
cid, and the copper thrown down by means of an iron
late. It weighed 17·2 grain [illegible]

7. To ascertain the quantity of sulphur and arsenic, 100 grains of the purified ore, in the state of a fine powder, were put into the bottom of a coated glass-tube, and exposed for two hours to a red heat. When the whole was cold, and the bottom of the tube cut off, the ore was found in a round solid mass, having the metallic lustre, a conchoidal fracture, and the colour and appearance of variegated copper-ore. It had lost 16 grains of its weight.

8. The upper part of the tube was coated with a yellowish-brown substance, like melted sulphur. It weighed 12·6 grains. Thus, there was a loss of 3·4 grains. As the tube was long, this loss can scarcely be ascribed to sulphur driven off. I rather consider it as water. For towards the beginning of the process, drops of water were very perceptible in the tube. Whether this water was a constituent of the ore, or derived from the previous digestion in muriatic acid, cannot be determined.

9. When the 12·6 grains of yellowish brown matter detached from the tube were digested in hot potash-ley, the whole was dissolved, except a fine blackish powder, which weighed one grain, and was arsenic. The dissolved portion I considered as sulphur.

10. The potash solution, being mixed with nitric acid, four grains of sulphur fell. The remaining 7·6 grains must have been converted into sulphuric acid, by the action of the nitric acid. Accordingly, muriate of barytes occasioned a copious precipitate,

11. The 84 grains of roasted ore being reduced to a fine powder, mixed with half their weight of pounded charcoal, and roasted a second time in a glass-tube, one grain of sulphur sublimed. But the tube breaking before the roasting had been continued long enough, the process was completed in a crucible. The roasted ore weighed 70 grains.

12. From the preceding analysis, we learn that the constituents of the Airthrey ore are as follows:

Iron -	45·5
Copper -	17·2
Arsenic -	14·0
Sulphur -	12·6
Water -	3·4
Foreign bodies	6·9
	99·6
Loss -	·4
	100·0

If we suppose the water and the earthy residue to be only cidentally present, then the only essential constituents are e first four, and the ore would be a compound of

Iron -	51·0
Copper -	19·2
Arsenic -	15·7
Sulphur -	14·1
	100·0

If we compare this analysis with several analyses of gray pper ore, lately published by Klaproth, we shall find, at the constituents are the same in both; but the prortions of the two first ingredients are very nearly rersed. Klaproth obtained from 0·4 to 0·5 of copper, and om 0·22 to 0·27 of iron. This renders it obvious, that e two ores were not in the same state. I have little doubt, at the difference, however, is merely apparent, and that arose, altogether, from a quantity of iron pyrites, and rhaps also of arsenic pyrites, which I could not separate om the gray copper ore which I examined. Both of these inerals could be distinctly seen in many of the specimens, timately mixed with the gray copper; and I have no ubt that the same mixture existed, even in those speciens which were selected as purest. The difference in the oportions of copper and arsenic, obtained by Klaproth[*] his various analyses, is so considerable, as to lead to a spicion, that even his specimens, in all probability, conined a mixture of foreign matter.

VIII. *Hints on the Subject of Animal Secretions.* By EVERARD HOME, *Esq., F.R.S.*[†] *Communicated by the Society for the Improvement of Animal Chemistry*[‡].

HE brilliant discoveries of Mr. Davy on the powers of ctricity in producing chemical changes, suggested to me the

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[*] Gehlen's Journ. vol. v. p. 9, 11, 13.

[†] From Philosophical Transactions, for 1809, Part II.

[‡] Dr. Wollaston's observations inserted in the Philosophical Magazine, re published after this paper had been laid before the Society.

was led to the present investigation, while preparing my lectures on the interian Museum, in which the secretions in different animals are to be isidered. In September last, I engaged Mr. William Brande to assist me prosecuting the inquiry. In November, I communicated my opinions to Joseph Banks, and stated that I should bring them forward in my lectures the idea that the animal secretions may be produced by the same means.

To prosecute this inquiry with every advantage, requires a knowledge of anatomy, physiology, and chemistry, rarely to be met with in the same person. I have therefore availed myself of the assistance of the different members of this society, the object of which is the improvement of animal chemistry. Their intimate acquaintance with these branches of science renders them peculiarly fitted for such an undertaking.

It is one of the most important subjects to which Mr. Davy's discoveries can be applied, and he has given it the consideration it deserves.

The Voltaic battery is met with in the torpedo and electrical eel; and although it is given only as a means of catching their prey, and defending themselves, and therefore not immediately applicable to the present inquiry, yet it furnishes two important facts: one, that a Voltaic battery can be formed in a living animal; the other, that nerves are essentially necessary for its management; for, in these fish, the nerves connected with the electrical organs exceed those that go to all the other parts of the fish in the proportion of twenty to one. The nerves are made up of an infinite number of small fibres, a structure so different from that of the electric organ, that they are evidently not fitted to form a Voltaic battery of high power; but their structure appears to Mr. Davy to adapt them to receive and preserve a small electrical power.

That the nerves arranged with muscles, so as to form a Voltaic battery, have a power of accumulating and communicating

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tures; at that time Dr. Young's Syllabus was not published, and Dr. Wollaston's opinions were unknown to me.

Dr. Berzelius, professor of chemistry at Stockholm, published a work on Animal Chemistry, in the year 1806, in the Swedish language, in which he states, in several places, that he believes the secretions in animals to depend upon the nerves, although he is unable to explain how the effect is produced. In proof of his opinion, the following experiment is adduced:

"Trace all the nerves leading to any secretory organ in a living animal, and divide them, being careful to injure the blood-vessels and the structure of the organ itself, as little as may be: notwithstanding the continued circulation of the blood, the organ will as little secrete its usual fluid, as an eye deprived of its nerve can see, or a muscle whose nerve has been divided can move. We may therefore easily conceive, that any trifling alteration in the nerves of a gland may materially affect its secretion, the supply of blood being in every way perfect."

He says, the agency of the nerves in secretion has generally been disregarded, because our attention is only called to their secret mode of acting, when we discover the insufficiency of all other explanation. Dr. Berzelius's work was shown to me by Mr. Davy while this paper was in the press.

iunicating electricity, is proved by the well-known expe- ment of taking the two hind legs of a vivacious frog, im- iediately after they are cut off, laying bare the crural erves, applying one of these to the exposed muscles of the ther limb, and then when the circle is completed by rais- ig the other crural nerve with a glass rod, and touching ie muscle of the limb to which it does not belong, the uscles of both are excited to contractions.

There are several circumstances in the structure of the erves, and their arrangements in animal bodies, which do ot appear at all applicable to the purposes of common sen- tion, and whose uses have not even been devised. Among iese are the plexuses in the branches of the par vagum hich go to the lungs, and in the nerves which go to the nbs. The ganglions, which connect the nerves belong- g to the viscera with those that supply the voluntary uscles, and the course of the nerves of the viscera which eep up a connexion among themselves in so many diffe- nt ways.

The organs of secretion are principally made up of arte- es and veins; but there is nothing in the different modes which these vessels ramify, that can in any way account r the changes in the blood, out of which the secretions ise. These organs are also abundantly supplied with rves.

With a view to determine how far any changes could be oduced in the blood by electricity, at all similar to se- etion, Mr. W. Brande, who has begun his career in ani- al chemistry with so much success, made the following periments, in the suggestion of which Mr. Davy afforded m every assistance.

Experiment I. Middle of January, 1809.

The conductors from twenty-four four-inch double plates copper and zinc, charged with a very weak solution of uriatic acid, were immersed in four ounces of blood, im- ediately on its having been withdrawn from a vein in the m. The temperature of the blood was kept up at 100° iring the experiment. The apparatus was so constructed to admit of the products at the negative and positive ires being separately collected and examined. When the ectrization had been carried on for a quarter of an hour, action seemed to have ceased. The blood which had rounded the negative wire was of a deep red colour and remely alkaline; that surrounding the positive wire was gly acid, and of a brighter hue.

In this experiment, the coagulation of the blood was not materially affected by the electrical power alluded to.

Experiment II. 8th of February, 1809.

Finding it necessary to submit perfectly fluid blood to the action of electricity, the following experiment was undertaken with a view of keeping it the longest possible time in that state.

A deer having been pithed, the abdomen was immediately opened into, and a length of about four inches of a large vein in the meso-colon was detached from the neighbouring parts. Two small platina wires, connected in the usual way with forty three-inch double plates, were inserted into this detached portion of vein, and secured by ligatures, having their points at a distance of about one inch from each other. The communication with the battery was kept up for one quarter of an hour, a third ligature was then tied in the centre of the detached vein, in order to cut off the connexion between the positive and negative ends. On removing the portion of the vein included by the ligatures, and containing the conductors, it was found that the gaseous products had forced out nearly the whole of the blood, at the part through which the wires were inserted; alkaline and acid matter were readily detected, but no new product could be discovered.

Finding the coagulation of the blood an insurmountable obstacle to the long-continued electrical action, the serum only was employed in the following experiments.

Experiment III. 10th of March, 1809.

The conductors from one hundred and twenty four-inch double plates, highly charged, were brought within two inches of each other, in some recent serum of blood, obtained free from the colouring matter, by carefully pouring it off from the coagulum. Coagulated albumen was rapidly separated at the negative pole, and alkaline matter evolved: at the positive pole, a small quantity of albumen was gradually deposited, and litmus paper indicated the presence of acid. These are the effects produced by a high electrical power upon serum.

Experiment IV. 14th of April, 1809.

Was undertaken to ascertain the effect of a low power: battery was employed, consisting of twelve four-inch ble plates of copper and iron. In this case, there was no appearance of coagulation at either pole; in five minutes,

inutes, the positive wire became covered with a film of bumen, and in fifteen minutes a filament of about a quarr of an inch in length was seen floating in the fluid, and lhering to the same wire.

Experiment V. 6th of May, 1809.

Two small platina cups, connected by a large quantity cotton well washed, and each containing one ounce of rum, were rendered positive and negative, by thirty double ree-inch plates very *weakly* charged. The process was ntinued during twenty-four hours. This power had not en sufficient to produce coagulation at the negative pole; n examining the fluid in the negative cup, it was found consist principally of an alkaline solution of albumen.

The fluid in the positive cup was rather turbid, it redned litmus, and was slightly acid to the taste. On standg, it deposited a few flakes of albumen. When evapoted, it afforded saline matter, with excess of acid, (super lts.)

By these experiments it is ascertained, that a low negare power of electricity separates from the serum of the bod an alkaline solution of albumen; that a low positive wer separates albumen with acid, and the salts of the ood. That with one degree of power, albumen is sepated in a solid form; with a less degree, it is separated in fluid form.

From these facts, the following queries are proposed:

1st. That such decomposition of the blood by electricity, ay be as near an approach to secretion as could be expected be produced by the artificial means at present in our power.

2d. That a weaker power of electricity, than any that n be readily kept up by art, may be capable of separating m the blood, the different parts of which it is composed, d forming new combinations of the parts so separated.

3d. That the structure of the nerves may fit them to have low electrical power, which can be employed for that rpose; and as such low powers are not influenced by imrfect conductors, as animal fluids, the nerves will not be bbed of their electricity by the surrounding parts.

4th. That the discovery of an electrical power, which n separate albumen from the blood in a fluid state, and other that separates it in a solid state, may explain the ode in which different animal solids and fluids may be oduced, since, according to Mr. Hatchett's experiments, umen is the principal material of which animal bodies composed.

5th. That the nerves of the torpedo may not only keep the electric organ under the command of the will, but charge the battery, by secreting the fluid between the plates, that is necessary for its activity.

6th. As albumen becomes visibly coagulated, by the effect produced from twelve four-inch double plates of copper and iron, a power much too low to affect even the most delicate electrometer, may not this be occasionally employed with advantage as a chemical test of electricity, whilst the production of acid and alkali, affected by still inferior degrees of electricity to those required for the coagulation of albumen, may likewise be regarded as auxiliary tests on such occasions?

If these facts and observations appear to the society to throw any light upon the principle of secretion, it may be an advantage to medical science, that they should be laid before the public, as hints for future inquiry.

XIX. *Geological Remarks and Queries on Messrs.* CUVIER *and* BROGNIART'S *Memoir on the Mineral Geography of the Environs of Paris. By Mr.* JOHN FAREY, *Mineralogical Surveyor.*

To Mr. Tilloch.

SIR, IT gave me great pleasure to observe, that you had in the commencement of the present volume of the Philosophical Magazine, introduced the term "*Geology*" into its title; a science, if I may so call it, which stands perhaps more in need than any other, of a respectable channel for freely discussing its facts and principles; and the satisfaction I felt, was much increased, by the perusal of the valuable body of facts which you have therein presented to the English reader, respecting the stratification in the neighbourhood of Paris. We are informed by the very able naturalists who drew up the Memoir alluded to (p. 37), that their geological survey of the environs of Paris (which commenced in 1804 or 1805) was unfinished at the beginning of 1809, but that "some circumstances" compelled them at that moment, to publish an abridged account of their labours, "and to assign *a date*" to their laborious researches, although they were not then brought to a conclusion. What the "circumstances" were which are here alluded to, we are uninformed; but it seems material to the credit of our country, and an act of justice to a meritoious Individual to state, that most of the important geological conclusions which our ingenious neighbours on the continent have deduced from their recent survey, were arrived at by Mr. *William Smith*, from an examination of the strata in the environs of Bath, of which he completed a coloured Map, such as M. Cuvier and M. Brogniart describe, and correct Sections also, about the year 1795; and that the same principles and conclusions were in the subsequent years applied and verified, in a general survey made by Mr. Smith, of nearly all the south and east and midland parts of England, and of parts of Wales and Scotland, as many hundred besides myself, who have seen Mr. Smith's Maps and collection of local Specimens, can testify*, as well as the printed accounts which have repeatedly appeared in our newspapers and magazines of Mr. Smith having publicly exhibited these Maps, accompanied by Sections of the British strata, at the Duke of Bedford's and Mr. Coke's sheep-shearings, Lord Somerville's agricultural fêtes, the meetings of the Smithfield Club, the Bath Society, &c. Mr. Smith's extensive collection of specimens, of Organic Remains in particular, which were collected by himself from all the various districts which he visited for forming his Maps, were for several years exhibited to his friends in Bath, and have now for seven years past, been accessible in the same way, at his house, No. 15, Buckingham Street, York Buildings, London.

An opportunity having presented itself in 1801 †, of my studying this subject practically under Mr. Smith, and having since amply verified the truth and ascertained the great value of his discoveries in several Surveys of my own; I ventured, in June 1806, to present the public with a concise sketch of part of Mr. Smith's discoveries, when speaking of the intended Thames Archway, vol. xxv. of your Magazine, and some months after to enlarge somewhat further on these discoveries, in something more than two quarto pages, which are inserted into an article on *Coal*, previously written by some other person, for *Dr. Rees's New Cyclopædia*: to these publications, and those which have followed ‡, I was principally prompted by the solicitations of the friends and well-wishers of Mr. Smith, who, lamenting the want of encouragement which suspended his intended publication, foresaw, that at no very distant period, the free communications which he, myself, and others had made, would produce other publications, by which the credit of his discoveries might be lost, not only to himself, but perhaps to our country.

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* The Rev. Joseph Townsend, rector of Pewsey in Wiltshire, was among hose who thus profited by Mr. Smith's labours, almost from their commencement.

† This was after Mr. Smith had issued printed proposals for the publication of his work and maps of the British strata, by Debrett, in Piccadilly.

‡ Among these are, *Clay* Strata, *Concentricity* of Strata, *Continent*, *Denudation*, *Excavation* of Valleys, *Extraneous* Fossils, &c., in the Cyclopædia; and Letter on *Wells and Springs* in the Monthly Magazine for April 1807.

The haste with which the Memoir on the Environs of Paris was drawn up, will be apparent to any one, who shall sit down carefully to digest it, and follow the writers through their details of the interesting geological facts which it contains; in doing which, I found it absolutely necessary to make other arrangements of the Places' names, and of the extraneous Fossils, than those which the writers have presented, as well as to arrange the Strata described, in a Table, to be seen at one view: and thinking that such may prove interesting and perhaps useful to others of your readers, I have transcribed them below, for insertion in your Magazine, in case that you should think them sufficiently suited to the new character which it has assumed.

The difficulty of understanding and following M. Cuvier and his able associate through their details, is greatly increased by the want of a good descriptive Map, such as they promise in their subsequent Memoir; this I have endeavoured to obviate, as well as my very limited knowledge of France and its maps would allow, by giving the bearings, nearly, and direct distances in English miles, of such of the places named as I could, reckoning from the island in the Seine in the centre of Paris.

Geological students will find an alphabetical arrangement of the *places* where observations have been made, with the strata and fossils annexed; and another alphabet of the *fossils*, with reference to the places and strata to which they belong, highly useful, as companions to the *sections* and descriptions of the strata in their relative positions, and to coloured *mineralogical maps*, showing the surface made by each *soil**; a term which, in the Memoir, answers

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* Mr. William Martin, in his useful work "Outlines of the Knowledge of extraneous Fossils," which should be in the hands of every geological student, defines this term, p. 155, and remarks at the bottom of page 158, that we are "to consider the comparative *ages of soils* to be marked, in the first instance rather by *the nature* than *the quantity* of their organic contents," which, though true, is not sufficient to guard us from the Wernerian errors, into which himself has been betrayed in this and some other sections of his work, in maintaining, that the absence of organic remains, marine remains, marine and vegetal, and wood and skeletons, denote the *relative ages* of the substances to which they belong: since experience must teach every one, as it has in part done our Paris mineral surveyors, that *super-position alone* can instruct

nswers to an assemblage of contiguous* and allied strata, s the genera of plants and animals do to their species. I hall begin with the names of the material places mentioned, nd refer to the pages of the Memoir in your Magazine in ach instance: adding occasional remarks of my own, in arentheses.

Antoni, 11 miles SW. by W. of Paris, has plaster quarries at the edge of the gypsous soil; wherein the remains of mammiferæ are found, and whose intervening marles are without lenticular gypsum; probably this is the upper or first mass of the quarryman. p. 53.

Aumont has sand-pits, pure, without fossils. p. 56.

Bagneux, (Bogneux by mistake) 4½ miles SSW., has plaster quarries in the first mass, (see *Antoni* above). p. 53.

Beauce is a very extensive and high platform of (alluvial) Sand, whose extremities are Courville NW., and Montarges SE., (which should have been described in article X. if the memoir had not abruptly terminated) to the westward of Paris, whose very indented north-eastern edge, from the Maulde river to Nemours, forms the south-western limits of the district called the Bason or Environs of Paris, which is the subject of this Memoir. p. 38 and 39.

Bois de Boulogne, 4½ miles NW. by W., is on a plain covered by flint gravel. p. 58.

Bougival near Marly, 10 miles W., has chalk-pits, in distinct beds, with few flints: the chalk here has no covering, except in some places (alluvial) marly sand, containing small and large blocks of calcareous stones of a very fine grain, (perhaps the gray-wethers or cherty blocks of our Wiltshire and other chalk hills). p. 41 and 43.

Champagne Province, to the eastward of Paris, is a very large district of chalk (forming large plains) having isolated patches (or hummocks) of (alluvial) sand upon them. p. 39 and 40.

Champigny, 8 miles SE. by E., has quarries and lime-kilns;

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nstruct us, as to the relative ages of particular soils. The formations or oils, numerous as they are which the Wernerians describe in the order of heir respective ages, according to his theory, are found inapplicable by most orrect observers, in districts distant from those on whose soils they were riginally founded; and hence the Derbyshire limestone is not found by Mr. W. Martin to accord with the Wernerian Hypothesis, p. 158: or the ypsums of Paris observed by M. Cuvier and M. Brogniart, with *any* of M. Werner's Formations, p. 53.

* In the order of super-position.

the limestone is fine-grained, and was porous (*caverneux*), but is now compact, owing to the infiltration of silex into its cavities; it is without fossils. p. 55.

Chantilly, 17 miles N., has madrepores, with camerines and other well preserved shells, on the declivity of the mountain, and coarse quartz grains, forming a sort of puddingstone. p. 47.

Chatou (Chateu by mistake), 8 miles NW., has flint gravel accumulations. p. 58.

Chaumont, 29 miles NW., is on the north-western edge of the bason or environs of Paris, described in the Memoir; having chalk strata as its boundary; with the coarse limestone strata of *Pallery* in its vicinity. p. 39 and 47.

Chelles has plaster quarries, at the extremity of the gypseous soil, in the first mass, (see *Antoni* above). p. 53.

Clamart, 5½ miles SW., has quarries of coarse limestone, and above these, quarries of plaster; which are at the edge of the gypseous soil, in the first mass, (see *Antoni* above). p. 49 and 53.

Compiegne, 33 miles NE., is at a reentering angle or indentation of the NE. side of the district, called the bason or environs of Paris in the Memoir. Mount Ganelon, with a puddingstone of quartz and shells on it, is near this place. p. 39 and 47.

Concale Bay near St. Maloes, 160 miles W., produced oysters on its shores since the earliest records, which have not given place to other shell-fish. p. 49.

Condé , has pits of gray potters' or plastic clay. p. 44.

Dammartin, 14 miles NE., has plaster-pits in the first mass, which have no covering strata of marle, &c. p. 50.

Espernay (Epernay), 56 miles NE. by E., is at the eastern edge of the bason of Paris, which is described in the Memoir; having chalk strata as its bounds. p. 39.

Etampes, 23 miles SSW., has sand-pits, pure, without fossils. p. 56.

Fontainebleau, 24 miles SSE.——ditto——ditto——ditto.

Fontainebleau Forest, SSE., a rugged and uneven district: siliceous limestone is here found, producing burstones, (French Burs, used here in making mill-stones): sometimes the covering or superficial strata are alluvia or argillaceous marles, and sometimes free-stone without shells, which last alternate with sand, which is sometimes displaced, leaving the blocks of stone lying in confusion: sometimes these free-stones are coloured or rendered calcareous or argillaceous by infiltration from the covering strata. p. 39 and 56.

ntenay-aux-Roses, 5½ miles SSW.: here M. Lopez sunk a deep well in his garden, through the gypsum and coarse limestone formations. p. 50.

anelon Mountain, near Compiegne, NE., has (alluvial) puddingstone on it, of coarse quartz grains and shells, &c. p. 47.

entilly, 3 miles S., has pits of coloured potters' clay, which stratum extends in a range to *Meudon*: it has also quarries of coarse limestone that produce in their lower beds periwinkles, solens, (omitted in the translation) oysters, muscles, pinnæ, calyptræ, pyrulæ, large tellines, terebellæ, porpytes, madrepores, nummulites, and fungites; above which are strata of green earth with some vegetable impressions, then gray or yellowish strata containing venuses, campreys, and numerous tuberculated cerites: beds of good building-stone; above these is a stratum containing an entire bed of small long and striated tellines: the fossils are the same in these quarries as at *Grignon* and *Meudon*, and on the top of *Montmartre*. p. 44, 47, 48 and 55.

isors, 35 miles NW., is at the extreme NW. corner of the bason of Paris described, and adjoining the chalk districts. Mount *Ouin*, with a puddingstone of quartz and shells in it, is near this place. p. 39 and 48.

rignon , has quarries of coarse limestone, whose lower beds contain periwinkles, and the twelve other kinds of shells and fossils mentioned at *Gentilly*, with the addition of cerites (as mentioned p. 54); also the fossils agree with those of the quarries at *Meudon* and on the top of *Montmartre*. pages 47, 54 and 55.

risy is situated at one extremity of the gypsous district and *Meaux* at another; it has only the first mass of gypsum in its quarries. p. 49 and 53.

uespelle , the strata of this place answer to those of *Gentilly*, above. p. 47.

igh Normandy province, to the north-west of Paris, is wholly composed of chalk (forming the NW. border of the basin of Paris). p. 39.

oudan, 28 miles W., has pits of gray potters' or plastic clay. p. 44.

y, 4 miles SW. by W., has quarries of coarse limestone,

stone, with periwinkles and the twelve other fossil remains in their lower beds, that are mentioned of *Gentilly*. p. 47.

Laonnois is a district (of chalk) NE. of *Compiegne* between the rivers Oyse and Aure, which bound the basin of Paris, and forms therein a considerable reentering angle. p. 39.

Longjumeau, 9 miles SW. by W., has siliceous freestone and sand, containing the 13 kinds of shells, &c., found at *Gentilly* and *Grignon*, with the addition of balani: towards the top of the hill a sand (probably alluvial) contains lymneæ and planorbis with siliceous wood, and other parts of vegetables. p. 54 and 57.

Mantes, 22 miles WSW., is on the NW. edge of the basin of Paris, and adjoins the chalk district without it. p. 38 and 39.

Maulde River, which empties itself into the Seine, is at that place the limits of the basin of Paris, at the NW. end of the (alluvial) sands of *Beauce*, which form its crooked boundary SE. thence to *Nemours*. p. 38 and 39.

Meaux, 19 miles NE. by E., is situate at one extremity of the gypseous district within the basin of Paris, and *Grissy* is at another. p. 49.

Meudon. 6 miles SW., has chalk-pits, in which are layers of flints about 6½ feet apart, but no detached flints in its mass; the upper part of the chalk is in a rubbly state, with the clay of the superior strata in its interstices. A stratum of coloured potters' clay, without fossils, covers the chalk here, and extends south-east wardly to *Gentilly*: above this clay, coarse limestone strata here occur, and the lower beds in the quarries contain calyptreæ, fungites, madrepores, muscles, nummulites, oysters, periwinkles, pinnæ, porpytes, pyrulæ, solens, large tellines, and terebellæ; which fossils are of the same kinds as those of the quarries of *Gentilly* and *Grignon* and on the top of *Montmartre*. The gypsum soil occurs here upon the coarse limestone soil; there are only thin beds of plaster in it, but which are sufficient to fix the relative super-position of these soils. pages 41, 43, 44, 47, 49, 53 and 55.

Montmartre, 2½ miles N., has had its strata and fossils described by M. Desmarets; it has large plaster quarries in which are three principal masses of gypsum, the lowest of which is without fossils, and has only thin beds of gypsum, frequently selenitous, with intervening strata of solid calcareous marles, that contain coarse crystals of lenticular yellowish gypsum; and with very scaly argillaceous marles that contain menilite silex. The middle mass has thicker and more numerous beds of gypsum, the intervening solid calcareous marles have in them numerous coarse crystals of lenticular yellowish gypsum: some of these beds, those that are marbled with gray and are compact and argillaceous, are used for scouring-stones (not building). It contains scattered nodules (*rognons*) of sulphated strontian in its lower beds; and it is chiefly in this mass that fossil fish (not shell-fish) are found.

The upper mass is very thick, chiefly of beds of gypsum, interlaid with a few marley strata: its lower beds of gypsum are intermixed with siliceous masses, its middle beds are columnar, and its upper gypsum beds are usually interlaid with five strata of marle: this upper gypseous mass contains astonishing collections of the skeletons of birds, of quadrupeds, mammiferæ, and fish, tortoise bones, and a few shells, agreeing with those of the fresh-water fish of our lakes. A large mass of marles occur above these, imbedding different fossils (for which I must refer to page 51 and 52, to the Section, and to what follows herein).

Above these marles, freestone and sand here occur, that contain the 13 kinds of fossil remains, mentioned at *Gentilly*, *Meudon* and *Grignon*, with the addition of balani shells. pages 37, 50, 51, 52, 54 and 55.

Montmorency, 10 miles NE., has the upper mass of gypsum, and its fossils (see *Montmartre*) forming the surface, in some parts; in others, it has above the gypseous and marles, the freestone and sand which contain the 14 kinds of fossil remains that are found on *Montmartre* &c. p. 50 and 54.

[illegible], 22 miles SE., is at the south-eastern edge of the basin of Paris described by our authors, and adjoins the great chalk districts in *Champagne*: gray potters' clay is here dug, upon the chalk. p. 39 and 44.

[illegible], [illegible] miles W., is near the western edge of the basin of Paris. p. 39.

[illegible], has plaster quarries, near the [illegible] the gypseous district, where only the first or upper mass of gypsum appears. p. 53.

[illegible] Forest, has pits of very white potters' [illegible] that covers the chalk, and is without fossils. p. [illegible]

Nanterre, 7½ miles NW., has a plain covered by flint-gravel. p. 58.

Nemours, 32 miles SSE., is at the limits of the basin of Paris, and at the SE. end of the (alluvial) sands of *Beauce*, which form its crooked boundary thence NW. to the mouth of the *Maulde* river. p. 39.

Neuilly, 4½ miles NW., has quarries of stone, in which are found crystals of quartz and rhomboidal crystals of variegated carbonate of lime, but no fossil remains. p. 48.

Ouin Mountain, near Gisors, NW., has (alluvial) pudding-stone of coarse grains of quartz and shells. p. 48.

Palaiseau , has strata of freestone, without shells, alternating with sand, which is sometimes washed out or displaced, and the blocks of stone are left in confusion. p. 56.

Pallery, near Chaumont, has in its lower strata of coarse limestone and sand, the thirteen kinds of fossil remains that are found at *Gentilly*. p. 47.

Paris Environs has had its stratification and extraneous fossils treated of by M. Desmarets, M. Gillet-Laumont, M. Lamarck, M. Coupé, and M. Cuvier. About the beginning of 1805, M. Cuvier and M. Brogniart commenced a Mineralogical Map of that district around Paris, denominated by them the *Basin of Paris*, in which the chalk in horizontal beds with flints, (being the lowest stratum there known) is wholly or in part covered by certain argillaceous, siliceous, calcareous, gypseous and alluvial strata; the nature, contents and relative situations and thicknesses of which, it is the object of their present labours to investigate and explain. This basin, measuring directly from *Espernay* to *Gisors*, nearly from east to west, is 87 English miles long, and from *Nemours* to the neighbourhood of Noyon (to which it must extend to form the reentering angle that is mentioned p. 39), nearly south and north, it is 70 miles broad. This noble and extensive field for geological investigation, has on all sides natural limits, which seem very crooked and indented, as might be expected: on the SW. from near *Nemours* to the mouth of the *Maulde* river (a direct distance of about 45 miles), it is limited by a covering stratum of Beauce *sand* (which I suppose from its description to be alluvium), and on all its remaining sides of solid calcareous marles, that contain coarse crystals of lenticular yellowish gypsum; and with very scaly argillaceous marles that contain menilite silex. The middle mass has thicker and more numerous beds of gypsum, the intervening solid calcareous marles have in them numerous coarse crystals of lenticular yellowish gypsum: some of these beds, those that are marbled with gray and are compact and argillaceous, are used for scouring-stones (not building). It contains scattered nodules (*rognons*) of sulphated strontian in its lower beds; and it is chiefly in this mass that fossil fish (not shell-fish) are found.

The upper mass is very thick, chiefly of beds of gypsum, interlaid with a few marley strata: its lower beds of gypsum are intermixed with siliceous masses, its middle beds are columnar, and its upper gypsum beds are usually interlaid with five strata of marle: this upper gypseous mass contains astonishing collections of the skeletons of birds, of quadrupeds, mammiferæ, and fish, tortoise bones, and a few shells, agreeing with those of the fresh-water fish of our lakes. A large mass of marles occur above these, imbedding different fossils (for which I must refer to page 51 and 52, to the Section, and to what follows herein).

Above these marles, freestone and sand here occur, that contain the 13 kinds of fossil remains, mentioned at *Gentilly*, *Meudon* and *Grignon*, with the addition of balani shells. pages 37, 50, 51, 52, 54 and 55.

Iontmorency, 10 miles NE., has the upper mass of gypsum, and its fossils (see *Montmartre*) forming the surface, in some parts; in others, it has above the gypsums and marles, the freestone and sand which contain the 14 kinds of fossil remains that are found on *Montmartre* &c. p. 50 and 54.

Iontereau, 22 miles SE., is at the south-eastern edge of the basin of Paris described by our authors, and adjoins the great chalk districts in *Champagne*: gray potters' clay is here dug, upon the chalk. p. 39 and 44.

Iontfort, 16 miles W., is near the western edge of the basin of Paris. p. 39.

Iont Valerian   , has plaster quarries, near the edge of the gypsous district, where only the first or upper mass of gypsum appears. p. 53.

Ioret, in Dreux Forest, has pits of very white potters' (or pipe-)clay; it covers the chalk, and is without fossils. p. 44.

Nanterre, 7½ miles NW., has a plain covered by flint-gravel. p. 58.

Nemours, 32 miles SSE., is at the limits of the basin of Paris, and at the SE. end of the (alluvial) sands of *Beauce*, which form its crooked boundary thence NW. to the mouth of the *Maulde* river. p. 39.

Neuilly, 4½ miles NW., has quarries of stone, in which are found crystals of quartz and rhomboidal crystals of variegated carbonate of lime, but no fossil remains. p. 48.

Ouin Mountain, near Gisors, NW., has (alluvial) pudding-stone of coarse grains of quartz and shells. p. 48.

Palaiseau , has strata of freestone, without shells, alternating with sand, which is sometimes washed out or displaced, and the blocks of stone are left in confusion. p. 56.

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Paris Environs has had its stratification and extraneous fossils treated of by M. Desmarets, M. Gillet-Laumont, M. Lamarck, M. Coupé, and M. Cuvier. About the beginning of 1805, M. Cuvier and M. Brogniart commenced a Mineralogical Map of that district around Paris, denominated by them the *Basin of Paris*, in which the chalk in horizontal beds with flints, (being the lowest stratum there known) is wholly or in part covered by certain argillaceous, siliceous, calcareous, gypseous and alluvial strata; the nature, contents and relative situations and thicknesses of which, it is the object of their present labours to investigate and explain. This basin, measuring directly from *Espernay* to *Gisors*, nearly from east to west, is 87 English miles long, and from *Nemours* to the neighbourhood of Noyon (to which it must extend to form the reentering angle that is mentioned p. 39), nearly south and north, it is 70 miles broad. This noble and extensive field for geological investigation, has on all sides natural limits, which seem very crooked and indented, as might be expected: on the SW. from near *Nemours* to the mouth of the *Maulde* river (a direct distance of about 45 miles), it is limited by a covering stratum of Beauce *sand* (which I suppose from its description to be alluvium), and on all its remaining sides it is said to be bounded by the naked chalk-stratum above mentioned*. The Seine river enters the basin of Paris near *Montereau*, and quits it again near *Gisors*; its tributary rivers the Loing enters it near *Nemours*, the Marne at *Espernay*; and the Aune and Oyse near *Compiegne*, after having for some distance skirted its boundaries. pages 37, 39, 40 and 41.

icardy Province, to the north of Paris, is wholly composed of chalk (forming the northern border of the basin of Paris), having isolated patches (or hummocks) of sand upon it, (perhaps alluvia). p. 39 and 40.

olleboise, on the banks of the Seine, has in the plastic or potters' clay, fragments of bituminous wood: this clay covers the chalk, and underlays the coarse limestone. p. 44.

omainville, 4½ miles NE., has the marley strata that cover the gypsum beds; and in a white and friable calcareous stratum there, siliceous trunks of palm-trees of large bulk, and lymneæ and planorbes shells are found, similar to the species that now exist in our marshes: at this place also freestone and sand are found that contain the 14 kinds of fossils mentioned at *Grignon*, and also balani shells. p. 51 and 54.

aint Cyr, 14 miles WSW., has the gypsum formation, but no beds of plaster; green marles accompanied by strontian here prevail. p. 53.

aint Germain, 12 miles NW., has coarse limestone quarries and their accompanying fossil remains (as at *Gentilly*), and above these is a soft green bed with marks of leaves and stalks of vegetables in its lower strata: on this a freestone rock containing roundish venuses, campreys and numerous tuberculated cerites, and above that a thin hard stratum whose seams abound with small long white striated tellines. p. 48.

aint Germain Forest has certain parts of it covered by flint gravel. p. 58.

int Prix, 11 miles NE., has free-stone and sand strata, that contain the 14 kinds of fossils mentioned at *Grignon*, and also balani shells. p. 54.

ran, on the canal of Ourque, has a deep excavation, made in a marshy bottom for the canal, in alluvial soil, where the bones of elephants and large trunks of trees were found. p. 58.

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* Which seems the same that once extended across the Channel to the itish shore, where it is now found, with local interruptions, from the Isle Wight to the mouth of the Thames. See vol. xxxiv. p. 310.

Sevres, 7 miles W., has quarries of coarse limestone and sand, in which are found the 13 fossil remains mentioned at *Meudon*. Near the glass-house, the mass of chalk (that underlays the above), is elevated near 50 feet above the Seine, and is apparently the highest part of it in the basin of Paris; the strata upon this elevated part seem thinner than usual, and the stone is sensibly inclined towards the Seine, (probably these appearances are occasioned by a fault and consequent tilt of the strata, as these are the only inclining strata that are mentioned in all the basin of Paris) p. 43 and 47.

Sezanne, 48 miles SE. by E., is at the south-eastern edge of the basin of Paris, adjoining the extensive plains of chalk in *Champagne*. p. 39.

Trappe, 14 miles SW., has hard siliceous limestone strata, which quickly perish on exposure to the air and rain; and such is here used for marling the land: in these strata burstones (French Burs) are found, with lymneæ and planorbes shells and gyrogonites, supposed to be of fresh-water origin. p. 56 and 57.

Triel, , has plaster quarries on the mountain, that are situate at an extremity of the gypsous district, in which only the first mass of gypsum is seen (as at *Antoni*): beneath which are strata and quarries of coarse limestone (and its accompanying fossils as at *Gentilly*). p. 49 and 53.

Vaucienne Valley has madrepores, with camerines and other well preserved shells, and grains of coarse quartz, that together make a sort of puddingstone. p. 47.

Vaugirard, 2¼ miles SW. by W., has quarries of coarse limestone, with calyptræ and the 12 other kinds of fossil remains in their lower beds, that are mentioned at *Meudon*. p. 47.

Versaillès Park, 14 miles SW., is situated in a strait between two hills (or in the inosculation of two opposite valleys) where the gypsum formation has no plaster beds in it, but green marles containing nodules of strontian prevail. p. 38 and 53.

Ville-d'Avray, 8 miles W., has plaster quarries in some places, and in others only thin strata of plaster in the gypsum formation; beneath which are strata and quarries of coarse limestone (and its accompanying fossils as at *Gentilly*). p. 49 and 53.

Villepreux, has coarse limestone quarries, and their accompanying fossil remains, (as at *Gentilly*) with the covering strata such as occur at *Saint Germain*. p. 48.

Villers-Cotteret, 31 miles NE., has madrepores, with camerines and other well preserved shells, and grains of coarse quartz, that together make a sort of puddingstone. p. 47.

Viroflay, 9¼ miles SW. The gypsum formation has here no plaster beds in it, but green marles containing strontian prevail. p. 53.

n Alphabetical List of the Fossils *or Organic Remains, which are mentioned by Messrs. Cuvier and Brogniart as found within the Basin of Paris; with the Names of the Places, and of the Strata which produce them, with References to the Pages in the Philosophical Magazine, and Remarks in parentheses.*

nanchites (Echini ?), the shells of calcareous spar, filled with black flint, in the chalk strata. p. 42.

nomites, three Terebratula shells in the chalk strata. p. 42.

ntelopes, bones of unknown species, in the alluvium (of valleys). p. 58.

alani shells, in the freestone at top of Montmartre, at Romainville, Saint Prix, Montmorency, Longjumeau, &c. p. 54.

elemnites, in the chalk strata; these differ from those found with ammonites in compact lime*. p. 42.

irds, skeletons of unknown ones, at Montmartre, &c., in the first or upper gypsum mass. p. 51.

alyptræ shells, at Chaumont, Gentilly, Grignon, Guespelle, Issy, Longjumeau, Meudon, Montmartre, Montmorency, Pallary, Romainville, Saint Prix, Sevres, Vaugirard, &c. in the lower beds of the coarse limestone, and in the freestone. p. 47, 54, and 55.

merine shells, at Chantilly, Ganelon mount, Ouin mount, Vaucienne, Villers-Cotteret, &c., either in the lower beds of the coarse limestone, or in a siliceous puddingstone. p. 47 and 48.

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* The limestone here spoken of, does not seem to belong to the basin of ris, and I should be very glad to learn, whether it is any where dug in ance? In the great argillaceous formation between the Bath Freestone l the blue Lias soils, in this country, there is a stratum of limestone called Mr. Smith the blue marle-stone, which contains very perfect belemnites, all cornu-ammoni and asteria. The limestone of Oakham, and of Maidwell, ween Northampton and Market-Harborough, belongs I believe to this atum, but I never had the opportunity of collecting any specimens at the er place.

Camprey shells, at Gentilly, Saint Germain, Villepreux, &c. in the building-stone rock above the coarse limestone. p. 48.

Cardium shells are found in the yellow argillaceous marle, some distance above the green potter's earth of the gypsous soil, near its top. p. 52.

Caryophyllæa (a polypier) in the chalk strata. p. 42.

Cerite, shells, often in fragments, at Gentilly, Grignon, Meudon, Montmartre, &c., in the lower beds of the coarse limestone, and in the freestone. p. 54 and 55.

——————, tuberculated, at Gentilly, Saint-Germain, and Villepreux, &c. in great numbers in the building-stone rock, above the coarse limestone. p. 48.

——————, often in fragments, in the yellow argillaceous marle near the upper part of the gypseous formation. p. 52.

Cranium, or skull, of some animal, in the chalk strata. p. 42.

Elephants' bones of unknown species, at Seran, &c. in the alluvium (of valleys). p. 58.

Fibrous shells, resembling very thick pinnæ, in fragments, in the chalk strata. p. 42.

Fish (not shell-fish), at Montmartre in the marbled gray scouring (not building) stone of the middlemost gypsum mass, having scattered nodules (*rognons*) of sulphated strontian in its lower beds. p. 50.

—— skeletons, at Montmartre, Dammartin, Montmorency, &c., in the gypsum or intervening marles of the upper gypsum mass or first of the quarrymen. p. 51.

Fresh-water shells (resembling recent species of such shells) Lymneæ and Planorbes, at Montmartre, Romainville, &c., in the upper gypsum mass, or the marles which cover it; and from the decomposing stone at Trappe, near Versailles, (denominated fresh-water by our Authors) resembling siliceous limestone, when first dug. p. 51 and 56.

Fungites, at Chaumont, Gentilly, Grignon, Guespelle, Issy, Meudon, Pallery, Sevres, Vaugirard, &c., in the lower beds of the coarse limestone. p. 47.

Geodes, round stones, some hollow, of a very fine-grained limestone, containing multitudes of small spiral univalves; whose cavities contain a new variety of crystallized sulphate of strontian, called apotamous sulphated strontian; these geodes are found in a marley (alluvial) sand, in some places, at Bougival near Marly, covering the chalk strata, which are themselves not found to contain any simple and regular spiral univalves. p. 42 and 43.

yrogonites, small round hollow (furrowed) bodies, resembling no known recent body, found at Trappe near Versailles, in the decomposing soil (denominated fresh-water soil by our Authors) which, when fresh dug, resembles hard siliceous limestone. p. 57.

eaves of vegetables and stalks, at Gentilly, Saint-Germain, Villepreux, &c., brownish, in the lower beds of the green earth, that underlay the building-stone rock of the coarse limestone soil. p. 48.

—— and parts of vegetables are found, changed into silex, near the top of the hill at Longjumeau, in (alluvial) sand. p. 57.

ituolites of two species, in the chalk strata. p. 42.

ymneæ shells agreeing with recent species, at Romainville, in a white and friable calcareous mass above the gypsums; and at Trappe near Versailles, in a marle there used in agriculture, which when fresh dug resembles hard siliceous limestone. p. 51, 56 and 57.

—— coarse shells in silex, are found in the (alluvial) sand, on the tops of the hills at Longjumeau. p. 57.

actre shells are found in the yellow argillaceous marle that is above the thick green potters' earth of the gypseous formation. p. 52.

adrepores, at Chaumont, Gentilly, Grignon, Guespelle, Issy, Meudon, Pallery, Sevres, Vaugirard, &c., in the lower beds of the coarse limestone. p. 47.

ammiferæ, bones of, at Antoni, Bagneux, Chelles, Clamart, Grisy, Montmartre, Triel, Valerain-mont, &c. in the upper gypseous mass. p. 51 and 53.

———— skeletons, of large unknown kinds, are found in the putrid alluvium (of some of the valleys) of the basin of Paris. p. 58.

illepora, (a polyper) of decomposing pyrites, in the chalk strata. p. 42.

ollusci of the chalk strata are quite different from the testaceous mollucsi of the strata above the chalk. p. 54.

uscles of one particular species are found in the chalk strata, p. 42.

—— at Chaumont, Gentilly, Grignon, Guespelle, Issy, Meudon, Pallery, Sevres, Vaugirard, &c., in the lower beds of the coarse limestone, are found to be of other species. p. 47.

Num-

Nummulites are found at Chaumont, Gentilly, Grignon, Guespelle, Issy, Meudon, Pallery, Sevres, Vaugirard, &c. in the lower beds of the coarse limestone strata. p. 47.

Oxen (bulls or cows), skeletons of unknown species, in the alluvium (of valleys). p. 58.

Oysters of two different species are found in the chalk strata. p. 42.

——— at Chaumont, Gentilly, Grignon, Guespelle, Issy, Longjumeau, Meudon, Montmartre, Montmorency, Pallery, Romainville, Saint Prix, Sevres, Vaugirard, &c. Other kinds of oysters are found in the lower beds of the coarse limestone, and also in the freestone. p. 47, 54 and 55.

——— are found in the marles that are immediately below the argillaceous sand, at the top of the gypseous formation, in two beds, the lower containing large, and the other small oyster shells; they are met with over all the district wherein the entire gypseous formation is found. p. 52.

Palm-trees, trunks of large size, are found converted into silex, lying along in a whitish friable calcareous mass above the gypsums. p. 51.

Periwinkles are found at Chaumont, Gentilly, Grignon, Guespelle, Issy, Meudon, Pallery, Sevres, Vaugirard, &c., in the lower beds of the coarse limestone. p. 47.

Pinnæ shells of a particular species are found in the chalk strata; in which also, fragments of thick shells of a fibrous structure are found, which our Authors consider as more like pinnæ than any other known genus. p. 42.

——— of different species are found at Chaumont, Gentilly, Grignon, Guespelle, Issy, Meudon, Pallery, Sevres, Vaugirard, &c., in the lower beds of the coarse limestone. p. 47.

Planorbis shells, analogous to the recent species of lakes and marshes, are found at Romainville, in a white and friable calcareous mass above the gypsums; also, at Trappe near Versailles in the husbandry marle, which though very decomposing, resembles a hard siliceous limestone when fresh dug. p. 51, 56 and 57.

———— coarse siliceous shells are found at the tops of the hills at Longjumeau, in a sand (which is probably alluvial). p. 57.

Polypiers of five or six different sorts are found in the chalk strata. p. 42.

Porpytes

Porpytes of different kinds are found in the chalk strata. p. 42.
——— at Chaumont, Gentilly, Grignon, Guespelle, Issy, Meudon, Pallery, Sevres, Vaugirard, &c. of different species from those in the chalk, are found, in the lower beds of the coarse limestone. p. 47.
yrulæ shells are found at Chaumont, Gentilly, Grignon, Guespelle, Issy, Meudon, Pallery, Sevres, Vaugirard, &c. in the lower beds of the coarse limestone. p. 47.
uadrupeds, skeletons of, at Antoni, Bagneux, Chelles, Clamart, Grisy, Montmartre, Triel, Valerain-mount, &c. of several kinds, are found in the upper gypsum mass. p. 51 and 53.
———, the skeletons of various large ones, are found in the (valley) alluvium of the basin of Paris. p. 38.
ea-shells (resembling recent genera or species of such shells), viz., cardiums, cerites, mactres, oysters, venuses, and other bivalves, are found in the marles that are below the argillaceous sand at the top of the gypseous formation. p. 52.
——— of various kinds abound in the lower beds of the coarse limestone at Chaumont, Gentilly, Grignon, Guespelle, Issy, Meudon, Pallery, Sevres, Vaugirard, &c. p. 47 and 54.
hark's Teeth, see Teeth.
olenæ shells (omitted in the translation), at Chaumont, Gentilly, Grignon, Guespelle, Issy, Meudon, Pallery, Sevres, Vaugirard, &c. in the lower beds of the coarse stone. p. 47.
pirorbis, a species of these shells is found in the chalk strata. p. 42.
talks of vegetables, see Leaves.
eeth of Squali are found in the chalk strata. p. 42.
elline shells of different species are found at Chaumont, Gentilly, Grignon, Guespelle, Issy, Longjumeau, Meudon, Montmartre, Montmorency, Pallery, Romainville, Saint Prix, Sevres, Vaugirard, &c., in the lower beds of the coarse limestone. p. 47 and 54.
—— small, long, white, and striated, are found in vast numbers in seams, close to each other, in a stratum that covers the building-stone rock of the coarse limestone formation at Gentilly, Saint-Germain, Villepreux, &c. p. 48.
—— small and elongated, closely packed in a very thin seam, below the thick green potters' earth of the gyp-
seous

seous formation; which thin seam can be traced in the quarries, over a space of 27½ miles long and 11 miles broad. p. 52.

Terebellæ are found at Chaumont, Gentilly, Grignon, Guespelle, Issy, Meudon, Pallery, Sevres, Vaugirard, &c., in the lower beds of the coarse limestone. p. 47.

Terebratula, see Anomites.

Thornback fish, fragments of the bones of, found in the yellow argillaceous marle, above the thick green potters' earth of the gypsous formation. p. 52.

Tortoise bones are found at Dammartin, Montmartre, Montmorency, &c., in the gypsum or intervening marles, of the upper gypsum mass. p. 51.

Trees, trunks of, large palms (apparently), converted into silex, are found lying flat in a white and friable calcareous stratum, above the upper gypsum mass. p. 51.

——, large trunks of, of unknown species are found at Seran, &c., in the putrid alluvium (of the valleys). p. 58.

—— see Wood.

Trochi shells are found in the yellow argillaceous marle, near the upper part of the gypsous formation. p. 52.

Vegetables, see Leaves.

Venus shells, roundish, are found at Gentilly, Saint-Germain, Villepreux, &c., in the building-stone rock of the coarse limestone formation. p. 48.

———, sometimes in fragments, are found in the yellow argillaceous marle near the top of the gypsous formation. p. 52.

Vermiculites, of three species, are found in the chalk strata. p. 42.

Wood, bituminous, at Rolleboise, by the Seine river, in the potters' or plastic clay formation, in which no other organized remains have been found. p. 44.

—— siliceous or petrified, at Longjumeau, on the tops of the hills in (alluvial) sand. p. 57.

——, see Trees.

In order to supply, in some degree, the want of vertical Sections across the basin of Paris, in some determinate directions, best adapted for exhibiting the thicknesses and relative positions of its several strata; (which sections, it is to be hoped, will accompany the promised Memoir of M. Cuvier and M. Brogniart); I have, in the mean time, attempted an arrangement from their abridged Memoir, which, perhaps, you may deem worth a quarto page, to enable it to be folded out at the end of this Letter. In this table, I have separated the IXth and Xth soils or formations by a double line, to mark the distinction between them and the remaining eight soils; which our authors seem to consider as composed of regular or undisturbed strata, that is, such as exhibit no marks of violent attrition or mechanical mixture, as the alluvial soils almost invariably do; and they, besides, never underlay regular strata.

I shall now, I hope, be excused in offering freely, a few of the observations which have occurred to me, in thus analysing the hasty but valuable Memoir of our able and industrious neighbours on the continent; and comparing it with what I know of the terrestrial stratification, and which is indeed limited in a great measure to the British Islands.

England presents us with the opportunity of examining the out-crop, or appearance on the surface, of a vast succession of strata, among which the fourth, or lowest Derbyshire limestone, is perhaps three miles of perpendicular depth, below the part of the chalk strata, which are described as the lowest that have been seen or reached in the basin of Paris: for it was ascertained by Mr. *William Smith* several years ago, that 200 feet or more of the lower part of the chalk strata are entirely without layers or nodules of flint; and that some of these beds contain large cornu-ammonis, nautili, and numerous other shells: this he has proved by an actual examination of very near 700 miles in length, of the basset or out-burst of these lower and hard beds of chalk, without flints, reckoning from the Isle of Wight to the westernmost points of the chalk in Dorsetshire and Somersetshire, and thence for the north-east angle of Norfolk; through Lincolnshire, and Yorkshire to Flamborough-Head; and including the edges of our great southern denudation * from Beachy-Head in Sussex, to near Petersfield and Alton in Hampshire, and Farnham in Surry; and thence again to Hythe in Kent. And here it may not be amiss to observe, that before I knew Mr. Smith, he had ascertained the peculiar fossils that belong to several of the thin strata, underlaying the whole extent of the edges of this vast mass of chalk, in the same manner as the Paris strata overlay it, had collected a series of specimens of each fossil, marked and brought together from as many places; and was well entitled, and did in fact often exclaim, though perhaps, in

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* This denudation is shortly mentioned in the Cyclopædia, article *Coal*, and is further exemplified by a manuscript section of the strata between London and Brighton, made by me in 1806, which is in the hands of many.

other

I have referred, as well as recollected by Mr. Smith's pupils and acquaintance, that he considers and teaches, that the western edges of the several strata on the south and eastern parts of England, are their natural *endings*, and that such strata never did proceed further towards the northwest than they do at present, and that all the strata are not entire or perhaps ever were, some having large and deep holes or patches wanting within their limits, as well as in their deeply fingered or indented edges; while other strata, particularly about Bath, have detached parts beyond their continuous mass forming hummocks, or capping the adjacent western hills. These facts or appearances of the strata have accorded, generally speaking, with the several English districts which I have of late years examined; but many of the minuter circumstances attending the holes or denudated patches in the strata, which have been more particularly the object of my researches, such for instance as the strata around and under these denudations, *rising on all sides* towards some central point or ridge; their edges being so often abrupt and straight, like fractures rather than the rounded endings of strata; these denudations, as far at least as observations extend, being all elongated in a marked manner from SE. to NW., or very nearly so; and others, besides those which I have elsewhere mentioned as the characters of *denudation*, and above all, the vast extent to which I find these denudations extending, in the southern and middle parts of England, have occasioned me to hesitate considerably, as to the real distinctions, between the natural endings and the denudated edges of our strata: and it should seem, that the idea so natural and general among neptunists, of the strata having been originally level and *concentric*, may, by extending the magnitude of the denudations (to which no limits have yet been assigned), account for all the external forms of the English strata, and perhaps for the admirable form of its valleys, although I may again invite the sarcastic lash of a certain class of writers, in frankly owning that I am unable to guess, much less to describe the mechanical operations by which this has been brought about; and in referring it *to the finishing and all-wise operations of Creative power, on the mass of the earth*, or old world, as our authors have denominated it; which had been the theatre of such a long and astonishing series of the creation and extinction of animated beings, during the accumulation of its matter, for purposes which are perhaps to us inscrutable.

Whether the isolated hillocks or hummocks of gypsous d other strata, which our authors have described in the sin of Paris, are the detached endings of similar and nnected strata, that lay to the south and west of Paris, d may either be concealed by vast alluvial tracts (p. 40), or ly occupy countries where nice observations have not yet en made; or, whether such hummocks be occasioned by egular or compound denudation[*], such as I have noticed veral instances of in Derbyshire, and as Mr. Smith has served about Bath, and Dr. William Richardson in the rth of Ireland[†], it is of course difficult to determine, d its discussion must be deferred, until a numerous train facts are better understood.

Our authors, it will be seen, recur to a local inundation lake of *fresh-water*[‡] for explaining the deposition of e matter composing their gypsous hillocks: to me, however, much better evidence seems wanting, of the probable istence of such a *lake* or local pool of fresh-water than ey offer, and particularly of the several successive *fresh-* d *salt-water* inundations which they mention, at pages and 55. Has it, or can it be proved, that fish nearly or actly resembling in the genera, or even the species, those at are *now* peculiar to either *fresh-* or *salt-water*, may not ve had other powers and habits in the *old world?* and at all the animals and even birds hitherto known of the imitive creation, and its vegetables also, may not have en adopted by their great Author, to a sub-aqueous exence? And again, Who has or perhaps can, determine, ether the fluid surrounding the earth during its formation ich I have called aqueous above, was either fresh or lt, in the sense that we now use those terms? Does it not em more probable, that this fluid varied essentially from ne to time in its nature and composition, and which perps occasioned the extinction of the beings adapted to its ior state, as well as the alternations of the strata, especially, if the matters of the strata were ever in chemical lution in it, as our authors seem to imply (p. 51), in speaking of the gypsums being crystallized from the waters of their Parisian lake?

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[*] Perhaps the basin of Paris may lie within the southern verge of the eat south-eastern denudation of England, that reaches far into Hampshire, d has apparently stript off all the chalk strata, from near Alton to Bologne the French coast.

[†] See our 33rd volume—EDIT.

[‡] It seems improbable that gypsum should be a produce of fresh-water, en we reflect, that the red soils or gypsous marles of England produce th sea salt and gypsum in vast abundance, and that salt rocks and springs e the common accompaniments of gypsum in various other parts of the rld: we are not told, whether there are any brackish or salt springs withthe basin of Paris.

The candid confession of the essential differences between the *seas*, the *fresh-waters* and the *alluvium* of the old world, and the seas, fresh-waters and alluvia of the present world, made by our authors, in the concluding paragraphs of their IIId, VIIIth, and IXth articles, do them credit, and give some weight to my suggestions above, which are not here made for the first time.

Our authors say (p. 37), that "a strongly marked character of a great eruption proceeding *from the south-east* is imprinted on the forms of the eminences," on which subject it may be proper to remark, that soon after Mr. Smith had commenced his investigation of the British strata, he discovered an important law regulating all the *known alluvia*, or that which consisted of or contained the fragments and reliquia of known strata, were moved *from the south-east* towards the south-west: the matters of any particular stratum being rarely if ever found as alluvium upon that stratum, but such matters are found more or less plentifully on the surface, beyond its western edge: instances are numerous in England of considerable and unbroken masses of soft or clayey known strata, being moved many miles and lodged in the alluvia; stones of large size thus moved, and scarcely at all rounded, are exceedingly common in some districts, and such are often lodged on the highest hills, but the most common state of the native alluvia, is in small water-worn and mixed fragments: numerous hills in Bedfordshire of considerable extent are thus formed or raised higher, by alluvia of the chalk strata and those which cover it, including a vast variety of limestone bolders that are full of shells, many of which, if properly examined, would I suspect agree with the coarse limestones of the basin of Paris: my researches in the midland counties, have detected many isolated patches of exactly similar alluvia, particularly on the height NW. of Leicester town, where an immense cap of alluvial clay, so abounds with hard chalk and these bolders of limestone, that at Burstal Cliff-house on the road to Thurnby, they have been dug for burning lime: the covering matter at Chellaston gypsum quarries S. of Derby, is of this same clay; but lime-bolders do not there so much abound, as they do near Leicester and thence in isolated patches towards Market Harborough, forming there the principal material for repairing the public road. The above with others are indications I think of vast tidal currents which have swept over all the surface

om SE. to NW., since or at the time, that the deposition
regular strata ceased, as observed p. 37.

It struck me, as among the curious circumstances of the
sin of Paris, that the strata throughout it are horizontal,
least only one instance to the contrary is mentioned
. 43), and that faults or such like derangements of the
rata are nowhere mentioned.

All the southern parts of Derbyshire and part of Lei-
stershire adjoining, abound with red earth or gypsous
arle (entirely without fossils I believe) that is in like
anner horizontal throughout, with three exceptions only,
at I could discern: viz. at the S. of Burton Trent bridge
the E. side of Catton in Croxall, and NW. of Stretton-
fields; the second of these only has any considerable
clination: although tilts of the strata are so rare in our
psous strata, yet many faults seem to occur in them: a
ost tremendous one forms the northern limits of the gyp-
us marle for 40 or 50 miles, and four or five isolated lime
d coal districts within it, seem surrounded by great faults
d to be intersected by numerous others, occasioning as
clining and dislocated strata at Breedon, Cloud's hill,
easham, &c. as can almost any where be found.

It is stated by our authors (p. 54 and 55), that their
h and VIth soils are placed side by side of each other:
these be really different soils, and not the same somewhat
odified in their substances, the fossils in one of them
so not being preserved, (of which I entertain some sus-
cion) a fault must, in all probability, range between the
o places where these soils have been noted, which ele-
tes one of the soils or depresses the other. I would also
re suggest as a query, whether their VIIIth soil be
t a similar repetition of part of these same soils? Its
sh-water shells are not sufficient, I think, to prove it an
per soil; and in the latter part of the Memoir we are not
esented with the same evidence of the superposition of
e different soils as we are with respect to the first five,
ich may however have arisen from the haste in which
e Memoir seems to have been prepared.

I do not remember to have seen any where, the appear-
ce of limestone strata upon the chalk in England, or to
ve heard of any such from Mr. Smith, or otherwise: of
psum I think we may safely say that there are no strata:
t crystals of selenite abound in the London clay, and
ere are numerous patches of plastic clay, used in some
aces on our chalk hills for pottery and brick-making,
hich seem to answer pretty well to the description of

some of that within the basin of Paris; but we cannot I think infer, that this same plastic clay underlays the London clay, in the neighbourhood of London, as there is generally a sand here immediately upon the chalk, but which varies greatly in thickness, from many yards (as in the sand-pits by the Thames between Greenwich and Woolwich) to a few feet only or perhaps inches, in the borings of some of the modern wells in and near the metropolis: still however this sand has always I believe been found in this district, and it has generally been forced up with great violence into the well, by the water issuing from the joints in the chalk beneath, on first pricking this spring with the auger, or on any after occasion when the column of water in the well is much and suddenly diminished, as often happens by the pumping at Meux's brewhouse and other places.

Mr. Smith and myself have been used to consider the London clays as regular strata, and the layers of small regular flattened chert-stones which it contains, as nodules peculiar thereto, and not as *rounded pebbles* and indicating an alluvial origin to these clays: I thought these conclusions well warranted by the regular beds of which this clay in many places consists; its regular layers of ludus helmonti; the uniformity in the shape of its chert pebbles, many of which appear concentric in their structure, and by their having an external coat or covering, which is nearly similar in all of them: but an able student under Mr. William Smith, of the same standing with myself, Mr. Benjamin Bevan of Leighton Buzard, Beds., whose situation as engineer to the Grand-Junction Canal Company, has for some years past given him better opportunities of examining the northern edge of the London clays than I have possessed, has for some time held the clays and sands above the chalk to be alluvia, and produces instances of chert pebbles taken from their bed on Rislip-common, Middlesex, in a pit where sand containing layers of these pebbles has chalk in its bottom, to prove these pebbles to have been rounded, and even some of them to have been broken and since partially rounded (which is perhaps the most unerring test of rounding, that we meet with). It has also been suggested by this gentleman, that the peculiar dark-coloured and uneven coat of these pebbles, is occasioned by a partial decomposition which they have suffered, since their rounding, the effects of which are also visible in the concentric stains within many of them, which give them so very nearly the appearance of original nodules. In candour I ought also to state, that the accounts which I have collected of the sinkings

inkings of various new wells, (on some of which I have
een consulted) near the metropolis, have exhibited less of
egularity in the succession and thicknesses of the strata,
han I had been led to expect: also, that at Alford in Lin-
olnshire, where numerous very shallow wells are sunk and
oles in them bored, through this clay and the sand under
into the chalk, from whence the water rises and perpe-
ially overflows the surface, I found this clay and sand
nuch thinner than it usually is about London. It is my
ish, to suspend entirely my opinion on the above interest-
ig questions, until many more facts are collected, and
specially until the situations of our principal assemblages of
ssil shells above the chalk at Hordel, Reading, Woolwich,
the shell marles of Suffolk, &c. &c. are ascertained, and
ey have been examined and minutely described by a com-
etent conchologist, and such descriptions have been care-
lly compared with the drawings and details by our author
nd M. Lamarck: an undertaking which I wish much to
ress on the immediate attention of the societies above
entioned.

If no part of the chalk be elevated more than 50 feet
ove the Seine (p. 43), it should seem probable, that the
0 chalk fossils mentioned (p. 42) belong to about only
ne-eighth of the whole thickness of the chalk strata, at
s top.

In commencing a very extensive Mineralogical Survey,
ke Mr. Smith's manuscript Map of England, Wales, and
art of Scotland, it will perhaps be best to follow his exam-
e, in selecting only such strata as usually form *distinct
nges of hills* through the country, by their bold or sudden
dings, to form his classes or principal assemblages of
rata, that are to be distinguished each by a different co-
ur in his Map and Sections, without much regarding the
ineral characters, or characteristic fossils of the several
in strata that compose each of them, leaving these to be
umerated and described in written details and in local
ctions on a large scale. In commencing more local, but
t considerable surveys, such as my square of map includ-
g Derbyshire, or the basin of Paris, the bold endings of
rticular strata ought I think still to have a principal share
determining the selection of such as are to have a differ-
it colour assigned to them; the other considerations should
the width or extent of surface which is made by the se-
ral strata, and the distinct mineral characters of particular
ick strata or beds: a few thin beds or strata which hap-
n to have very striking characters either in their appear-

ance, economic uses, or conspicuous fossils, may have a colour assigned them, especially if such fall between, and not so as to subdivide thick masses, selected for separate soils, or genera on the principles above.

In coal-districts or others, where grit or limestone alternate with argillaceous strata, it will be found right perhaps, to select all the thick masses of rock, which are sufficiently separated by clay-shists and other argillaceous strata, to have different colours assigned them, either using some general colour for the clays formed on the surface by all the argillaceous strata, or assigning a colour to each of these argillaceous assemblages, according to the number of the strata and thicknesses of them, so that the map be not over crowded. The transitions from light to heavy or dry to wet land, made by the above two classes of strata, are the best defined characters that we meet with in mineralogical surveying; and are indeed almost the only ones by which the farmers and occupiers of the lands can give any assistance by their information, except while pacing over every part of the surface with the surveyor, in answer to his questions, as they may arise.

The plan which M. Cuvier and M. Brogniart seem to have followed, in assigning colours to their soils (p. 41), and dividing some parts of the Paris strata into their ten formations or soils, seems to me much inferior to the above; and in particular their sea-sands, freestone without shells, and fresh-water soils, appear calculated to lead to no useful result, but rather to mislead. The more popular and obvious the divisions of the strata are made on mineralogical maps and sections, the sooner will the geological facts they convey be understood and rendered useful; for after all that can be said, they must, in the present state of our knowledge at least, be but arbitrary classes or divisions, exhibiting the order of super-position, principally.

The freedom of the remarks which I have ventured to make on the opinions and writings of geological observers of such high celebrity, will I trust in candour be ascribed to their true motives,—a desire to render justice to a valuable friend and to our country, and to advance and perfect the science, which of all others seems to me to claim our serious cultivation and attention. An apology may perhaps be necessary to your other correspondents and readers for the great number of your valuable pages which I have occupied.

I am, sir, Your obedient servant,

JOHN FAREY.

February 12th, 1810,
No. 12, Upper Crown-street, Westminster.

X. *Report on a new Navigable Canal proposed to be cut from Okeham to Stamford, and from thence to the Town of Boston.* *By* THOMAS TELFORD, *Esq.*

THE gentlemen of Lincoln and Rutland having for some me been turning their attention to the discovery of the est means for extending and improving the benefits al- ady resulting from inland navigation, in such a manner iat the greatest local advantages to the town of Stamford ay be united with the general increase of the agricultural nd commercial prosperity of a large district of surround- g country, and having employed Mr. Telford to take a iew of the different lines of country embraced by their lan, have received the following

REPORT.

“ Having, in compliance with the directions of the ommittee, carefully examined the districts of country, ex- nding from Stamford westwardly to Okeham, and east- ardly to Peterborough, Spalding, the Foss-Dyke, and oston;—I shall state my ideas with regard to the sundry nes of inland navigation, which appear best calculated promote the most perfect intercourse, and, consequently, e general prosperity of the country.

“ The whole of Leicestershire being already intersected om north to south by an inland navigation, and also om west to east by a line of canal, passing by Melton Mowbray to Okeham in Rutlandshire; there now only mains to be considered and determined, the most ad- iseable mode of proceeding from the last point, eastwardly, the ports situated upon the great bay or inlet, called the Wash; and thereby opening a direct and commodious ommunication with a point on the east coast, nearly cen- rical between the Trent and the Thames.

“ The town of Okeham, at which the last-mentioned anal terminates, being situated upon the summit of the idge of land which occupies this part of the country, nd from which the adjacent streams have their course to he river Welland, affords an opportunity of choosing a ine of canal, either down the river Wash, or Guash, which alls into the river Welland, about a mile and a half below Stamford bridge; or down the river Chater, which unites with the Welland about two miles and a half above Stam- ord.

“ The river Wash, occupying the valley nearest to the

[To face page 140.

An arranged Table or Section of the STRATA in the Environs of PARIS. in the relative Positions in which

the town of Okeham, has induced Mr. Whitworth, in his survey, to follow that stream to its junction with the Welland; but by that line, though sufficiently regular in its descent, being obliged to skirt the north side of the high ridge of land which lies to the north of Stamford, an awkward circuity is created before it can reach the town, so that the distance by the navigation between Stamford and Okeham would be nearly nineteen miles.

"In proceeding from the sea-coast to the interior of the country, the vessels navigating that line must either pass at the distance of about one mile and a half from Stamford; or having come up to the town, must return the same distance, along the same line, to get into the valley of the Wash.

"These circumstances, in my opinion, render that line objectionable, and inferior to another line which may be obtained by means which Mr. Whitworth has himself partly pointed out. This is by continuing the head level from Okeham over the south field, along the before-mentioned line, about a mile and a quarter from Okeham; and from thence, instead of locking down the Wash valley, to continue through Egleton into Gunthorpe, and there locking down to reduce the embankment across the Wash valley, so that the cutting through the ridge, at Martinsthorpe, shall afford earth sufficient for its construction. This will enable the line to be carried into the Chater valley, without being encumbered with a tunnel, which Mr. Whitworth, from a cursory view, apprehended necessary. The valley between Gunthorpe and Martinsthorpe will also be a convenient place to receive the feeder from the reservoir proposed to be formed at Braunston.

"This line, having crossed the ridge at Martinsthorpe, should be locked down nearly to the bottom of the valley of the Chater, and be carried down the north side to near Ketton, where, in order to avoid the village, it should cross to the south side of the valley. After passing the village, it must be again brought to the north side, and be continued to the most favourable point for crossing the river Welland, below where the Chater has fallen into it. After crossing the Welland, the line should be carried along the skirts of the wood, as nearly as possible, in the division between the uplands and the meadows, and it should fall into the river above Stamford bridge, in the most convenient way, to enable wharfs to be formed on each side of the river. For accomplishing this object the opportunities are ample, without interfering with buildings. I prefer the south bank of the river Welland, from near the junction of the Chater, because it is more favourable ground for a canal than the north bank, which is composed of loose rock; and because, if the proposed line to Harborough be executed, the last-mentioned two miles and a half would answer the purposes of both navigations. The length of the line between Stamford and Okeham, as nearly as I can at present make out, would not exceed fifteen miles, being shorter than the line laid down by Mr. Whitworth by four miles. Besides that this line arrives at Stamford from Okeham by a shorter distance than the other, it will be more satisfactory to the principal land-owners in the county of Rutland; it bears more equally upon the general population of that county than the other does: and it will for ever fix, more directly, the intercourse by inland navigation through the town of Stamford.

" Proceeding from Stamford towards the sea coast, it will be necessary to continue upon and improve the present navigation of the Welland to the second lock, being a distance of about three miles; but from thence to the sea, it will cost more to render the old navigation perfect, and acquire a proper outlet to the sea, than will construct an entirely new canal navigation to the town of Boston.

" I am therefore of opinion, that, at or near the second lock, a line of canal should depart from the north bank of the Welland, and,—passing along a line between the uplands and the meadows to the westward of Tallington, and immediately eastward of Barholm, to the west of Kate's Bridge,—it should enter the Car Dyke, along which it should be carried, until that ancient work approaches the South forty-foot drain; and opposite Billingborough, or Horbling, and then should proceed along that excellent drain, to the town of Boston.

" From the Welland to Car Dyke, an entirely new canal must be formed. Its passing, as much as practicable, between the uplands and meadows, will be favourable to the adjoining properties, and afford good ground for the necessary works. Along the Car Dyke, in many places, it has been preserved as a drain, and will become a part of the proposed navigation. It will then also form a more perfect boundary to the fields which have always been separated by it, and when they are accommodated by necessary bridges, the adjacent properties will be much improved. Along the South forty-foot drain, with the exception of deepening its bottom from half a yard to two feet, and widening the lock at Boston, to suit the breadth and draft of vessels which navigate the Leicestershire canals, very little more is required to render it an excellent navigation.

"By the canal line being carried along the Car Dyke, it will not at all interfere with the navigations of the rivers Glen and Bourne, because it passes above the places where those navigations terminate: and as the waters of those rivers will be passed under the canal, no apprehensions can be entertained that their usual supplies will be lessened.

"The river Welland cannot be injured, because, during winter and rainy seasons, the superabundance of water will be more than sufficient for all purposes: and, in dry seasons, as the supplies of water for this navigation are proposed to be drawn from reservoirs situated near the summits of the country, they will be passed through locks in the upper country, which are at least double the depth of those in the lower country, so that unless there is more than double the quantity of business in the Fens, to what is carried through the upper country, no additional water can be required. Besides, the leakage from the upper locks will be much more than from the lower ones, consequently the surplus water must fall into the Welland.

"The South forty-foot drain proprietors will have no apprehensions of too great increase of water, in their drain, when they consider the interest the canal proprietors will have to preserve their water, by constructing very shallow and perfect locks, and by adding to this, the great extent of surface each lockfull has to spread over, and the regulation which will constantly be taking place by the lock at Boston.

"The conservators of the port and haven of Boston, besides the certain prospect they will have of increasing the prosperity of the place, must be sensible of the evident advantage of deriving additional supplies of water to assist in scouring out and maintaining the bed of the river between Boston and the sea.

"The communication between Stamford and Boston, by passing along the line of division between the Upland and Fen countries, where the most populous market towns and villages are situated, will (exclusively of the thorough trade) be of great advantage to all the district of country through which it passes, and afford adequate tonnage dues in return; so that, upon the whole, this appears to be an improvement which will be generally beneficial, and will interfere, as little as possible, with any established rights.

"In order to render the inland navigation of this distr-

ict of country, and the connections with the interior
tricts more perfect, and to afford a fair competition of
al advantages, I am of opinion that a canal should be
ried between the Welland and the Nene: and the coun-
appears to be particularly favourable for this junction.
is line should depart from the Welland precisely where
branch to Boston does, and in a manner similar thereto.
should be carried in a line dividing the upland from the
country, and terminating at or near Peterborough. By
se means, the elevation would be small; and proper
und would be obtained for the canal works: and the
al, being supplied with waters, which now pass partly
o the Welland and partly into the Nene, those waters
uld be turned by lockage to the respective rivers, so as to
ure neither. The country through which the canal would
s is very populous, and requires communication: and
towns, and whole population of the valleys of the Wel-
d and the Nene, would thereby have an opportunity af-
ded them to benefit by the navigation of all the rivers
ich fall into the great bay—with the choice of such of
m as should best suit their interest and conveniency.

'Having, I trust, stated satisfactory reasons why the
mer surveyed line of canal should be abandoned, and
ing hitherto been enabled to recommend other lines only
m a general inspection of the country, the committee
l readily conceive, that, until regular and careful surveys
l sections have been made of the new lines, it is im-
sible for me to enter into a more minute detail, either
h regard to the precise situation of the lines, or the na-
e of the works required, or to form any correct estimate
the expense. But if it be judged adviseable, after this
eral explanation, to authorize me to proceed in getting
se surveys and sections made, no time shall be lost in
forming the service, and furnishing the committee with
necessary data for making an application to parlia-
nt.

'In the mean time, I may venture to state, that although
m the quantity of lockage necessary to ascend to the
als, already made upon the summit of the country, the
ense of the line from Stamford to Okeham will be
ly equal to the general average of canals of similar di-
nsions; yet those from Stamford towards Boston and
erborough, from their small elevations—the favourable
ure of the ground—and from having, in the South
ty-foot drain, about fifteen miles of canal nearly com-
te, as well as great facilities in the Car Dyke, will be
greatly

greatly under the general average of expense; so that, upon the whole scheme, (embracing an inland navigation of from 60 to 70 miles,) taking into view its extensive connexions, there appears a fair prospect of ample remuneration for the adventurers. **Thomas Telford.**

Stamford, 8th Jan. 1810.

In consequence of the foregoing Report, it was resolved, at a meeting of the committee held at Stamford on the 8th of January, 1810,—That it appears to this committee to be impracticable to go to parliament in the now ensuing session for the sanction of the legislature to the plan which Mr. Telford has proposed, inasmuch as the necessary surveys cannot be in due time prepared, nor the notices given, nor the plans delivered, which are required preparatory to the introduction of navigation bills into parliament: but that Mr. Telford be directed to take the steps proposed in his report, and that every other proper measure be pursued for the introduction of the bill into parliament in the session following.

XXI. *On Crystallography. By* M. Haüy. *Translated from the last Paris Edition of his* Traité de Minéralogie.

[Continued from vol. xxxiv. p. 466.]

If these ridges were subject to a different law, which gave rise, for example, to subtractions of two ranges, the sign would become $\overset{1}{D}\overset{2}{e}\overset{0}{E}P\underset{1}{B}\underset{2}{b}$. According to this it has been considered as settled, that the decrements represented by a large letter accompanied by any cypher would not implicitly contain similar decrements represented by the small letter of the same kind, or vice versa, *i. e.* for example, that $\underset{2}{B}$ would not implicitly contain $\underset{2}{b}$, or, vice versa, that when the second letter would not enter into the expression of the sign with a different cypher, we should not use the same cypher accompanied by a zero. In the first case each of the two letters expresses a decrement which is peculiar to the ridge or to the angle which it indicates; in the second, that which is affected by a zero shows that the angle or the edge to which it exclusively relates undergoes no decrement. Thus in the sign $\overset{1}{D}\overset{2}{e}\overset{2\,1}{E}P\underset{1}{B}\underset{2}{b}$, B expresses a decrement by one range, which only takes place on the ridges contiguous to the upper summit A (fig. 73); $\underset{2}{b}$ indicates a decrement by vo ranges, which only acts in the same way on the ridges ntiguous to the lower summit. Finally, the quantities *e* nd E ought to be thus considered independently of each ther; the first as expressing a decrement by two ranges n the angles *e* solely, and the second as indicating zero of ecrement on the angles E opposite to the foregoing.

I have enlarged on the detail of the principles of the me- od, in order to leave nothing to be desired, if it were possi- e, of what can be of use in enabling my readers to have a ear idea of the art, and put an observer in possession of e method of instantly representing a secondary crystal of given form. But if any person confined himself to the mple comprehension of the signs employed in the system, nd was only anxious to read without being able to write em, he would only require some simple and easily under- ood rules, which we shall here succinctly explain:—they ill form a kind of recapitulation of the preceding details.

1. Every vowel employed in the sign of a crystal designates e solid angle marked with the same vowel on the figure hich represents the nucleus; and every consonant indi- ates the ridge which bears this same consonant, or the ce the middle of which it occupies.

2. Every vowel or every consonant is accompanied by a ypher, the value as well as the position of which indicates e law of decrement which the corresponding angle or edge ndergoes. We must except the three consonants P, M, T, ch of which, when it forms part of the sign of a crystal, dicates that this crystal has faces parallel to that which ears this same letter.

3. Every letter comprehended in the sign of a crystal is arked below with the cypher that accompanies it, on all e angles or all the edges which perform the same function ith that which on the figure is marked immediately with e letter in question.

4. Every number added to a letter indicates a decre- ent, the angle or edge of which marked with this letter is e term of departure. If the number be entire, it indicates ow many rows are subtracted in breadth, with the condi- on that every lamina has only the thickness of one mole- ule; if the number be fractionary, the numerator makes nown how many rows are subtracted in breadth, and the enominator how many are subtracted in height.

5. According as the number is placed below or above e letter which it accompanies, it indicates that the decre-

ment descends* or ascends, setting out from the angle, or from the edge marked with this letter. If it be placed towards the top and to the right or left of the letter, it designates a decrement which takes place in the lateral direction to the right or to the left of the angle which bears the same letter.

6. When a letter is found written twice successively with the same cypher placed on two different sides, such as $^2G\ G^2$, or $G^2\ ^2G$, $^2A\ A^2$, or $A^2\ ^2A$, the two edges or the two angles which it designates, ought to be considered on the figure according to the same relative positions, *i. e.*; for example, as in the sign $^2G\ G^2$, the quantity ^2G indicates the effect of the decrement on the edge G situated on the left, and the quantity G^2 the effect of the decrement on the edge situated on the right.

7. When a letter bears the same cypher repeated both on the right and left, as $^3G^3$, it is applied indifferently to any one of the ridges G which it designates. It is the same with the letters which belong to the angles.

8. The parenthesis in such as $(\overset{3}{O}\,D^1\ F^2)$ designates an intermediary decrement. The letter $\overset{3}{O}$ expresses in the first place, that the decrement takes place by three rows on the angle O, and that its effect is ascending. $D^1\ F^2$ make known that for one ridge of molecule subtracted along the side marked D, there are two ridges subtracted along the side marked F.

9. Every small letter comprehended in the sign of a crystal, indicates the angle or edge diametrically opposite to that which bears the large letter of the same kind, or the figure in which the small letter in question is omitted as superfluous. We must except the letter *e*, which is always employed on the figure of the rhomboid, and which indicates, according to the principle, the angle opposite to that which bears the letter E.

10. When a sign contains two letters of the same kind, the one large and the other small, with different cyphers, the two opposite edges or the two opposite angles to which these letters answer, are considered as each undergoing exclusively the law of decrement indicated by the cypher added to the letter.

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* We only allude here to the general progress of decrements, to which the particular cases refer that seem to form an exception. For instance, if the decrement be produced by one row on the angle at the summit of a rhomboid, then the face produced will be horizontal. But this decrement comes within the description of those that are descending, and of which it is as it were the limit.

11. Every letter, whether large or small, marked with a cypher which has a zero after it, shows that the decrement indicated by this cypher is null on the particular angle or edge to which this letter refers.

We have omitted the applications which would be necessary for understanding these rules, if they had been presented on our first setting out. These applications are found already in the detailed explanation, which we have previously given, of the principles of the system, and the perusal of which is presumed to have preceded that of these same rules.

OF INDETERMINABLE CRYSTALLIZATION.

When the crystalline molecules disseminated in a liquid experience obstacles which affect their tendency to reunite in conformity to the laws of their mutual affinity, the forms which result from their aggregation have no longer that regularity which belongs to an exact and precise determination. Their ridges are obliterated, their faces are curved, their pyramids are sharpened. Hence the crystals called *lenticular*, or which imitate the form of a lentil; *cylindroids*, the prism of which is rounded off; *acicular*, or similar to needles; &c.

If a multitude of small indeterminable crystals are so intimately connected with each other that they form only one body, we then consider this body as a particular being, and hence the substances which we call *striated*, *fibrous*, &c., and which are formed by the junction of an infinite number of crystalline needles, sometimes parallel, sometimes divergent, and at other times crossing in different directions.

Lastly, The appellation *amorphous* has been given to substances which present, as it were, the last degree of confused crystallization, and the vague and indefinable form of which is, as it were, *mute* to the eye of the observer.

Of Concretions.—The formation of the bodies which we have hitherto mentioned, particularly of crystals properly so called, essentially depends on two conditions only: one of which is, that the molecules of these bodies should be in the state of integrant molecules; and the other, that they should be kept in suspension in a liquid capable of abandoning them to the attraction which solicits them towards each other. In short, every thing is regarded as passing in the same manner as if, the force of gravity being null, the liquid was not coerced by the sides of any surrounding matter, and as if the crystal itself remained isolated

lated in the liquid, without having occasion to be supported.

This is not the case with the bodies which we are now about to consider. The modifications which they present are owing to certain local circumstances, such as points of attachment, props or moulds which influence their form. We unite all these modifications under the common denomination of *concretions*, which in the ordinary acceptation signifies a *congealed or fixed substance.*

But in order to fix in a more precise manner our ideas on this head, we shall comprehend under the term concretions, the different bodies, the aspect of which depends, partly at least, on their molecules being in contact with other bodies. We shall now give an idea of the various circumstances which contribute to vary this appearance.

1. *Stalactites.* The water which filters into the fissures of stones situated in the arched part of subterranean cavities, or which oozes through the lax and porous texture of these vaults, arrives at the surface after hollowing out certain stony molecules which are united to it in any way. The drops which remain suspended from the arch during a certain time, undergo a desiccation, which commences on the external surface; and the stony molecules which the liquid gets rid of, exerting their attraction on each other, and attracted at the same time by the side of the vault which they adjoin, form in this place an initial tube, or kind of small ring. This rudiment of tube increases and grows longer by the intermedium of other drops, which succeed to the first, conducting new molecules which the orifice of the tube attracts in its turn. Sometimes this tube preserves the form of a hollow cylinder, similar to a quill. But frequently it increases in size, and is enveloped with concentric layers, the matter of which is furnished by the liquid which descends along the external surface. It then becomes a thick cylinder or cone; and sometimes the molecules hollowed out by the drops which thus flow into the interior of its canal, finish by obstructing it entirely. These different modifications are peculiarly sensible in bodies which belong to carbonated lime.

But a part of the liquid, on falling from the arch upon the ground, forms there other depositations composed of strata generally undulated, or protuberances, the figures of which vary ad infinitum. Lastly, the liquid which flows along the lateral partitions gives rise to bodies, the form of which we might compare to that of a drop of congealed water.

Stalactites are those bodies which are formed in the arch the vault; and *stalagmites* are those which originate in the falling of the liquid on the ground. It is, however, much more convenient to call both *stalactites*, as it sometimes difficult to distinguish between the two kinds formation, when the bodies under consideration have n removed from their original position.

2. *Incrustations.* In the preceding concretions, the aggregation of the molecules depends more especially on the evaporation of the liquid which has flowed over them. [illegible] concretions, which have been called *incrustations*, [illegible] proceed from a kind of precipitation of molecules originally suspended in the liquid. The [illegible] are sometimes deposited on the surface of different [illegible] bodies, particularly on those which belong to vegetable kingdom, and sometimes cover the inside of [illegible] bodies, such as sewers or drains.

When the liquid is introduced into a subterranean cavity small dimensions, where it can remain, the stony molecules incrust the sides of this cavity, which is generally of a [illegible] form, and sometimes end by studding it with crystals. This is what has been called *geode*. Some of these bodies contain a solid and moveable nucleus, or a pulverulent [illegible] matter*. Of this description also are certain pieces [illegible]. Sometimes also the geode is en- [illegible] with a matter which may be distinguished by [illegible] from that of which it is itself composed.

[illegible] that a substance may be incrusted [illegible] nature, by being as if moulded [illegible] instance, we are acquainted with [illegible] carbonated lime incrusted with [illegible] quartzous envelope remains [illegible] from the crystals which it [illegible]

[illegible] There exists a third order of [illegible] pseudomorphoses, *i. e.*, *bodies* [illegible] *figure*; because the sub- [illegible] order present in a very remark- [illegible] forms, which they have in [illegible] other bodies which had re- [illegible]

[illegible] apparent transformation is a [illegible] enough that the shell still covers [illegible] substance, which is as if moulded into its interior*, and then nothing appears simpler than the explanation of the fact, by the introduction of a liquid charged with stony molecules into the cavity of the shell; and this observation leads to a similar explanation of the formation of the kinds of nuclei modelled into shells, which we meet with isolated and stripped of every envelope.

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* [illegible] the term *geode* is derived, i. e., a body [illegible]

Sometimes the shell itself has been penetrated by another matter generally siliceous, which has been substituted for the cartilaginous substance of which this shell had been partly composed †; and it may happen in this very case that the interior of the shell has remained empty. It is no longer, properly speaking, a pseudomorphosis. It is a fossil which has merely become more stony than it was before.

This last kind of modification takes place equally with respect to the bones and to the other solid parts of animals which are found immured in the bowels of the earth; *i. e.*, they may pass to an almost entirely stony state, by the help of a substance which supplies the place of their cartilaginous part.

The case cannot be the same with vegetable productions as with shells. They have no testudo, or envelope, which can exist after the destruction of the interior substance, and serve as a mould to a stony or other substance for receiving an impression of their form. If we supposed that one of these productions, such as a portion of the branch of a tree, were entirely destroyed, so that the cavity which it occupied in the bowels of the earth remained empty, we could conceive that a stony matter might afterwards fill this cavity and there be modelled to it. In this case the new body would resemble externally the branch of a tree; it would have the appearance of knots and wrinkles, but its inside would not present any trace of organization, and it would only be, as it were, the statue of the vegetable production, which it would have displaced.

What is generally called *petrified wood* is a much more faithful imitation of real wood. On a transverse section we distinguish the appearance of concentric layers, which in the living tree must have proceeded from its increasing in thickness; all the principal lineaments of organization are preserved to such a degree, that they sometimes serve to enable us to recognize the species to which the tree belonged which has undergone petrifaction.

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* De l'Isle Crystall. tome ii. p. 161.

† We know that shells, as well as the bones of animals, are formed of two substances; the one calcareous, which is not susceptible of putrefaction; the other cartilaginous, membranous, or fleshy, which may be destroyed by fermentation.

Among the different explanations which have been given this phænomenon, that which seems to be most gene- 'y admitted, although not exempt from objections, con- .s in supposing that the stony matter is substituted for vegetable in proportion as the latter is decomposed; l because the substitution takes place successively, and it were molecule by molecule, the stony particles, in anging themselves in the places rendered empty by the appearance of the ligneous particles, and by moulding mselves into the same cavities, take the impression of the etable organization, and copy the traits of it precisely.

The mineral kingdom also has its pseudomorphoses. e find some substances of this kingdom under crystal- forms, which are only borrowed; and it is probable t, in some cases at least, the new substance has been istituted gradually for that which has ceded its place t, as we suppose takes place with respect to petrified od.

The various pseudomorphic bodies imprint their form the matter which surrounds them, and frequently also impression serves as a cell for an organic substance which imply in a fossil state, or which has received a certain ree of alteration only. This takes place in particular h respect to the ferns and other plants of the same family, form of which is moulded on a schistous matter, as we ll afterwards more fully detail.

We generally denominate *petrifactions* all the variously dified substances which we have mentioned, even those ich only present impressions of animal or vegetable ductions. Daubenton applies this term only to bodies ich, in their natural state, being partly stony and partly tilaginous, such as shells, have become entirely stony.

As we merely purpose to mention a few examples of the difications in question, and not to unite them methodi- 'y under one and the same point of view as several authors 'e done, we shall confine ourselves to the enunciation some of them in speaking of the substances which have med their secondary matter, and shall adopt the no- nclature to this method of classifying.

We ought not to omit that there are also pseudomor- oses, which arise from the substitution of a metal in the m of an organic body. Sulphurated iron presents se- al examples of this kind of metallization.

By referring to all that has preceded, we may define in following manner the different concretions of which have given the description:—

The *stalactite* is a concretion composed of successive layers of a circular or undulated form, which is the effect of desiccation.

The *incrustation* is a concretion in the form of a crust applied to the surface or to the interior of a body. To this we may refer the *geode*, which is a concretion in the form of an envelope, spherical or nearly so, sometimes empty and sometimes containing a nucleus.

The *pseudomorphosis* is a concretion endowed with a form foreign to its substance, and for which it is indebted to its molecules filling a space formerly occupied by a body of the same form.

[To be continued.]

XXII. *On a Hard Artificial Stone that generates a considerable Quantity of Heat during its Consolidation; with the Application of this Fact to the Cause of Volcanic Fires*[*].

THE artificial stone about to be described presents a remarkable example of the great degree of solidity which water, in certain combinations, can acquire.

Water forms more than half the weight in the composition of these stones: the other ingredients are one part of sulphuric acid, and two parts of burnt clay (bricks or earthen-ware) reduced to powder.

The mixture of these substances yields, in fact, a solution of sulphate of alumine: but when in the mixture circumstances are favourable to their reciprocal action, heat is speedily produced, and the quantity evolved is sometimes so considerable as to produce ignition in the mass.

If from 25 to 30 hundred weight of materials be employed, this extraordinary phænomenon lasts for more than an hour.

The most remarkable circumstance is, that if no water be added to the mixture, when the reaction of the substances upon each other is the strongest, the mass, although still liquid, suddenly acquires a great degree of solidity: the heat which it produces is augmented, and the substance afterwards becomes almost entirely insoluble.

This last property being acquired by a mixture calculated to yield very soluble salts, proves that there is a great penetration of the earth by the water and acid, as the whole mass forms a stony composition only.

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[*] From M. de la Metherie's *Journal de Physique*.

The stones here alluded to, although apparently pos-
sing all the properties just described, are not inso-
le: they were prevented from acquiring that state, as
y would then be useless. But as this composition, with
exception of its insolubility, has all the external cha-
ters of the hardest stones, it possesses some claim to
ntion. Might it not, after having been softened by a heat
erior to that of boiling water, be employed with much
antage as a cement, or to cast into models of statues or
es, &c.? Bodies formed of this artificial stone must
protected, however, from the influence of water or
isture.

What also contributes to the interest excited by this com-
ition is, that its analogy with the stones of solfaterra,
the peculiar theory of its formation, will not require
to recur to the hypothesis of subterranean fires, to ac-
nt for volcanic eruptions.

n short, as water, by merely passing in an instant from
liquid to a solid state, develops such a considerable de-
e of heat, may it not be the immediate cause of volca-
eruptions? May it not also be to the slow and progres-
passage of water to the solid state, that the heat found
great depths in the interior parts of the globe is owing?
tly, May not the heat developed in the animal and vege-
le organs be also owing to water?

The above suggestions are thrown out with a view to call
attention of chemists, mineralogists, natural philoso-
rs, and physiologists, to a subject which cannot fail to
ive much light from their concurrent observations.

XXIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

HIS society on the 1st and 8th of February was occu-
d with reading a letter from Mr. Gibson of Manchester
Mr. Thomas, describing an extraordinary fœtus, having
heads, only one body, two arms, two legs, and of
h sexes. There was nothing in the physical organiza-
n of this monster which could apparently have impeded
vital functions; its two heads were perfectly formed
joined side by side, that on the right being masculine
that on the left feminine; both were united to one
ne; and although it had two hearts, only one stomach
was

was found. The nerves from the head on the right side passed to the right arm and leg; those on the left to the left arm and leg; so that, had the creature lived, one head and mind would have directed the right side, and the other the left. The organs of the two sexes were very distinct; and the uterus was found in the bladder of the male. The author described the physical structure of this *lusus naturæ* very minutely, but the details would not be interesting.

Feb. 15. A paper on uric acid, by Mr. Brandè, communicated by the Society for improving Animal Chemistry, was read. The author related the effects of the alkalies and lime on the uric acid and phosphats, in patients labouring under the influence of calculi, but in none of the four cases which he stated were they successful in giving relief or curing the disease. Magnesia, however, had the desired effect, and brought off in the urine great quantities of uric acid and phosphats, in the form of triple salts. The discharge of these salts, after taking small doses of magnesia, was so copious, that the patients were radically or effectually cured in two or three weeks. The suggestion to use magnesia was made by Mr. Hatchet, who knew of nothing so capable of acting on uric acid, and experience has confirmed his conclusion.

Feb. 22. In consequence of the indisposition of Sir J. Banks, Mr. Marsden was in the chair, when the reading of a supplementary paper, by Dr. Herschel, on coloured concentric rings, commenced. The present object of the author is to explain and elucidate the positions laid down in his former papers on this subject, and in some measure to insure to himself more completely the sole merit of discovering the red bow, as Newton did the blue one. The introductory remarks chiefly referred to the 42d and 43d sections of the author's preceding paper, in which the nature of the red rings, the transmission of light, and the prismatic colours, were particularly discussed. The conclusion of this paper was postponed till next meeting.

SOCIETY OF ANTIQUARIES.

Mr. J. A. Repton presented to the society a series of designs of wooden houses, windows, or other parts of buildings constructed of wood, in order to show the origin and progress of architecture in wood throughout England. His views included the principal structures of timber in London, Essex, Suffolk, Norfolk, and Lincolnshire. In the explanatory observations which were read to illustrate the views, it was stated that sash-windows were not introduced into this

country till the age of Charles I., and that they did not
me general before the beginning of the last century.
sashes were originally constructed of very thick timber,
the joinings were left in square pieces in order to add
eir strength, as it was then believed.

s an appendage to these views, the design of Melk-
e-street, a curious old rustic building, entirely of tim-
near Ashford, Kent, was exhibited to the society.
drawing was executed by the late F. Grose, and pre-
ed considerable variety in its architectural ornaments.
design was made in 1760, and has not yet been en-
ed.

WERNERIAN NATURAL HISTORY SOCIETY.

t the meeting of this society, on Saturday the 13th of
ary, the Rev. Dr. Macknight read a mineralogical ac-
t of Ben Ledi, and the environs of Loch Ketterin.
description of the rocks in that district (which consist
mica-slate and clay-slate, with an overlying conglo-
ate, formed at a lower level from the débris of primi-
mountains) tended, in the author's opinion, to illus-
one branch of the Wernerian doctrine respecting the
r of formations in the mineral kingdom. It also ap-
ed, in confirmation of another principle in the Geo-
y, that the direction from SW. to NE. of the strata
posing the Highland mountains, corresponds to what
been observed in general relative to the bearings of
primitive strata in the crust of the earth. Such an
ormity of direction, it would seem, could have resulted
from the action of powers in nature that are slow and
lar in their operation; and must be referred to some
inal law, which later discoveries render it probable
be found to depend on the constitution of the terra-
ous globe in regard to magnetism and electricity.

t the same meeting the secretary laid before the society
mmunication from Mr. William Scoresby junior, of
itby, comprising a meteorological journal of three
enland voyages, with remarks on the effects of the
ther on the barometer in Greenland, and on the different
tallizations of snow to be observed in high latitudes.

XXIV. *Intelligence and Miscellaneous Articles.*

ON PROCURING AN EQUAL TEMPERATURE.

To Mr. Tilloch.

Sir, On accidentally looking over a volume of the Medical Journal for 1801, I found a short but pleasing account of the effects of the climate of Madeira in cases of pulmonary tubercles. The communication is dated in January in the above year, and is addressed by Dr. Adams, then resident in that island, to a medical friend in this country. It concludes with the following suggestions:—"These are, I believe, the principal inquiries you wished to make:—it is true they are of little consequence compared to the important fact you have in view. It is however satisfactory to trace probable causes, *and it may be well worth your while to try whether spacious buildings, regularly heated, safely ventilated, and large enough to admit of necessary exercise, may not answer the purpose for such whose want of means, of courage, or of leisure, prevents their taking a voyage to a more genial climate.*"

It is not my wish, by sending you the above, to detract from the claims of Dr. Pearson, with whom the same idea seems to have originated: perhaps that truly respectable practitioner is not even aware of the existence of the passage in question. It is but fair, however, that the claims of others should be recognized, when the public, as in the present instance, begin to reap the benefits of their suggestions.

I am, &c.

X. Y.

MAGNETISM.

Mr. Leopold Vacca has discovered a method of communicating magnetism to a bar of iron without a magnet.

He takes a bar of iron of about three feet in length, which gives no sign of possessing any magnetic virtue as long as it lies in a horizontal position: but it possesses this in a very sensible degree when placed perpendicularly. These signs disappear again when it is laid down horizontally, and appear again when it is lifted up vertically.

A small bar of steel rubbed several times in the same direction, against the extremity of the other bar, when situated vertically, acquires magnetism: whence the discoverer concludes, that magnetism may be communicated to a body, without either a natural or an artificial magnet.

COBALT.

Those interested in the prosperity of our porcelain ma-
actures will rejoice to be informed that a mine of very
ellent cobalt has been discovered in this country. We
e been informed by Mr. Hume of Long-Acre, to whom
pecimen has been submitted for examination, that on
lysis he has found it to contain nearly 30 per cent. of
metal.

LIST OF PATENTS FOR NEW INVENTIONS.

o Thomas Bayley, of Birmingham, for certain improve-
its in sliding pulleys for window blinds, and for other
poses.—January 15.

o Peter Cox, of Fairford, in the county of Gloucester,
l engineer, for certain improvements upon the thrashing
hine.—January 23.

o Joseph Manton, of Davies-street, Berkeley-square,
-maker, for an improvement in telescopes.—Jan. 23.

o David Cock, of Dean-Street, Soho, in the county of
dlesex, stereotype manufacturer, for vessels of a new
truction, for melting and heating fluids.—Feb. 1.

o Augustus Frederick de Heine, of Moor Lane, Fore-
et, in the city of London, gent., for certain improve-
ts on printing and stamping-presses.—Feb. 1.

o John Craigie, of Quebec, in our province of Lower
ada, in North America, esq., now residing in Craven
et, in the county of Middlesex, who in consequence
mmunications made to him when residing abroad, and
an inventions by himself, is in possession of a me-
of making an improved kitchen fire-place.—Feb. 1.

Stedman Adams, in the city of Hartford, in the state
onnecticut, in North America, esq., at present residing
arey Street, Lincoln's Inn Fields, in the county of
llesex, for certain improvements on steam-engines,
n distillation.—Feb. 1.

William Maker, of the Hay Market, in the county
ddlesex, for certain improvements in the construc-
of pumps.—Feb. 12.

John Slater, of Birmingham, in the county of War-
, coach spring maker, for an improvement in hanging
securing grind stones from breaking in the middle
one.—Feb. 12.

William Doughty, of Birmingham, in the county of
wick, engineer, for his improvement in the combina-
of wheels for gaining mechanical power.—Feb. 12.

George Wake, esq., of the city of Bath, for certain
improve-

improvements in the construction of wheel carriages of various descriptions.—Feb. 12.

To Peter Warburton, of Corbridge, in the county of Stafford, china manufacturer, for his new method of decorating china, porcelain, earthenware, and glass, with native pure or adulterated gold, silver, platina, or other metals, or fluxed,. or lawered with lead, or any other substance; which invention leaves the metals after being burned in their metallic state.—Feb. 13.

To Richard Witty, of the town of Kingston-upon-Hull, gent., for certain improvements in making, arranging and combining certain parts of rotative steam-engines, by which means the most complex parts of the steam-engines now in use are dispensed with and rendered unnecessary, and the whole of the mechanism made much more simple, less expensive, and not so liable to be out of repair, as that of the steam-engines now in use, and applicable to giving motion to all sorts of mill-work or machinery.—Feb. 14.

To Eneas Morrison of the town of Greenock in Scotland, for a machine for conveying persons from the upper parts of houses on fire, and for lowering goods from warehouses, and other purposes.—Feb. 22.

To Peter Stuart, late of Fleet Street, in the city of London, printer, for his method of engraving and printing maps of counties, charts, or other plans or designs, music, mathematical diagrams or figures on wood, metal, or any other substance, so that they may be thrown off in a common printing press or presses, either for books, newspapers, or any other printed paper whatever.—Feb. 26.

To William Bainbridge, of the parish of St. Andrew Holborn, in the city of London, musical instrument maker, for certain improvements on the English flute and flageolet.—Feb. 26.

To Major Pratt, of Spencer Street, St. George's-in-the-East, in the county of Middlesex, farmer, for certain methods of manufacturing machines for performing various agricultural operations by mechanical powers.—Feb. 26.

To Augustus de Heine, of Burr Street, in the county of Middlesex, gent., for certain apparatus by the application of known principles to preserve animal food, vegetable food, and other perishable articles, a long time from perishing or becoming useless.—Feb. 26.

To Charles le Caan, of the town of Llanelly, in the county of Carmarthen, gent., for certain apparatus to be added and united to the axle-trees and wheels or naves of wheels of carriages, so as to impede, resist, or check their action.—Feb. 26.

METEORO-

METEOROLOGICAL TABLE,

BY MR. CAREY, OF THE STRAND,

For February 1810.

ys of the Month.	Thermometer.					
	8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
an. 27	32	32°	31°	30·21	0	Cloudy
28	31	33	30	·22	7	Cloudy
29	30	33	30	·28	6	Cloudy
30	33	33	30	·40	4	Cloudy
31	32	43	47	·25	0	Cloudy
eb. 1	47	47	46	·02	0	Rain
2	46	47	44	29.90	0	Rain
3	45	47	46	·70	0	Rain
4	45	46	36	·92	10	Fair
5	33	45	44	30·00	10	Fair
6	46	47	44	29·96	7	Fair
7	47	47	46	·94	4	Cloudy
8	46	47	45	·90	0	Rain
9	44	47	46	·70	0	Rain
10	47	50	44	·78	0	Rain
11	44	44	40	·81	0	Rain
12	40	41	40	·42	0	Rain
13	38	42	38	28·95	6	Stormy
14	36	39	36	29·50	5	Foggy
15	35	37	30	·90	7	Cloudy
16	26	38	27	30·00	10	Fair
17	27	35	26	·10	9	Fair
18	27	33	36	·11	0	Snow
19	28	36	28	29·98	5	Fair
20	27	30	24	30·22	7	Fair
21	19	30	26	·40	10	Fair
22	26	34	28	·08	5	Fair
23	35	45	40	29·40	0	Rain
24	40	49	47	·50	0	Small rain
25	47	51	40	·46	35	Fair
26	37					

N. B. The Barometer's height is taken at one o'clock.

XXV. *On Injuries of the Brain.*

To Mr. Tilloch.

Sir, In the last Number of your Magazine, there is a very interesting case recorded of considerable derangement of the structure of the brain, thoracic and abdominal viscera. —The former it is my intention particularly to advert to.

The person in whom this disease (tumours in the medullary substance of the brain) existed, is noticed to have been an acute reasoner, a man of good understanding:—in short, to use the author's own words, "distinguished for the facility with which he could converse upon most subjects; and reasoned so closely that his intellectual powers were generally regarded as of a superior kind."

Lately I examined the brain of a person who died of insanity. It was a female between 60 and 70 years of age, who, as far as I could learn, had been deranged for some considerable time (at least 10 years); but, as I had not an opportunity of seeing her whilst alive, cannot distinctly say of what species:—suffice it to observe, it was of the raving kind.

Upon removing the calvaria (or upper part of the cranium), and raising the dura mater, I discovered the tunica arachnoides to be very opake; I could easily distinguish it upon the superior surface of the brain, which cannot be done with facility in the natural state, or where no disease is present. The vessels of the pia mater were loaded with blood, but no effusion was apparent between the membranes. Upon separating the hemispheres to observe the corpus callosum, I found the arteriæ callosæ considerably enlarged, and in that state which precedes ossification, and there was no appearance of raphè. I then proceeded to remove part of the left hemisphere, in order more readily to examine the ventricles; in doing which I nearly cut through the whole of a *tumour*, which I found to be situated partly in the cineritious and partly in the medullary substance of the brain, opposite to the temporal fossa.

The tumour was about the size of a half-crown piece, and somewhat of that shape, though inclining to oval; it was of a granulated appearance, highly vascular, and around the edge presented a dark blue colour; it was in structure *precisely* similar to those (for I had an opportunity of examining them in the recent state) described by Mr. Taunton;—there was an artery entering at the outer side which was in a state of ossification.

It

t is necessary to observe, that the substance of the brain s of a very soft consistence, whereas in the case related Mr. T. it was of a peculiar hardness. I do not imagine t this softness arose from the mental derangement; as it occurred to my lot to examine the brain of many perns who have died insane, where this organ has been of usual texture, and sometimes unusually firm.

Many other diseased appearances were observed during dissection, but which are generally attendant in cases mania; such as water in the ventricles, opacity of the tum lucidum, bloody points in the medullary substance the brain, &c besides opakeness of the tunica arachnoides, l turgescence of the vessels of the pia mater before menned.

The whole of the nerves arising from the brain were unnmonly firm, and the olfactory (as in the instance reded by Mr. T.) were in appearance similar to a piece of row tape, adhering strongly to the cribriform process of ethmoidal bone, and afforded some little resistance to knife.

Mr. T.'s case appears to establish the fact, of the brain, most delicate organ in the whole structure of man, beg capable of accommodating itself to an extraneous subnce, without producing any visible alteration in the opeions of the mental faculties. The question which seems turally to arise from this circumstance is: Whether the bstance of the brain was not absorbed in proportion to quantity of deposition secreted by the arteries?—and, hether this could be effected sufficiently gradually, not impede the functions of that wonderful and anomalous gan*? placed (as Harwood elegantly expresses it) on the ubtful confines of the material and spiritual worlds!

That the brain may become absorbed in proportion to e growth of the tumours, appears to me highly probable; d that these tumours, in Mr. T.'s case at least, if not in me, were in the first instance exudations of lymph, which course of time became organized. It is an interesting ject, and I hope will be considered by more able anaists and physiologists than myself.

With respect to the abdominal viscera:—That the *pan*as as well as many other of the organs contained in the domen may be diseased and not suspected, I have witsed in several instances. I have in a number of cases after death upon examination found that organ in a considerable state of disease, where the only symptom that existed during life was a slight degree of dyspepsia.

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* That the arteries formed this secretion in a very slow manner must obvious, as no symptoms of compression were present during life.

I am yours, &c.

Fleet Street, Feb. 10, 1810. T. J. PETTIGREW.

XXVI. *On Salmon-Leaps.*

To Mr. Tilloch.

SIR, WHOEVER carefully peruses Mr. Carr's paper " On the ascent of salmon over the elevations in the course of rivers called salmon-leaps," as given in your Magazine for November last, must needs be astonished indeed at the rapidity of the growth of young salmon, from the period of their being spawned to their departure out of the rivers. As he is not precise as to the time when these occurrences happen, I must beg leave to state them:—The spawn is deposited chiefly in December and January, and the salmon depart in the beginning of April, being on an average a space of about fourteen weeks. Now admitting the eggs to be hatched in eight weeks (which I believe is much too little), we have only six weeks for the young fry to arrive at the length of *six or eight inches;* an increase which is absolutely incredible, more especially when we consider the comparative *want* of food incident to the season.

The fact is; the young fry do not descend the rivers with the old salmon, in the spring after they are spawned; for in the month of October following they are no bigger than a minnow. Mr. John Clayton of Stockport (who is reckoned to be one of the most experienced anglers in the kingdom), and others, have frequently caught them of the size, and at the time stated, and are fully satisfied of their being young salmon. In the months of June and July they are caught about five or six inches in length: this I know to be fact; and it is not till the ensuing spring that they pass with the old ones down the rivers with the floods into the sea. Their growth is there very rapid, as they are found on their return, in the months of August and September, to weigh from 14 to 20 ounces.

This statement, if not established beyond all doubt by incontrovertible facts, is highly probable, and accords more with their progressive growth, and rational conjecture, than the account given to us by Mr. Carr. I am, &c.

Feb. 10, 1801. PISCATOR.

XXVII. On

VII. *On Platina and Native Palladium from Brasil.* y WILLIAM HYDE WOLLASTON, *M.D. Sec.R.S.*[*]

THOUGH platina has now been known to mineralogists nore than 60 years, yet it had not been discovered in other places than Choco and Santa Fé, whence it was nally brought, until about two years since M. Vau- in discovered it in some gray silver ores from Guadal- l in Estremadura. In analysing these ores, he found e fragments that contained as much as one-tenth of weight of platina, but he did not find it accompanied ny of the new metals that have lately been discovered e Peruvian ore of platina.

he specimen which I am now about to describe is de- l from a third source, and it is rendered the more in- sting by having grains of native palladium mixed with This new mineral has lately been received from the mines in Brasil, by H. E. Chev. de Souza Coutinho, assador from the court of Portugal, resident in this ntry; and I am in hopes that some account of it may cceptable to the Royal Society, although the analysis t necessarily be very imperfect, from the small quantity hich my experiments have unavoidably been confined.

he general aspect of this specimen is so different from common ore of platina, that I could form no con- ure of what ingredients it might be found to consist, appearance was such indeed, as at first sight to induce uspicion of its not being in a natural state, for it had much the spongy form which is given to platina from erfect attempts to render it malleable by means of nic.

ne circumstance, however, occasions a presumption no art has been employed in giving the grains their ent appearance; as upon close inspection many small icles of gold are discernible, but there is none of the netic iron sand with which the Peruvian ore abounds, any of the small hyacinths, which I have formerly no- d as accompanying that mineral[†].

t is very well known, that the common ore of platina in eral consists of flattened grains, that appear so much n at their surface, as to be in a considerable degree po- ed, and the roughness observable in some of the larger ns arises from concave indentations of a reddish brown or black colour. The Brasilian platina, on the contrary, has no polish, and does not appear worn; but most of the grains seem to be small fragments of a spongy substance, and even those which are yet entire and rounded on all sides, present a sort of roughness totally different from that of the former, as their surface consists of small spherical protuberances closely coherent to each other, with the interstices extremely clean, and free from any degree of tarnish.

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[*] From Philosophical Transactions for 1809, Part II.

[†] Phil. Trans. for 1805, p. 318.

The first portion that I employed for solution was taken without any selection, and being digested with a small quantity of nitro muriatic acid, two of the grains were acted on much more rapidly than is usual with platina, and seemed to give a redder colour than that metal alone. These grains were consequently taken out, washed, and reserved for separate examination, and the solution was allowed to proceed till the rest were entirely dissolved. By the addition of muriate of ammonia an abundant precipitate was formed of a bright yellow colour. This precipitate was evidently platina, and its colour satisfied me that the grains had not been brought into their present state from Peruvian platina by means of arsenic; for where arsenic has been employed, I have observed that the iridium contained in that ore is rendered more soluble than before, and hence communicates its red colour to the precipitate.

From the grains thus examined, there appeared not to be any iridium dissolved, nor any black powder containing iridium undissolved.

I next endeavoured, by prussiate of mercury, to ascertain the presence of palladium; but though a precipitate which occurred indicated a certain quantity, it remained doubtful whether it was derived from the grains of platina themselves, or from the two small fragments that had been in part dissolved before they were separated from the rest.

By addition of ammonia to the solution, no iron was precipitated; and when the solution was afterwards allowed slowly to evaporate, I could discern no crystals or colour that I could ascribe to the presence of rhodium. In short, it seemed that these grains are really native platina nearly pure.

In order to discover whether the grains themselves contained any portion of gold, I selected three of the largest, weighing together eight grains and a half; and after a solution and precipitation, as before, by muriate of ammonia, I added a solution of green sulphate of iron, and obtained a precipitate of gold. It was, however, far too small in quan-

to be estimated with correctness, but certainly did not
ed the $\frac{1}{200}$ of a grain. This, it is to be observed, is
her circumstance in which the present mineral differs
the Peruvian ore of platina, which I believe never
ains (in the ore itself) the smallest quantity of gold.

this experiment also, I tried to detect the existence of
dium in the solution, and by prussiate of mercury again
rtained its presence; but it was in too small quantity
stimating the proportion it bore to the whole mass.

may deserve to be remarked, that though neither the
vian nor Brasilian grains of platina contain any silver,
he gold which accompanies them is in each instance
uch alloyed with silver, that from about thirty small
s of gold picked from Peruvian platina, weighing two
s, I obtained as much as four tenths of a grain of
r, or one fifth part of their weight.

Native Palladium.

he two fragments, that had been separated from the
solution, next claimed my attention, and evidently de-
ed a careful examination. They were each placed in
p of nitric acid, and each communicated a deep red
ur, which, by the tests of prussiate of mercury and
n sulphate of iron, I was satisfied arose from palladium.

smaller fragment was then divided, and one portion
ved to remain in the acid till it seemed completely dis-
ed, and the other examined by the blow-pipe. The
ost heat that could be given, appeared to have no effect:
when a small piece of sulphur was applied to it, it
d instantly; by continuance of the heat, it parted with
sulphur, and became completely malleable In short,
fectly resembled palladium; and as it retained its bril-
cy in cooling, I judged it to be nearly pure.

ut as the surfaces which had been acted upon by nitric
had a degree of blackness, that might be owing to
e insoluble impurity, I have since that time dissolved
larger fragment for the sake of discovering the cause of
appearance. Hot nitric acid dissolved by far the
test part; but there remained a black powder on which
sh addition of this acid alone had no further effect.
when a drop or two of muriatic acid was added, the
de was very soon dissolved. By the addition of muriate
mmonia, it became evident from the precipitate that
esiduum was principally platina. But this precipitate,
ad of being yellow, had the deep red colour, which is
lly occasioned by the presence of iridium. The pla-
tina

tina reduced from this precipitate was also too black for pure platina, and when it was again dissolved, the solution was of a deep red, and the precipitate by muriate of ammonia red, as before; so that although the grains of Brasilian platina appear to be free from iridium, as well as from many other impurities that form part of the Peruvian ore, yet the grains of native palladium that accompany them, afford a trace of this ingredient, and occasion a presumption that osmium and rhodium may hereafter appear, when we can obtain this mineral in larger quantity.

Since the whole weight of metal employed in the last experiment did not exceed $1\frac{8}{10}$ grain, it is in vain to attempt to estimate the proportion of the ingredients; but if I am near the truth, in considering the quantity of the red precipitate as about one fifth of a grain, of which less than half is platina, those who are best acquainted with the intense colouring power of iridium may endeavour to form a conception of the extremely small quantity that can be present.

As soon as I had ascertained the existence of native palladium, I endeavoured, by examination of its external characters, to distinguish its appearance from that of the surrounding substances, and I found it by no means difficult, although no difference of colour could be discerned. Having remarked that the larger fragment appeared rather fibrous, and that the fibres were in some degree divergent from one extremity, I examined the remainder of the small specimen which had originally been given to me, and by this peculiarity of structure I soon detected a third fragment, which upon trial proved to be the same substance. By favour of the Chev. de Souza I was also permitted, with this view, to examine the specimen which remained in his possession, and had soon the satisfaction of discovering two more fragments of the same mineral; and as I was in no one instance deceived in my choice, by attending to the radiating fibres, I am in hopes that this external character will enable persons to distinguish that metal, in situations where they have not an opportunity of deciding by chemical experiment.

VIII. *On an Improvement in the Manner of dividing tronomical Instruments.* *By* HENRY CAVENDISH, *sq., F.R.S.*[*]

E great inconvenience and difficulty in the common
hod of dividing, arises from the danger of bruising the
ions by putting the point of the compass into them,
from the difficulty of placing that point mid-way, be-
n two scratches very near together, without its slipping
rds one of them; and it is this imperfection in the
mon process, which appears to have deterred Mr.
ghton from using it, and thereby gave rise to the in-
ous method of dividing described in the preceding part
his volume[†]. This induced me to consider, whether
bove-mentioned inconvenience might not be removed,
sing a beam compass with only one point, and a mi-
cope instead of the other; and I find, that in the fol-
ng manner of proceeding, we have no need of ever
ng the point of the compass into a division, and con-
ently that the great objection to the old method of di-
g is entirely removed.

this method, it is necessary to have a convenient sup-
for the beam compass: and the following seems to me
as convenient as any. Let C C C (Plate V. Fig. 1.) be
ircle to be divided, B B B a frame resting steadily on its
and made to slide round on it with an adjusting mo-
to bring it to any required point: $d\delta$ is the beam com-
having a point near δ, and a microscope m made to
from one end to the other. This beam compass is
orted at d, in such manner as to turn round on this
t as a centre, without shake or tottering; and at the
δ it rests on another support, which can readily be
red, so as either to let the point rest on the circle, or
event its touching it. It must be observed, however,
as the distance of d from the centre of the circle must
aried, according to the magnitude of the arch to be
ed, the piece on which d is supported had best be
e to slide nearer to, or further from, the centre; but
frame must be made to bear constantly against the
of the circle to be divided, so that the distance of d
the centre of this circle, shall not alter by sliding the
e.

is being premised, we will first consider the manner
viding by continued bisection. Let F and f be two

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[*] From Philosophical Transactions for 1809, Part II.

[†] See Phil. Mag. vol. xxxiv. pages 81 and 163.

points on this limb which are to be bisected in φ. Take the distance of the microscope from the point nearly equal to the chord of $f\,\varphi$, and place d so that the point and the axis of the microscope shall both be in the circle in which the divisions are to be cut. Then slide the frame B B B till the wire of the microscope bisects the point F; and having lowered the support at δ, make a faint scratch with the point.

Having done this, turn the beam compass round on the centre d till the point comes to D, where it must rest on a support similar to that at δ; and having slid the frame till the wire of the microscope bisects the point f, make another faint scratch with the point, which, if the distance of the microscope from the point has been well taken, will be very near the former scratch; and the point mid-way between them will be the accurate bisection of the arch F f; but it is unnecessary, and better not to attempt to place a point between these two scratches.

Having by these means determined the bisection at φ, we must bisect the arches F φ and $f\,\varphi$ in just the same manner as before, except that the wire of the microscope must be made to bisect the interval between the two faint scratches, instead of bisecting a point.

It must be observed that when the arch to be bisected is small, it will be necessary to use a bent point, as otherwise it could not be brought near enough to the axis of the microscope; and then part of the rays, which form the image of the object seen by the microscope, will be intercepted by the point; but I believe, that by proper management this may be done without either making the point too weak, or making the image indistinct; but if this cannot be done, we may have recourse to Mr. Troughton's expedient of bisecting an odd number of contiguous divisions.

It must be observed too, that in the bisections of all the arches of the same magnitude, the position of the point d on the frame remains unaltered; but its position must be altered every time the magnitude of the arch is altered.

It is scarcely necessary to say, that the bisections thus made are not intended as the real divisions, but only as marks from which they are to be cut. In order to make the real divisions, the microscope must be placed near the point, and the support d must be placed so that $d\,\delta$ shall be a tangent to the circle at δ. The wire of the microscope must then be made to bisect one of these marks, and a point or division cut with the point, and the process continued till the divisions are all made.

It is plain that in this way, without some further pre-
ition, we must depend on the microscope not altering
position in respect of the point during the operation;
which reason I should prefer placing the axis of the
roscope at exactly the same distance from the centre of
tion d, as the point; but removed from it sideways, by
rly the semi-diameter of the object glass; so that hav-
made the division, we may move the beam compass till
division comes within the field of the microscope, and
n see whether it is bisected by the wire, and consequently
whether the microscope has altered its place.

n the operation of bisection, as above described, it may
observed, that if the two scratches are placed so near to-
her, that in making the second the point of the compass
s into the burr raised by the first, there seems to be
e danger that the point may be a little deflected from
true course; though in Bird's account of his method,
o not find that he apprehends any inconvenience from
One way of obviating this inconvenience, if it does
st, would be to set the beam compass not so exactly to
true length, as that one scratch should run into the
r of the other; but as this would make it more difficult
udge of the true point of bisection, perhaps it might be
ter to make one scratch extend from the circle towards
centre, and the other from it.

t is clear, that the entire arc of a circle cannot be divided
legrees, without trisection and quinquesection; and I
not know whether our artists have recourse to this ope-
on, or whether they avoid it by some contrivance similar
Bird's; namely, that of laying down an arch capable of
tinued bisection; but if the method of quinquesection
referred, it may be performed by either of the three fol-
ing methods:

First Method.

et $a\ \alpha$ (Fig. 2) be the arch to be quinquesected. Open
beam compass to the chord of one fifth of this arch;
g the microscope to a, and with the point make the
atch f; then bring the microscope to f, and draw the
tch e; and in the same manner make the scratches d
b. Then turn the beam compass half round, and hav-
brought the microscope to α, make the scratch β; and
ceeding as before, make the scratches δ, ε and φ. Then
true position of the first quinquesection will be between
nd β, distant from β by one fifth of $b\ \beta$, and the se-
d will be distant from δ by two fifths of $d\ \delta$, and so on.
Then, in subdividing these arches, and striking the true
divisions,

divisions, the wire of the microscope, instead of bisecting the interval between the two scratches, must be brought four times nearer to β than to b. But in order to avoid the confusion which would otherwise proceed from this, it will be necessary to place marks on the limb opposite to all those divisions, in which the interval of the scratches is not to be bisected, showing in what proportion they are to be divided; and these marks should be placed so as to be visible through the microscope, at the same time as the scratches. Perhaps, the best way of forming these marks, would be to make dots with the point of the beam compass contiguous to that scratch which the wire is to be nearest to, which may be done at the time the scratch is drawn.

Perhaps an experienced eye might be able to place the wire in the proper manner, between the two scratches, without further assistance; but the most accurate way would be to have a moveable wire with a micrometer, in the focus of the microscope, as well as a fixed one; and then having brought the fixed wire to b, bring the moveable one to β, and observe the distance of the two wires by the micrometer; then reduce the distance of the two wires to one fifth part of this, and move the frame till the moveable wire comes to β, and then the fixed wire will be in the proper position, that is four times nearer to β than to b.

It will be a great convenience, that the moveable wire should be made in such manner, as to be readily distinguished from the fixed, without the trouble of moving it.

In this manner of proceeding, I think a careful operator can hardly make any mistake: for if he makes any considerable error in the distance of the moveable wire from the fixed, it will be detected by the fixed wire not appearing in the right position, in respect of the two scratches; and as the mark is seen through the microscope, at the same time as the scratches, there is no danger of his mistaking which scratch it is to be nearest to, or at what distance it is to be placed from it.

To judge of the comparative accuracy of this method with that of bisection, it must be considered that the arches $\alpha\beta$, $\beta\delta$, &c. though made with the same opening of the compass, will not be exactly alike, owing partly to irregularities in the brass, and partly to other causes. Let us suppose, therefore, that in dividing the arch $u\,\alpha$ into five parts, the beam compass is opened to the exact length, but that from the abovementioned irregularities the arches $\alpha\beta$, $\beta\delta$, $\delta\epsilon$, and nd $\varepsilon\varphi$ are all too long by the small quantity ε, and that e arches af, fe, ϵd, and db are all too short by the me quantity, which is the supposition the most unfavourble of any to the exactness of the operation; then the ror in the position of $\beta = \varepsilon$, and the point b errs 4ε in the me direction, and therefore the point assumed as the true oint of quinquesection, will be at the distance of $\frac{3\varepsilon}{5}$ from and the error in the position of this point $= \varepsilon \times 1\frac{3}{5}$.

By the same way of reasoning, the error in the position the point taken between d and $\delta = \varepsilon \times 2\frac{2}{5}$.

In trisecting the error of each point $= \varepsilon \times 1\frac{1}{3}$; and in secting, the error $= \varepsilon$; and in quadrisecting, the error of e middle point $= 2\varepsilon$.

It appears therefore that in trisecting, the greatest error e are liable to does not exceed that of bisection in a greater oportion than that of 4 to 3; but in quinquesecting the ror of the two middle points is $2\frac{2}{5}$ times greater than in secting. It must be considered, however, that in the ethod of continued bisection, the two opposite points ust be found by quadrisection; and the error of quinquection exceeds that of quadrisection in no greater proporon than that of six to five; so that we may fairly say, at if we begin with quinquesection, this method of diding is not greatly inferior, in point of accuracy, to that continued bisection.

Second Method.

This differs from the foregoing, in placing dots or scratches the true points of quinquesection and trisection, before begin to subdivide. For this purpose, we must have a icroscope placed as in page 170, first par. at the same diance from the centre of motion as the point is; and is microscope must be furnished with a moveable wire d micrometer, as in page 171; and then having first ade the fixed wire of this microscope correspond exactly th the point, we must draw the scratches b and β, d and &c. as before, and bring the fixed wire to the true point quinquesection between b and β, in the manner directed page 226, and with the point strike the scratch or dot; d if we please, we may, for further security, as soon as is is done, examine, by means of the moveable wire, hether this intermediate scratch or dot is well placed.

The advantage of this method is, that when this is done, may subdivide and cut the true divisions, by making e wire of the microscope bisect the intermediate scratches, stead of being obliged to use the more troublesome operation of placing it in the proper proportion of distance between the two extremes.

This method certainly requires less attention than the former, and on the whole seems to be attended with considerably less trouble; but it is not quite so exact, as we are liable to the double error of placing the intermediate point and of subdividing from it.

As in this method the intermediate points are placed by means of the micrometer, there is no inconvenience in placing the extreme scratches b and β, &c. at such a distance from each other, that the intermediate one shall be in no danger of running into the burr raised by the extremes.

Third Method.

Let $a\,\alpha$ (Fig. 3.) be the arch to be quinquesected; lay down the arches $a\,b$, $d\,b$, and $d\,e$, as in the first method; then turn the beam compass half round, and lay down the arches $\alpha\,\beta$ and $\beta\,\delta$; then, without altering the frame, move the moveable wire of the microscope till it is four times nearer to δ than to e, and, having first rubbed out the former scratches, lay them down again with the compass thus altered: but as this method possesses not much, if any, advantage over the second, in point of ease, and is certainly inferior to it in exactness, it is not worth while saying any thing further about it.

It was before said*, that the centre of motion of the beam compass is to be placed, so that the point and axis of the microscope shall both be in the circle in which the divisions are made; but it is necessary to consider this more accurately. Let Aδ (Fig.4.) be the circle in which the scratches are to be made, δ the point of the beam compass, which we will suppose to be exactly in this circle, d the centre on which it turns, and Mm the wire in the focus of the microscope, and let m be that point in which it is cut by the circle; and let us suppose that this point is not exactly in the line $d\,\delta$, then, when the beam compass is turned round, the circle will cut the wire in a different point μ, placed as much on one side of $d\,\delta$, as m is on the other, so that if the wire is not perpendicular to $d\,\delta$, the arch set off by the beam compass, after being turned round, will not be the same as before; but if it is perpendicular, there will be no difference; for which reason, care should be taken to make the wire exactly perpendicular to $d\,\delta$, which is easily examined by observing whether a point appears to run along it,

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* Page 168.

, while the beam compass is turned a little on its centre.
t is also necessary to take care that the point δ is in the
e of the circle, while the bisection is observed by the
icroscope, which may most conveniently be obtained, by
lacing a stop on the support on which that end of the
eam compass rests. If proper care, however, is taken in
lacing the wire perpendicular, no great nicety is required
ther in this or in the position of *d*.

Another thing to be attended to, in making the wire
isect two scratches, is to take care that it bisects them
the part where they cut the circle; for as the wire is
ot perpendicular to the circle, except in very small arches,
is plain, that if it bisects the scratches at the circle, it
ill not bisect them at a distance from it.

There are many particulars in which my description of
e apparatus to be employed will appear incomplete; but
there is nothing in it which seems attended with dif-
culty, I thought it best not to enter further into particu-
rs, than was necessary to explain the principle, and to
ave the rest to any artist who may choose to try it.

It is difficult to form a proper judgement of the con-
niences or inconveniences of this method, without ex-
rience; but, as far as I can judge, it must have much
lvantage, both in point of accuracy and ease, over that of
viding by the common beam compasses: but it very
kely may be thought that Mr. Troughton's method is
tter than either. Whether it is or is not, must be left for
termination to experience and the judgement of artists.
hus much, however, may be observed, that this, as well
his, is free from the difficulty and inaccuracy of setting
e point of a compass exactly in the centre of a division.
also requires much less apparatus than his, and is free
om any danger of error, from the slipping or irregularity
the motion of a roller; in which respect his method,
otwithstanding the precautions used by him, is perhaps
ot entirely free from objection; and, what with some
tists may be thought a considerable advantage, it is free
om the danger of mistakes in computing a table of errors,
d in adjusting a sector according to the numbers of that
ble.

XXIX. *On*

XXIX. *On M.* Bemetzrieder's *erroneous Calculations of the Magnitudes of certain musical Intervals. By Mr.* John Farey.

To Mr. Tilloch.

Sir, When I some time ago took up my pen*, to combat the erroneous principles advanced by *Earl Stanhope*, respecting the accuracy of expressing musical intervals by the *difference* of the *lengths* of strings producing the sounds, I thought that his lordship had an exclusive claim to this and other similar and "important musical truths," which he has advanced; but a folio work opening the long-way was this day put into my hands, entitled, "General Instructions in Music," by M. Bemetzrieder, printed at London, about the year 1780 (as I have been told), price one guinea. Who after stating, at page 92, that a major tone $\frac{8}{9}$ taken from a minor fourth $\frac{3}{4}$, or $\frac{3}{4} \times \frac{9}{8}$ produces $\frac{27}{32}$ (which he calls a minor third) and which taken from a fifth $\frac{2}{3}$, or $\frac{2}{3} \times \frac{32}{27}$ produces $\frac{64}{81}$ (which he calls a major third), and mentioning, that $\frac{5}{6}$ and $\frac{4}{5}$ are ratios *usually* assigned to the 3^{d}. and III^{d}., thereon subjoins these remarks: "It is "probable, that the *facility of arithmetical calculation* has "been preferred to geometrical exactitude: besides, the "difference of the two (major) thirds $\frac{4}{5}$ and $\frac{64}{81}$ is no more "than $\frac{4}{405}$ $\left(\text{being the } \textit{difference} \text{ of } \frac{324}{405} \text{ and } \frac{320}{405}\right)$ the 90th "part of a tone $\left(\text{because } \frac{4 \times 90}{405} = \frac{360}{405} = \frac{8}{9}\right)$ which is *bet*"*ter perceived by the understanding than by the ear;* the "(major) third $\frac{4}{5}$ is a 90th part of a tone lower than the "third $\frac{64}{81}$. The difference of the two minor thirds $\frac{5}{6}$ "and $\frac{27}{32}$ is $\frac{1}{96}$; the (minor) third $\frac{5}{6}$, is $\frac{1}{96}$ too acute:" for as above, $\frac{162}{192} - \frac{160}{192} = \frac{2}{192} = \frac{1}{96}$, and still proceeding as above, $\frac{1 \times 83\frac{1}{3}}{96} = \frac{8}{9}$; or, $\frac{27}{32}$ is more than $\frac{5}{6}$ by an 83$\frac{1}{3}$ part of the same tone $\left(\frac{8}{9}\right)$ as above. Now, if instead of taking the *numerical differences* of the thirds, in Mr. B.'s erroneous

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* See our 27th Volume, page 198, and 33d Volume, page 294 and 5.—Editor.

method,

ethod, the *ratios* had been subtracted thus, $\frac{64}{81} \times \frac{5}{4} =$ $\frac{0}{4} = \frac{80}{81}$, and $\frac{5}{6} \times \frac{32}{27} = \frac{160}{162} = \frac{80}{81}$ the results would be e major comma in both instances, instead of one being 90th, and the other an 83⅓rd part of a tone, neither of hich are much different from one ninth part of what they *ght to be*, being altogether the produce of egregious undering; for the major comma is known to be nearly e 9⅑th part of a major tone, or more exactly, 9·48141 × = T. And thus we see, that the error of a whole comma said by this gentleman not to be perceivable by the ear! d guided as I suppose by the above erroneous principle, has added to the confusion which already reigned, as to e *names* of intervals, by giving many new and absurd ones; r instance, he calls $\frac{16384}{19683}$ a superfluous *second*, although exceeds a minor *third* in magnitude, $\frac{59049}{65536}$ he calls a diminished *third*, although the same is less than a minor tone, comma-deficient major *second*, and so of many others. page 116 he asserts, that 1, $\frac{1}{3}$ and $\frac{1}{5}$ are not the Haronicals or "replies" of a sound, but 1, $\frac{1}{3}$ and $\frac{16}{81}$!

Hoping, Sir, that this will be the last time that I shall ve to expose such gross ignorance in authors, on the nature d fundamental principles of musical calculation,

I remain yours, &c.

stminster, 12th March 1810. JOHN FAREY.

XX. *On the Culture of Parsnips, and their Utility in feeding Cattle.* By CHARLES LE HARDY, *Esq., of the Island of Jersey**.

R, HAVING observed in the book of Premiums offered by Society, that they wished for information on the culture of rsnips, which are much used in the island of Jersey;—as ving practised it for many years, I take the liberty to comunicate what I know on the subject, with the result of me comparative experiments.

The culture of parsnips and beans is looked upon as one the regular courses of crops in the island. There is no mer, be the extent of his grounds ever so small, who does not yearly plant a proportionate quantity, for the purpose of fattening his hogs and cattle, or of feeding his milch cows.

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* From *Transactions of the Society for the Encouragement of Arts, Manuures, and Commerce*, for 1809.——The Society voted their silver medal Mr. Le Hardy for this communication.

A few years ago, the culture of potatoes was substituted by some farmers to that of parsnips, and apparently with advantage; but further experience has brought them back again to their former practice. Potatoes produce more weight and measure on a given extent of ground, and may be cultivated with less expense; still the parsnip is found to answer best for the farmer's purpose. A perch of the island, which is twenty-four square feet, will produce on an average crop, seven cabots of potatoes, each weighing forty pounds; the same extent in parsnips will only average six cabots, which weigh only thirty-five pounds each, making twenty pounds weight in favour of the potatoes;—but they are not so nutritious as parsnips.

Parsnips will thrive almost any where, but better in a deep stiff loam. They are generally cultivated in the island after a crop of barley, in the following manner.—At the end of January or the beginning of February, the soil, which requires for that purpose to be stirred from the bottom, is either dug with spades after a skimming plough, or with two ploughs of different shapes following one another. The latter of the two, invented some years ago by a farmer in the island, will go to a depth of fifteen inches. In both these ways the neighbouring farmers assist each other: in the season, it is not uncommon to see forty or fifty men in one field digging after a plough. When the large plough is used, less men are required, but more strength of cattle; two oxen and six horses are the team generally used. Those days are reckoned days of recreation, and tend to promote social intercourse among that class of men.

After the ground has been tilled in this way, it is coarsely harrowed, and a sufficient number of women are provided to plant beans. These are dibbled in rows three by three ⁝⁝⁝⁝⁝⁝⁝⁝⁝ at the distance of five feet from row to row. Two women may plant one vergee in a day: two vergees and a half being equal to an English acre. Three sixtenniers of parsnip seed (about $\frac{1}{4}$ of a Winchester bushel) are then sown upon each vergee, and the whole is finely harrowed.

This crop now requires no attendance till the month of May, when weeding becomes necessary. This is the most expensive part of the culture. It is generally done by hand, with a small weeding fork; and as the parsnips require to be kept very clean, the expense is proportionate to the quantity of weeds. This last summer four women were employed

ployed twenty-eight days each in weeding about five vergees. I tried a few perches with the hand-hoe, and thinned them like turnips; they proved finer than those which were hand-weeded. In Guernsey they make use of the spade for hat purpose.

In the beginning of September the beans are pulled up from among the parsnips, and about the latter end the digging begins. The instrument used is the common three-pronged fork. This work is done gradually as the cattle want them, till the ground requires to be cleared for sowng wheat, which after parsnips is generally done about the middle of December. They are reckoned an excellent falow for that kind of grain, and the finest crops are generally hose which succeed them : as it is a tap-rooted plant, it does not, like the potatoe, impoverish the surface, but eaves it mellow and free from weeds, to a succeeding crop.

When parsnips require to be kept for the use of cattle, hey are brought dry under sheds, and will keep good without any care till the end of March. Should they require o be kept longer, they are laid in double rows over one another, their heads outward, with alternate strata of earth, which, when finished, have the appearance of small walls, or, if made circular, of small towers. Those for seed are always preserved in this manner, and sometimes carrots and beets for culinary purposes.

Parsnips are not injured by frost; after having been frozen, they are fit for vegetation : the only sensible alteration s their acquiring a sweeter taste, and by that perhaps becoming more nutritive. They are given raw to hogs and to horned cattle. Though horses are fond of these roots, they are not suffered to eat them, as they make them languid, and are apt to injure their sight. Their leaves when wet are o caustic as to blister the hands of the weeders, and someimes to occasion a violent inflammation in the eyes and dders of the cattle feeding upon them.

Cows fed on parsnips in the winter months, give a greater uantity of milk and butter, and of better flavour, than hose fed upon potatoes. The butter is nearly equal to that from spring grass. Though the root of this plant has the uality of improving that article, it must be observed, that the leaves give it a very disagreeable taste, which, however, is of no consequence when intended to be potted, as it goes ff in a short time.

Parsnips are dangerous food for sows before they farrow, nd might occasion them to lose their litter. Hogs may be attened with them in about six weeks. It is the custom during that time, to thicken their swill with the meal of beans and oats ground together. Pork fattened in this way is very firm, and does not waste in boiling.

Horned cattle may be fattened with parsnips in about three months. I never knew them used for sheep.

It is the general opinion in the island, that hogs or cattle fed on parsnips may be brought in a condition for slaughtering in less time, and with half the quantity that would be required of potatoes. The butchers are sensible of the superiority of the former, and will give a halfpenny per pound more for cattle fattened with them, than for such as have been fed any other way. Upon inquiry I was informed, they always contained a greater quantity of tallow.

This I believe to be a full account of the culture and use of the parsnip, and a just comparison with the potatoe. Should the Society wish any further information, either on this, or on my Telegraph, I shall think myself in duty bound to give it.

I remain, sir,
Your most obedient humble servant,
CHARLES LE HARDY.

The above communication was accompanied with certificates of the correctness of the statements which it contains.

XXXI. *Analysis of the Carbonated Chalybeate Well, lately discovered at Middleton Hall, the Seat of Sir* WILLIAM PAXTON, *Kt., near Llanarthney in Carmarthenshire. Communicated by Mr.* HOWELL. *The Analytical Results from an Analysis of Mr.* ACCUM.

MEDICINAL waters have from time immemorial been much resorted to by the afflicted; and many traditionary as well as written accounts of their virtues have been transmitted from one age to another.

During the first efforts of science, accident seems to have given some of these waters an illegitimate value, and prejudice or fiction clothed others with mystery.

Before the Christian æra, the effects of particular waters were known; and to some, such as the fabled waters of Lethe, supernatural powers were attributed. Soon after the commencement of that æra, medicinal springs increased every where, and superstition found in almost every situation a holy or canonized well. The first rays of reformation exposed the impotency of many, and the unchastened glare that too often leads into extremes, brought others deservedly into contempt. Those individuals, however, who l received benefit from particular springs, could not for- their obligations; and others being daily relieved, a series evidence presented itself that could not be resisted; and refore a few springs retained their celebrity, whilst ers fell into deserved neglect.

The medicinal spring under consideration has been lately covered in the park of Sir William Paxton, at Middleton ll, near Llanarthney. The medicinal effect which this ing has already produced, bids fair to hope that this water l occupy a very distinguished place among those fountains health which can never be viewed with indifference. The er of this spring has been analysed by Mr. Accum of ndon. Omitting to state the analytical processes by which philosopher renders his results legitimate, it will be suf- nt to announce the summary of the analysis, which is as ows:

Gazeous Contents in 100 *Parts.*

	cubic inches.
Carbonic acid gas - - - - - -	16·50
Atmospheric air - - - - - - -	4·50
Cubic inches	21·

Solid Contents in 100 *Parts.*

	grains.
Carbonate of iron - - - - - - -	6·25
Muriate of soda - - - - - - -	6·00
Carbonate of lime - - - - - - -	4·75
Muriate of lime - - - - - - -	3·25
Sulphate of lime - - - - - - -	2·00
Grains	21·25

XII. *On the Preparation of a Fibrous Substance from Bean Stalks, applicable to the Uses for which Hemp is mployed. By the Rev.* James Hall, *of Walthamstow**.

THOUGH it has not been attended to, nor, so far as now, has ever been mentioned by any one, yet it is cer- that, according to its size, every bean plant contains n 20 to 35 filaments, or fibres, running up on the out- , under a thin membrane, from the root to the very top

all

From *Transactions of the Society for the Encouragement of Arts, Manufac-* and *Commerce*, for 1809.——The Society voted their silver medal to Hall for this communication.

all around, the one at each of the four corners being *rather thicker* and stronger than the rest. It is also certain that, next to Chinese, or sea-grass, in other words, the material with which hooks are sometimes fixed to the end of fishing lines, the filaments or hempen particles of the bean plant are among the strongest yet discovered. These with a little beating, rubbing and shaking, are easily separated from the strawy part, when the plant has been steeped 10 or 12 days in water; or is damp, and in a state approaching to fermentation, or what is commonly called rotting. Washing and pulling it through hackles, or iron combs, first coarse and then finer, is necessary to the dressing of bean hemp; and, so far as I have yet discovered, the easiest way of separating the filaments from the thin membrane that surrounds them.

From carefully observing the medium number of bean-plants in a square yard, in a variety of fields on both sides the Tweed, as well as in Ireland, and multiplying them by 4840, the number of square yards in an acre, and then weighing the hemp or filaments of a certain number of these stalks, I find that there are at a medium about 2cwt. of hemp, or these filaments, in every acre, admirably calculated for being converted into a thousand articles, where strength and durability is of importance, as well as, with a little preparation, into paper of all kinds; even that of the most delicate texture.

Now since there are at least 200,000 acres of ticks, horse and other beans, planted in Great Britain and Ireland, and since, where there is not machinery for the purpose, the poor, both young and old, females as well as males, belonging to each of the 9700 parishes in England, &c. where beans are raised, might (hemp having risen of late from 60 to 120 pounds per ton) be advantageously employed in peeling, or otherwise separating these filaments from the strawy part of the plant, after the beans have been thrashed out; I leave it to the feelings of the Society for the Encouragement of Arts, &c. to judge of the importance of the idea held out here, not only to the poor, but to the land-holders and the community at large.

It is nearly twelve months since, by analysing its component parts, I discovered hemp in the bean plant. I would have written to you then, Sir, on the subject, and sent a specimen, but that I was trying experiments with other plants, as I am during my leisure hours doing at present; and I wished to ascertain in what degree this species of hemp is liable to injury from different situations, and the changes of the atmosphere. With a view to this, I exposed

one

parcel nearly 12 months, to all the varieties of the air
hin doors, and kept another nearly as long *constantly*
ler water, and find them not in the least injured. The
ef difference I perceive is, that the one kept constantly
ler water, namely the *whitest* of the specimens sent you,
assumed a rich silky gloss, and a much more agreeable
ur than it had before.
ut though this is the case with bean-hemp *after* it is
ned and dressed, and which, though stiff and hard when
, is pliable and easily managed when rather damp or wet,
eems otherwise with it *previous* to its being separated
n the straw. If bean-straw be kept for years under
er, or quite dry, it produces, I find, hemp as good and
h as at first. But if the straw be sometimes wet and
etimes dry, the filaments or fibres are apt to be injured,
specimens of bean-hemp accompanying this letter, in
form of oakum for caulking ships, having been long
osed to the varieties of the weather, previous to being
arated from the straw, is a proof of its being considerably
ired. If the straw of the bean was scattered thin on the
und, and exposed to the weather for two or three
nths, I have uniformly found that the hemp, or fibres,
loosened, and easily separated from the strawy part,
hout any other process than *merely* beating, rubbing and
king them, and perhaps this is the easiest way of ob-
ing bean hemp; but then, from being thus exposed,
the fermentation that takes place in the strawy part,
ch is of a spongy nature, communicating itself to the
es, or hemp, I find that these are generally less or more
red, though not so much so, in my opinion, as to pre-
t them from being excellent materials for making paper.
have also found, and the importance of the idea will,
pe, be an excuse for mentioning it here, that, though
water in which bean straw has been put to steep, in a
days generally acquires a black colour, a blue scum
a peculiar taste, yet cattle drink it greedily, and seemed
ned by it. But my experiments have hitherto been on
limited a scale to be able, in a satisfactory manner, to
rtain this last circumstance. When the water in which
n straw has been put to steep, becomes fœtid, which I
it is *scarcely* more apt to become than common stag-
t water, on being stirred by driving horses or cattle
ugh it, by a stick, or in any other way set in motion,
is the case with all putrid water, even the ocean itself,)
fœtid particles fly off, and the effluvia dies away.
hen straw is to be steeped for bean hemp, the beans

are to be thrashed in a mill: the beans should be put to the mill, not at *right angles*, but on a *parallel*, or nearly so, with the rollers, else the straw, particularly if the beans are very dry, is apt to be much cut. If the straw is *not* to be steeped, on putting the beans to be thrashed at right angles, or nearly so, with the rollers of the mill, a certain proportion of the fibres, or hemp, may easily be got from the straw, these being in general not so much cut as the straw; but often found torn off and hanging about it like fine sewing threads. The hemp thus taken off, though its lying under water for months would do it no harm, requires only to be steeped a few minutes, drawn through a hackle and washed, previous to its being laid up for use. If the hemp, or fibres, collected in this way (which is a fine light business for children, and such as are not able for hard work, and which requires no ingenuity), are intended only for making paper, they require neither steeping nor hacklings, but only to be put up into parcels and kept dry till sent off to the manufacturer.

The straw of beans contains a saccharine juice, and is highly nutritive, perhaps more so than any other; and, like clover, the prunings of the vine, the loppings of the fig-tree, &c. produces a *rich* infusion, and commonly fine table-beer, as well as an *excellent* spirit by distillation. It is the hemp or fibres that prevents cattle from eating it. These, like hairs in human food, make cattle dislike it. The collecting of it, therefore, should never be neglected, nor the boys and girls in workhouses and other places be permitted to be idle, while business of this kind would evidently tend both to their own and their employers' advantage.

It is a fact, that about the generality of mills for beating and dressing hemp and flax, a large proportion, in some inland places both of Great Britain and Ireland, amounting nearly to one-half of what is carried thither, is either left there to rot, under the name of refuse, or thrown away as of no use, because too rough and short for being spun and converted into cloth. Now, from the experiments I have tried, and caused to be tried, I have uniformly found, that though too rough and short for being converted into cloth, even of the coarsest kind, the refuse of hemp and flax, on being beat and shaken, so as to separate the strawy from the stringy particles, which can be done in a few minutes by a mill or hand-labour, as is most convenient, becomes thereby as soft and pliable, and as useful for making paper, as the longest, and what is reckoned the most valuable part of the

plant,

ant, after it has been converted into cloth and worn
ars.

In its natural state, it is true the refuse of hemp and fl
generally of a brown and somewhat dark colour. B
hat of that? By the application of muriatic acid, oil
triol, or other cheap ingredient, well known to the ch
ist, as well as to every bleacher, such refuse, withc
ing *in the least* injured for making paper, can, in a f
urs, if necessary, be made as white as the finest cambr

There are, at a medium, published in London, eve
orning, 16,000 newspapers, and every evening ab
,000. Of those published every other day there are ab
,000. The Sunday newspapers amount to about 25,0
d there are *nearly* 20,000 other weekly papers, maki
all the enormous sum of 245,000 per week. At a
um 20 newspapers are equal to one pound—hence
hole amount to about 3 tons per week, or 260 tons
nnum. But though this, perhaps, is not one-half of
per expended in London on periodical publications, a
hat may be called fugacious literature, and not one-fou
rt of what is otherwise consumed in printing-houses
e country at large, yet there are materials enough in
fuse of the hemp and flax raised in Britain and Ireland
l this and much more.

Nor is this all: for as the bine or straw of hops, a c
mstance well known to the Society, contains an excell
emp for making many articles, so also will it prove a m
cellent material for making all kinds of paper. And i
fact, that were even the one-half of the bine of hops rai
the counties of Kent Sussex, and Worcester, instead
ing thrown away, or burnt, after the hops are picked,
commonly done, steeped for ten or twelve days in wat
d beat in the same way as is done with hemp and flax,
pendent of what might be got from bean-hemp, an
riety of articles well-known to the Society, there wo
found annually materials enough for three times the qua
ty of paper used in the British dominions.

I have the honour to be,
with much respect,
Sir,
Your most humble servant,
JAMES HALL.

Streatham, Jan. 9, 1809.

C. TAYLOR, M. D. SEC.

Certifica

Certificates of the Truth of the foregoing Statement.

Streatham, Surry, Jan. 9, 1809.

We, the undersigned, do hereby certify, that the speciments of hemp inclosed and sealed up by us, addressed to Dr. Taylor, secretary to the Society for the Encouragement of Arts, Manufactures, and Commerce, Adelphi, Strand, are the produce of common bean straw :—That we never saw nor heard of bean hemp till lately ; when the Rev. James Hall, who resides here at present, was trying experiments respecting it at Mr. Adams's farm, Mount Nod, and other parts of this parish :—That, in the present obstructed state of commerce with the continent, it appears to us the discovery of bean hemp may be extremely useful to the manufacture of canvass, ropes, paper, &c. ;--and that, as it affords a new and important prospect of employment for the poor, we think Mr. Hall, the discoverer, is deserving of the approbation of the public. We shall only add, that as the Society for the Encouragement of Arts, Manufactures, and Commerce, have contributed so often in a high degree to the exertion of genius, the improvement of the arts, and the public good, we have no doubt but they will not only take the proper steps to prosecute the discovery and encourage the manufacture of bean hemp, but also, by some mark of their favour, show their approbation of Mr. Hall's merit in the discovery he has made, as well as of his high public spirit and liberality in communicating the discovery to the public without reserve.

William Adams, Mount Nod.
Edward Bullock, Curate.
Wm. Gardner, Surgeon.

Streatham, Surry, Jan. 9, 1809.

These are to certify to the Secretary of the Society for the Encouragement of Arts, &c. London, and all whom it may concern, that having seen (at first to our astonishment) the Rev. James Hall, who has resided here for some time past, procuring hemp from common bean straw, steeped some days in water, we steeped some also, and easily got hemp from it ; there being no mystery in the matter more than *merely* steeping the straw, peeling off the hemp, and then washing and cleaning it, by pulling it through a hackle or comb.

These are also to certify, that having tried bean hemp, and

l found it to take both wax and rosin, we have sewed
h it, and find the fibres of which it consists in general
strong, that the leather never failed to give way sooner
n the seam. We have only to add, that as hemp has of
: become uncommonly dear, while much of it is bad,
anxiously wish the prosecution of the discovery, and
appearance of bean hemp in the market; and shall, so
n as we hear of its being spun and on sale, be among
first to purchase and use it.

John Hounk, Shoemaker.
Thomas Alford, Shoemaker.

Letter from Mr. Hume, of Long Acre, to the Rev. James Hall.

:, I inclose a specimen of the bean filaments or thread
ich have been submitted to the bleaching process. The
ture and strength seem not in the least to have been im-
:ed, but retain the primitive tenacity; and I am persuaded
; substance will prove an excellent substitute for hemp
l flax, for the manufacture of various kinds of paper,
dage, and other materials. I did not find more difficulty
accomplishing the bleaching of this than in other vege-
les which I have occasionally tried, and I believe this
cle is susceptible of a still greater degree of whiteness.

I remain, sir,
Your very obedient servant,
Jos. Hume.

g Acre, Feb. 24, 1807.

Letter from Mr. H. Davy to the Rev. James Hall.

:, I shall inclose in this paper a small quantity of the
n fibres, rendered as white as possible by chemical means.
t seems to bear bleaching very well; and, as to chemical
perties, differs very little from hemp.

.he question, Whether it is likely to be of useful applica-
n, is a *mechanical* one, and must be solved by experiments
its comparative strength. I am, sir,
Your obedient humble servant,
H. Davy.

XXXIII. On

XXXIII. *On Crystallography. By M.* Haüy. *Translated from the last Paris Edition of his* Traité de Minéralogie.

[Continued from p. 158.]

OF MINERALOGICAL METHODS.

All the productions of nature, considered in the point of view in which she presents them directly to our eyes, form a picture complicated with a multitude of details, in the midst of which the eye is lost at the first glance, and sees every thing at once without distinguishing any thing.

With the view of facilitating the study of this picture, there have been contrived, with respect to mineralogy, as with zoölogy and botany, methodical distributions of the subjects which are therein embraced; their different parts have been dissected in imagination, so as to form a kind of factitious table, with which we may afterwards compare the former, and which serves it as a kind of explanation.

However slightly we reflect on the progress of these methodical arrangements, we may easily perceive that they are founded on the faculty possessed by the human mind of regarding certain qualities in an object, by abstracting others; and of raising ourselves gradually from particular to general ideas.

Thus, when speaking of an oak as a determinate object which I can point out with my finger, I make no abstraction; I consider in the object which I name, all the qualities that can accord with it: in a word, I designate an individual, *i. e.*, a being which has a particular existence. But if, in pronouncing the word OAK, I have not seen any particular oak, then I abstract the idea of a particular existence; I designate in general a collection of individuals similar in all their parts, and this collection is what is called a SPECIES.

The sense in which I have taken the word Oak (or *Quercus*) is that which every body attaches to it in ordinary language. Now, on comparing the individuals of the species in question with those of another species, to which the name of Holm Oak (*Ilex*) has been given, I remark that the latter have the organs of the flower similarly constructed, and that their fruits are acorns also; but that they differ from each other in several respects, and particularly in the form and consistence of the leaves; which in the former are broad, soft, and terminated by round lobes, and in the latter narrow and indented at the edges. I can, therefore, fix my attention solely on the resemblance of the flower

[illegible] individuals of the two species,
[illegible] all the parts which
[illegible] nomenclature to this
[illegible] my mind. I shall extend
[illegible] species. Returning my mind
[illegible] one side,
[illegible] them in language, distinguish-
[illegible] *mmon oak* the individuals of the
[illegible] name of *green oak* those of the
[illegible] have a genus, of which the common
[illegible] will be two species.

[illegible] can consider in the two oaks no-
[illegible] neous consistence, and the faculty
[illegible] a certain number of years; and
[illegible] kinds of productions, different from
[illegible] consistency and are very long lived,
[illegible] species are of lower stature,
[illegible] year or two only, I shall unite, un-
[illegible] idea, the first by the name of *trees*,
[illegible] common all the others by the
[illegible] thus have two great classes*, each
[illegible] into a certain number of genera,
[illegible] species. Finally, if I have no
[illegible] faculty which all these ob-
[illegible] of being nourished from the
[illegible] include them under the general
[illegible] I shall thus attain, by a series of
[illegible] the most elevated point of view
[illegible]

[illegible] a host of examples of similar
[illegible] spirit of analysis has suggested
[illegible] by directing their views in the
[illegible] have formed their systems
[illegible] merely subjected these methodical
[illegible] and more rational principles;
[illegible] divisions and subdivisions, and
[illegible] arranged them by the indication of
[illegible] the objects which each division
[illegible]

[illegible] precedes, that in proportion as we
[illegible] abstractions, we connect together
[illegible] according to the relation or
[illegible] degree of abstraction. Thus the

[illegible] rigorous limits here between the divisions
[illegible] of the progress of ideas by examples
[illegible]

idea which the word *tree* expresses, embraces incomparably more plants than that which is attached to the word *oak*, and the latter has a greater latitude than the idea presented to the mind by the word *green oak*. Reciprocally, every abstraction from an inferior degree compresses into a smaller space the number of the objects to which it is extended. What does methodizing effect then?—It divides and subdivides successively the assemblage of objects, according to their various characters or relations, so that, at every division, all the characters enunciated in the preceding divisions being regarded as still subsisting, the method adds the expression of a new character, a new trait of resemblance, which detaches the objects contained in this division. The more the sum of the relations increases, and the more on the contrary the number of objects with which these relations agree are diminished, and when this sum is the greatest possible, when it is extended to all the faces of the objects which it includes, each of these objects is considered as representing all the others, and we say that all these objects are of the same species.

On the other hand, in proportion as the degree of abstraction is raised, the number of subdivisions which answers to this degree diminishes: and it was this manner of regarding methodical order that the illustrious Bacon had in view, when he compared Nature to a pyramid the base of which was occupied by an almost infinite number of individuals; above this base rise the species formed by the assemblage of individuals, and which are consequently extended over a narrower space than the base; afterwards come successively the genera composed of species, then other superior genera (which answers to our orders and classes); until Nature, after having become narrower and narrower, terminates in a point, or in unity*.

We may also be able to see that the character which served to connect with each other the productions of one and the same division, distinguished them from those of another division. Hence, and from all that precedes, result two remarkable advantages of the method. The first is, to make us acquainted with objects not only by themselves but also by comparison, each of them being placed by the method in such a manner that it turns in some measure towards the rest the side in which it resembles them, and presents in an opposite direction that by which it is distin-

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* Bacon, *De Augment. Scient.* t. ii. c. 13. See the work which has for its title "Le Christianisme de François Bacon." Paris, an 7, t. i. p. 1.

guished

ished from them. The second advantage is, that after e have been exercised in making applications from the ethod to a certain number of objects already known, we ay attain a knowledge even of that which would be new us, by consulting successively the characters which ac- ompany each division, and by making use of the method inquire into the subject, and to learn from the object it- lf the place which it occupies in the method.

The series of divisions and subdivisions in mineralogical stributions is nearly the same as in those which regard ganic bodies. This series, taken by descending from nerals to particulars, gives the following gradation: asses, orders, genera, species, varieties. But there is a nsible difference relative to the methods used in these o departments of science, relative to the manner in which e consider objects, or to the choice of the methods em- oyed to classify and characterize these objects.

Thus, in botany, we call ***species*** the succession of plants hich reproduce each other. In mineralogy, there is nei- er reproduction nor species, if we take this term in a gorous acceptation. There is nothing, however, to hinder from following the example of Linnæus, Bergman, and veral other celebrated naturalists, in applying the word *ecies* in a wider sense, to an assemblage of inorganic ings which have a common basis, and the differences of hich ought to be regarded as purely accidental.

But this leads us to an important question, to which it es not seem that sufficient attention has hitherto been id. In what consists in the present case the type of the ecies;—and when are we justified in regarding several mi- rals as belonging to one and the same species*? It ems at first view as if chemical composition was the basis this union; so that the true notion of the species con- sts in conceiving an assemblage of minerals formed of e same principles united to each other according to the me laws. But we shall see how much this idea is sus- ptible of restriction, and to what point even we should nder from our object, in a multitude of circumstances, taking it for our guide, in assembling of varieties which ght to bear one and the same specific name.

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* I shall by and by recur to the reasons which induced me to apply this rd, rather than that of genus, to the different objects which in the language eneral among naturalists have a common name, such as *topaz*, *emerald*, [illegible] &c. or, if an [illegible] substance is alluded to, the term *carbonated* [illegible] gas, &c. It is sufficient for my purpose at present to int out such of the divisions and subdivisions of the method to which I e the denomination *species*.

In order that I may be better understood, I shall take an example from feldspar. Mr. Kirwan, to whom we are indebted for a treatise on mineralogy, in which that celebrated author has brought together, in the development of the science, the external characters of minerals and the results of his own researches as well as of those of other chemists, as to the composition of these bodies, cites 13 analyses of the substance in question, to which we may add a 14th made by Vauquelin. Now, not only do the products vary among each other in the proportions of the same principles, but there are ingredients which are found in certain products, and not in others. Thus Mr. Kirwan has procured from a reddish feldspar eleven per cent. of barytes and eight of magnesia; whilst the result obtained by Wieglieb in another feldspar of a red colour, furnished neither of these earths, but only silex and alumine, with a small quantity of oxide of iron and fluoric acid. Vauquelin found about one seventh of potash in the feldspar called *adular*, and in the green feldspar of Siberia, and yet no other analysis has presented this alkali. Besides, this expert chemist has discovered neither magnesia nor barytes in the same mineral.

Mr. Kirwan concludes, from the different analyses quoted by him, that every compound of silex and alumine (the silex being predominant) to which is added a slight proportion of lime and magnesia, or of lime, of magnesia and barytes, (but sufficient to render the whole fusible at a degree of heat not exceeding 140°,) would form a feldspar; and we ought not to hesitate in giving it this name, if at the same time it presents a lamellous texture. But he adds that iron seems in this case to be an accidental principle.

I do not observe that this rule laid down by Mr. Kirwan leaves any thing to be desired, so far as simplicity and precision are concerned; but in spite of the efforts of the author to render it general, at the hazard of loading it with conditions, it is no longer applicable to the result of the analysis made by M. Vauquelin of the feldspar known by the name of *adulary*; and finally, if we should undertake to give similar rules for all minerals, the result would be a complication from which it would be difficult to extricate ourselves, and it would even very probably, happen, that a rule which should have for its object such a particular species might be applied nearly equally well to an entirely different species.

I shall not examine if all the analyses alluded to by Mr. Kirwan deserve an equal confidence. But we may at least co-

nclude that they indicate perceptible differences of con
sition between the specimens analysed. I could produ
her examples of a mineral, the different analyses of whi
ade by skilful operators have given different products ; a
shall presently see that this must necessarily take pla
a variety of circumstances.

Now I return, and I ask, on what foundation Mr.Kirw
es the name of feldspar to the various specimens whi
ve been the subjects of the analyses above mentione
is intimacy surely is not founded on the results of the
alyses ; since we should rather be inclined to infer, fro
differences which they have presented, that a few
st of the substances to which they are referred, co
tute distinct species. In a word, it is visible that M
rwan has tacitly supposed that, abstracting all consider
n of the analyses, the substances in question had be
arded as feldspars.

On perusing what has been written by naturalists on t
bject of this mineral, we see that its place was assign
cording to a certain assemblage of characters, such as
rdness capable of producing sparks with steel ; a lea
xture added to its breaking into rhomboidal fragments
specific gravity of about 2·5 ; a fusibility into a whi
amel ; &c.

But these characters are for the most part variable to
rtain point ; and this variation may even be extended f
ough, in certain cases, by a consequence of that whi
component substances undergo. These are so ma
eful ways of assisting us to recognize bodies which b
ng to one and the same species ; but in addition to the
t presenting to the mind a sufficiently simple and preci
a of what constitutes this species, their results are n
ays proper for tracing the limit which separates o
ecies from another : and it is on account of our being r
icted to consulting them only, that we have confound
pyroxene with the amphibolus, the chabasie wi
mesotype, and so on with several other relative co
xions, the fault of which will become evident from wh
shall say under the respective heads of the substanc
which they are referred.

There exists a character much more solid and mu
re proper by its invariability, to serve as a rallying poi
the different bodies, which belong to one and the sam
ecies. This is derived from the exact form of the int
ant molecule, because this form subsists, without a
sible alteration, independently of all the causes whi

m

may cause the other characters to vary. Thus, in order that we may not quit the example of feldspar, such in this substance is the arrangement of the natural joints, that the molecule resulting from it is an oblique-angled parallelopipedon, in which the three plane angles which concur to the formation of one and the same solid angle, form among them a first angle of 90°, a second of 120°, and a third of 111° and a half; and these angles will be constantly the same in specimens variously crystallized; in those which will give by analysis barytes or potash, as well as in those which will exhibit no vestige of it.

And not only may we estimate by observation combined with theory the angles of the integrant molecule, but we even succeed in ascertaining the relations between its dimensions, and there results a geometrical form perfectly determinate, which is the same in all the individuals of the species, and which presents as it were a fixed point in the midst of the oscillations of all the other characters; so that we may even say, that in general the bodies of each species touch closer in the results of the theory relative to their structure than in those of chemical analysis.

I do not pretend to raise the character I have just mentioned above its real importance. I am even led to suspect the predilection which I ought naturally to have for that character which belongs to a branch of mineralogy which I have cultivated with particular care. But this predilection does not prevent me from stating a truth which I think useful to the progress of science; which is, that this character, borrowed from the structure, ought to have a great influence in the distinction of the species; and by neglecting it we deprive ourselves of one of the most advantageous methods for the formation of an exact and regular system.

It may be objected, that the determination of the integrant molecule of a body is frequently a delicate operation, which requires minute experiments, and besides presupposes a knowledge of calculation which the whole world is not in possession of. But chemical analysis has also its difficulties, and is not the affair of a moment. It requires a good deal of art to employ the most proper agents for seizing and coercing principles invisible, and, if we may be allowed the expression, impatient to escape from the hands of the chemist: it requires art also that the operation may take nothing from the result that belongs to it, and add nothing that is foreign to it; and sometimes it is only by repeatedly recurring to the subject, that we succeed in forming the faithful picture of the relations which existed among these various principles combined with each other in the substance as yet untouched. We never purchase at too high a price what contributes to the perfection of a science, and we ought not to calculate time when it is necessary to arrive at immutable truths.

It will perhaps be said also, that a mineral substance is found in compact or granulous masses, which refuse to submit to any mechanical division. I shall answer, that frequently also these masses form a continuity with crystallized substances, or such as have a lamellated texture, in such a manner that it is visible that they are referred to the same species; and as to those which we meet with in an insulated state, if nothing more is then wanted to determine them, except characters less certain than that which is derived from the structure, it merely results that we ought to regret that this last character has not a greater generality; and this very regret is a kind of avowal of its preeminence, n every case in which it can be employed*.

Will it be said that there are forms of integrant molecules which are common to substances of different natures? I shall observe, in the first place, that this does not take place except with respect to solids which have a peculiar character of regularity, in such a manner that in all the other cases the form of the integrant molecule is sufficient of itself for determining the species. I shall answer afterwards, that most of the substances which have one common molecule (and this may be said of all those which, ike the ductile metals, never have a lamellous texture) may be easily distinguished by other characters. For example, the cube agrees best as the integrant molecule with borated magnesia, muriated soda, sulphurated lead, sulphurated ron, &c.; all of them ascertainable independently of mechanical division.

In a word: all that I wish to infer from this discussion s, that the character drawn from the structure ought to occupy a very distinguished rank among those which are made use of to mark the general character (*triage*) of original bodies of one and the same species. It certainly has its obscure sides, and there are circumstances in which it disappears. But wherever it shows itself, there is a ray of light against which we ought not to shut our eyes.

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* Sometimes the rareness of crystals is alleged as a proof of the scanty resources furnished by the character drawn from crystallization. This difficulty does not seem to be well founded, since a single crystal clearly deined, is sufficient to determine a multitude of irregular masses, which, with his crystal, would have relations indicative of an identity in nature: and if, by objecting that crystals are rare, it is meant to say that there are several mineralogical species which are never presented under crystalline forms, shall ask if these are properly species *per se*, and not on the other hand mixed species, in the production of which different species have concurred?

I shall add, that, with a little expertness in applying calculation to theory, we may decide if a given form enters into, or ought to be excluded from, any given species. Thus we shall find that the cube which has been quoted among the varieties of carbonated lime is foreign to the crystallization of this substance*. Now it is easy to see of what service this adaptation of calculation must be, in order to separate the crystallized minerals into their respective species, assigning to each what belongs to it, and freeing it from what it might have usurped.

All that precedes leads us to an interesting consideration relative to the chemical composition of minerals: viz. that the principles which concur to form their integrant molecules must, as I think, be constant, both as to their quantities and their qualities, in such a manner that the substances which cause a variation in the products of the analysis are foreign to the molecules, and merely interposed among them in the mass of the mineral†. We may compare a substance mixed with these additional principles to certain salts with which other salts are accidentally united, as is the case with nitre of the first formation. When we make this salt undergo successive solutions and crystallizations in order to purify it, the liquid in no respect alters the figure of its molecules; it only separates them from each other, and frees them from those of the other salts which were associated with them, and which had gone for nothing in their composition. In the same way the principles on which the differences between the analyses of various pieces of one and the same mineral depend, merely form with the substance peculiar to the latter a simple mixture, from which the integrant molecules would come out untouched, if power were not given to us to regulate, if we may be allowed the expression, their departure‡.

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* Vide under the head Carbonated Lime, the variety which we have called *cuboid.*

† I even think that, in the case in which we say that there is an excess of one of the principles in other respects essential to the composition of a mineral, the superabundant part goes for nothing in the formation of the molecule, and ought to be ranked among heterogeneous and purely accidental principles.

‡ These are the accidental principles which produce a variation in certain external characters, such as colour, lustre of external surface, of fracture,

From these considerations it appears to me that we
ay define a species in mineralogy to be, *A collection of*
odies, the integrant molecules of which are similar, and com-
osed of the same elements united in the same proportion.
his last condition generalizes the definition, and extends
to substances which, having their molecules of the same
onfiguration, differ essentially in the principles which
ompose these molecules.

Some mineralogists are of opinion that these collections
hich I have called species ought rather to be regarded as
enera. But where then would be the species which
ould subdivide the genus? Would these be crystals of
ifferent forms? It seems to me that these modifications,
hich in truth are the results of so many determinate laws,
ut which after all belong only to local circumstances, such
s the density or other qualities of the liquid,—do not fur-
ish a sufficient reason for establishing specific distinctions
etween them. They do not touch the substance, and are
onfined to the giving of different envelopes to one and the
ame nucleus. Besides, on the supposition in question we
hould be embarrassed with unshapen masses, which surely
o not deserve to be erected into species. The same an-
wer applies to the hypothesis on which the species would
orm groups, one of which, for example, would compre-
end bodies regularly crystallized; a second, concretions,
&c. But let us conceive that the molecules which pro-
uced the concretion had been freely suspended in a tran-
uil liquid; they would have assumed another arrange-
ment, and might have formed crystals. The idea originated
y the word *species* goes straight down to the bottom of
he substance, and does not stop at simple windings.

We should not be better founded in regarding as so many
pecies the mixtures of a substance with accidental prin-
iples, which only modify the principal species, but do not
ransform it into another which may be really distinguished
rom it. Even these mixed bodies only belong to the

---

ure, &c. Thus we find at mount Vesuvius crystals of pyroxene (Augit of
Werner), the surface and fracture of which have a very shining appearance,
whereas amongst those of Norway several have a rough and dull surface,
nd their interior is not very shining. Nevertheless both are divided under
he same angles, and have forms either similar, or which may be referred
o the same molecule; so that the species to which they belong preserves its
nity, in spite of the differences of aspect which we have mentioned; and
onsequently these differences are all of them at most the index of a varia-
ion in the principles which concur in any given way to the composition
f the substance, but they do not announce any variation in the essential
nd truly constituent principles.

principal species, because they admit of the existence, at least in part, of the predominant characters. If this was not the case, they ought no longer to occupy a place in the method: they ought to be thrown into the general appendix, in which are placed the mixed substances called *rocks*. And this shows how contrary it is to the spirit of the method, to place on the same line (as so many particular species) with the true species, marles, argils, schists, and other bodies, which are only fortuitous aggregates of species already classed in other places in the method, and no one of which imprints its character on the whole; so that we are even unable to decide to what species they ought to be referred, as being but a simple appendage.

From all that I have said, we shall easily conceive how important it would be to determine by the aid of analysis, (with respect to each species) those principles which concur by themselves to the formation of the integrant molecule, by operating on picked pieces, the composition of which contains only what cannot be dispensed with, without ceasing to be what it really is, and which had borrowed nothing, if we may be allowed the expression, from the liquid in which it had originated. We should thus have the limit from which the analyses of other pieces remove more or less, according as the latter contain principles purely accidental, or if one of the constituent principles is found there in excess. This limit would give what would be called the analysis of the mineral submitted to experiment, and the other results would make known the accidental diversities of which the composition is susceptible: they would serve to indicate to what term a certain principle has varied in its proportions, and to unveil the principles which have only a transitory existence, and are rather a surcharge with respect to the mineral which contains them, as they do not contribute to its integrity.

I think I have sufficiently shown how much strength may be acquired by chemistry and mineralogy by their mutual cooperation. Without the former we should be ignorant in what class a mineral ought to be placed if it contains an acid, or if earths only enter its composition, or if it does not conceal a metallic substance under the appearance of a simple stone. Without the latter art, it would frequently be difficult to refer to the type of the species the varieties which appertain to it. The one indicates the first link of the chain, and marks the point to which it ought to be attached; but the intervention of the

ier is necessary for continuing this chain and arranging
different links of it.

I trust that this long discussion will be forgiven. I
ought it necessary, because it appeared to me that at no
ne has the influence been sufficiently acknowledged,which
neralogy ought to have on a well-arranged method ; and
cause, if there are cases in which the mineralogist cannot
rain from saying to the chemist, *Make me acquainted*
th the substance which you have analysed,—there are
ers in which the chemist, if he is cautious, ought to
to the mineralogist, *Make me acquainted with the sub-*
nce which I have analysed.

M. Vauquelin, who adds to dexterity of operating a great
curacy of reasoning, has proved more than once, that he
l not regard the consequences inferred from the geometry
crystals as useless in assisting us to fix the term at which
analysis ought to begin. Placed by circumstances
side each other, we have frequently consulted together
the subject of our inquiries, and the results which
attained, by two methods of interrogating nature
different, were mutually of service as guarantees by
eir conformity. I feel strongly the advantages which
s cooperation has produced, and I am anxious that it
ould be publicly known, that it is in the *Ecole des Mines*
France that chemistry and crystallography, so long se-
rated, have contracted a strict alliance, which bids fair
be of long duration.

If we now resume the comparison between the me-
ods of botanists and those of mineralogists, we may
serve that the former are entirely founded on charac-
s furnished by observation of the external form, but
latter having a necessary connexion with the internal
ganization, which is constant in all the individuals of
e and the same species, each of them may serve as a
mmon model, in order to paint as if at a single stroke
entire species.

In the mineral kingdom, on the contrary, in which the
ernal characters undergo continual variations, in which
en the best defined forms are only evanescent disguises,
thing that speaks to the eye can serve as a foundation to
method. It belongs to analysis to lay this founda-
n, and to regulate the order of classification, making use
all occasions of the light afforded by crystallography.
t this object once attained, the observer must be able to
ertain the substances classified by methods independent
of

of analysis, by employing a choice of characters happily combined, some of which are presented as of themselves to our senses, and the others only require, for being regularly defined, prompt and easy operations.

Here crystallography, which had seconded analysis in the formation of the method, will reappear to advantage, in order to furnish characters founded on the angles of crystals, which may be measured in an instant.

Thus the means which have presided in the composition of the method will be at the same time the most solid, and those which will be the least arbitrary, and the means which shall direct the use of the method will have the merit of simplicity and convenience.

It may be seen from what precedes, that a mineralogical system, being complicated with characters frequently borrowed from very different considerations, requires more cautious proceeding in the person who employs it than a botanical system, the uniform and regular progress of which is traced according to a modification which speaks to the eyes, in such a manner that all the labour of the observer is confined to the different applications of one and the same principle. But this inconvenience, if it be one, is compensated in a great measure by the advantage of only having to determine among a number of species incomparably less than that which botany embraces: and the method being in this respect confined in a narrower circle, the observer finds his way more easily in the midst of the circuits which he is obliged to take in order to attain his object. We may add, that even the variety of the characters which he associates in one and the same inquiry, and the various kinds of knowledge which he combines with the observations of what is presented to his senses, contribute to give additional interest to the study of the objects which interest his mind, and to render this study at once more gratifying and instructive.

We have given in the preliminary discourse a detailed explanation of the method which we have adopted. The plan of it has been conceived in such a manner that, without suffering ourselves to fabricate connexions disavowed by nature, with a view to anticipate the results of analysis, we might profit by all those which should admit of our establishing a regular distribution of those parts of the method, in which the labours of the chemists have procured more certain and better connected information. We have divided the whole mineral kingdom into four classes, the titles of which we here place before the reader.

1. Acidi-

. Acidiferous substances ; composed of an acid united
n earth or an alkali, and sometimes to both.

. Earthy substances ; into the composition of which
hs only enter, united sometimes with an alkali.

. Combustible substances (not metallic).

. Metallic substances.

Ve have also explained the reasons which induced us
hoose the base rather than the acid for characterizing
genera of the first class, and those to which metallic
stances belong, which have also an acid for one of their
ıponent principles. Hence, in the last place, the ne-
ity of modifying, by the help of a very simple inversion,
specific names adopted by modern chemists, when they
sulphated lime instead of *sulphate of lime* ; *carburetted*
i, instead of *carburet of iron*, &c., in order to modify
nomenclature to the system itself, and to preserve that
cision and regularity so happily introduced by Linnæus
the language of natural history.

he care which we have taken to restrict our method to
it is founded on exact and precise experience, dictated
t we should exclude certain substances, the nature of
ich is not yet sufficiently verified to admit of our de-
ing whether they constitute distinct species, or if they
y be referred to any of the species already classified.
shall arrange these substances in a first appendix, in
ich we shall give the description of them, detailing the
mises which their characters may have already originated,
o what they would become if better known. A second
endix will contain the substances which are only ad-
tures of different species, and among which are found
aggregates known by the name of rocks, and which
shall therein more particularly describe. Lastly ; there
l be a third appendix for the productions of volcanos,
l for those of subterranean fires, but not volcanic.

After all it must be remembered, that our methods, even
en best managed, represent nature but imperfectly ; and
er having conducted us a certain length they leave us
ourselves. It should seem as if they had only been
nposed from selected pieces, in which the characters
ich they indicated had a clearer or better pronounced ex-
ssion. Incomplete as they are, however, they have the
cious advantage of putting our ideas in order, and pre-
e us for a more detailed study of their object. The in-
uction having been once begun by the exact application
the principles to selected specimens, is more easily
l more happily terminated, by the close observation of
every

every thing which may be presented in nature; and the art of advantageously applying the method, leads an expert eye to find the art of knowing how to dispense with it altogether.

[To be continued.]

XXXIV. *On the Identity of Columbium and Tantalum.*
By William Hyde Wollaston, *M.D. Sec.R.S.*[*]

Within a short time after the discovery of columbium by Mr. Hatchett in 1801[†], a metallic substance was also discovered in Sweden by M. Ekeberg[‡], differing from every metal then known to him; and accordingly he described the properties by which it might be distinguished from those which it most nearly resembled. But although the Swedish metal has retained the name of Tantalum given to it by M. Ekeberg, a reasonable degree of doubt has been entertained by chemists, whether these two authors had not in fact described the same substances; and it has been regretted that the discoverers themselves, who would have been most able to remove the uncertainty, had not had opportunities of comparing their respective minerals, or the products of their analyses.

As I have lately obtained small specimens of the two Swedish minerals tantalite and yttro-tantalite, from which I could obtain tantalum, and was very desirous of comparing its properties with those of columbium, Mr. Hatchett very obligingly furnished me with some oxide of the latter, which remained in his possession.

The resemblance was such in my first trials, as to induce me to endeavour to procure a further supply of columbium; and by application to the trustees of the British Museum, I was allowed to detach a few grains from the original specimen analysed by Mr. Hatchett.

Notwithstanding the quantity employed in my analyses was thus limited, I have, nevertheless, by proportionate œconomy of the materials, been enabled to render my experiments sufficiently numerous, and have found so many points of agreement in the modes by which each of these bodies can or cannot be dissolved or precipitated, as to prove very satisfactorily that these American and Swedish specimens in fact contain the same metal: and since the

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[*] From Phil. Trans. 1809, Part II.

[†] Phil. Trans. for 1802.

[‡] *Vetenkaps Academiens Handlingar.* 1802, p. 68.—*Journal des Mines*, vol. xii. p. 245.

re-agents

agents I have employed are in the hands of every che-
t, the properties which I shall enumerate are such as
l be most useful in the practical examination of any
er minerals in which this metal may be found to occur.

n appearance the columbite is so like tantalite, that it
xtremely difficult to discern a difference that can be
ed upon. The external surface, as well as the colour
lustre of the fracture, are precisely the same; but co-
bite breaks rather more easily by a blow, and the frac-
of it is less uniform, appearing in some parts irre-
rly shattered; nevertheless, when the two are rubbed
nst each other, the hardness appears to be the same,
the colour of the scratch has the same tint of very
brown.

y analysis also, these bodies are found to consist of
same three ingredients; a white oxide, combined with
and manganese.

ither of these minerals, when reduced to powder, is very
lily acted upon by potash; but as the iron contained in
n is not affected by alkalies, it appeared better to add
all proportion of borax.

ive grains of columbite being mixed with 25 grains of
onate of potash and ten grains of borax, were fused
ther for a few minutes, and found to be perfectly in-
orated. The colour was of a deep green, from the
ntity of manganese present. The mass when cold
d be softened with water, and a portion of the oxide
ld be so dissolved; but it seemed preferable to employ
te muriatic acid, which, by dissolving all other ingre-
ts excepting columbium, left the oxide nearly white,
he removal of iron and manganese that had been com-
d with it.

he muriatic solution having been poured off and neu-
zed with carbonate of ammonia, the iron was then
rated by succinate of ammonia; after which the man-
se was precipitated by prussiate of potash.

he products thus obtained from five grains of columbite,
each had been heated to redness, were nearly,

White oxide	4 grains
Oxide of iron	$\frac{3}{5}$
Oxide of manganese	$\frac{1}{5}$;

it cannot be supposed that *proportions* deduced from
riments made on so small a scale can be entirely de-
led upon, although the *properties* of bodies may be so
erned, nearly as well as when larger quantities are em-
ed.

An equal weight of tantalite taken from a specimen, of which the specific gravity of 7·8, yielded, by the same treatment,

White oxide	4¼	grains.
Oxide of iron	½	
Oxide manganese	$\frac{2}{10}$.	

The white oxides obtained from each of these minerals are remarkable for their insolubility in the three common mineral acids, as both Mr. Hatchett and M. Ekeberg have observed.

In muriatic acid they cannot be said to be absolutely insoluble; but they are not sufficiently soluble for the purposes of analysis.

In nitric acid they are also nearly, if not perfectly, insoluble.

In sulphuric acid, when concentrated and boiling, the oxide of columbium may be dissolved in small quantity, and so also may the oxide obtained from tantalite.

The proper solvent, as has been observed by Mr. Hatchett and by M. Ekeberg, is potash; and as it is not required to be in its caustic state, I employed the crystallized carbonate of potash on account of its purity and uniformity. Of this salt about eight grains seemed requisite to be fused with one of the oxide obtained from either of these minerals to render it soluble in water.

Soda also combines with the oxide, and may be said to dissolve it; but a far larger proportion of this alkali is necessary, and a larger quantity of water. And although a solution may have been effected that is transparent while hot, it very soon becomes opaque in cooling, and finally almost the whole of the oxide subsides combined with a portion of the soda in a state nearly insoluble.

When a solution of the white oxide, obtained from either of these minerals, has been made, as above, with potash, the whole may be precipitated by the addition of an acid, and will not be redissolved by an excess of sulphuric acid, of nitric, of muriatic, succinic, or acetic acids.

But there is a further agreement in the properties of these two minerals, which appears above all others to establish their identity; for though they are both so nearly insoluble by any excess of the mineral acids, yet they are each completely dissolved by oxalic acid, by tartaric acid, or by citric acid; and the solution of each is subject to the same limitations; for if the precipitate has been dried, it is become intractable, and can scarcely be dissolved again till after a second fusion with potash.

f to the alkaline solution of either of them there be
ed infusion of galls, prussiate of potash, or hydrosul-
ret of potash, no precipitate occurs; but when a suffi-
it quantity of acid has been added to neutralize the re-
dant alkali, the infusion of galls will then occasion an
nge precipitate; but prussiate of potash causes no pre-
tate, nor does the hydrosulphuret precipitate the oxide,
ough the solution may become turbid from precipita-
ı of sulphur by a redundant acid.

'he characteristic precipitant of columbium is conse-
ntly the infusion of galls; but in the employment of
test certain precautions are necessary. For as an ex-
s of potash may prevent the appearance of this preci-
te, so also may a small excess of oxalic or tartaric acids
vent precipitation, or dissolve a precipitate already
ned. A larger excess of citric acid seemed requisite for
purpose, and would also dissolve the gallat of colum-
n. In each case the precipitate may be made to ap-
r by neutralizing the redundant acid; and for this pur-
e carbonate of ammonia should be employed: for al-
ugh pure ammonia has no power of dissolving the oxide
ie, yet the gallat seemed to be perfectly redissolved by
alkali.

Vhen infusion of galls is poured upon the white oxide
ntly precipitated, and still moist, it combines readily,
forms the orange-coloured compound.

'russiate of potash occasioned no change in an oxide
t had been purified by a second fusion with potash; but
ppeared to dissolve a small portion of the oxide, as in-
ion of galls, poured into the clear liquor, occasioned a
ıdy precipitate of an orange colour, though no such
cipitate took place when the infusion was mixed with
same prussiate alone.

Iydrosulphuret of potash being added to the oxide, and
ted upon it, impaired the whiteness of its appearance,
seemed to detect the remains of some impurity which had
yet been removed by other means; but no appearance
icated the formation of a sulphuret of columbium.

'rom a careful repetition of these experiments upon each
the oxides, I see no reason to doubt of their perfect
eement in all their chemical properties; but there is
ertheless a very remarkable difference in the specific
vities of the two minerals from which they are ex-
ted.

The specific gravity of columbite was ascertained by Mr.
tchett to be 5·918; that of tantalite was found by M.
Ekeberg

Ekeberg to be 7·953; and I have every reason to suppose their results correct, since a small fragment of the former appeared upon trial to be 5·87, while a specimen of tantalite, weighed at the same time, was as much as 7·8. I should, however, observe, that the specific gravities of three other fragments borrowed for this purpose were not so high, that of one being 7·65, of another 7·53, and of a third so low as 7·15.

It is evident that no variation of mere proportion of the ingredients can account for an increase of specific gravity from 5·918 to 7·953, which are in the ratio of 3 to 4; for, since columbite contains four fifths oxide, if the whole remaining one fifth part in weight of that oxide could be supposed added to the same bulk, without diminution of the quantities of iron and manganese, the specific gravity would not then exceed 7·1; and even if a weight equal to one third of the whole were thus added, without increase of bulk, still the aggregate would not quite equal the heaviest tantalite in specific gravity; but, on the contrary, the quantity of white oxide in this specimen certainly does not amount to six sevenths, and probably is not more than five sixths of the whole mass.

The only chemical difference, by which this circumstance could be explained, would be the state of oxidation, which my experiments cannot appreciate; but it may also arise in part from actual cavities in the mass of columbite, and in part from the state or mode of aggregation.

XXXV. *Description of a Method of fitting up in a portable Form* The Electric Column *lately invented by* J. A. De Luc, *Esq. Also an Account of several Experiments made with it. By* B. M. Forster, *Esq.*

To Mr. Tilloch.

Sir, Having been informed that a row of galvanic plates had been constructed without any fluid being interposed, and that it acted very sensibly on a gold-leaf electrometer, I formed one, of about two hundred small circles of zinc, and the same number of blotting-paper and Dutch gold-leaf, the Dutch leaf being cemented on the paper with a solution of gum arabic; the blotting-paper was double, two pieces were gummed or pasted together before the Dutch leaf was put on. Through these circles, or plates, a silken string was passed for connecting them together. This small instrument

t acted sufficiently powerfully on a very delicate gold-
electrometer, to encourage me to make a row consist-
of a greater number of plates. To the two hundred I
d about three hundred more, using, instead of the
ch metal, silver-leaf, and inserted the whole in a glass
fitted up with brass caps, screws, and balls. The
ument thus fitted up may be called an *Electric Rod.*
re some of these rods with the plates not connected by
ing through them; which, provided the glass tube is
nearly of the same diameter as the plates, may be the
way of placing them; but unless the tube fits accurately
ther mode will probably be found preferable, as the
s can be more easily placed regularly.

e Dutch metal, or silver-leaf, may either be fastened
he paper with gum, or paste made over the fire with
and water.

e following experiments were made with a rod of five
dred series of plates,—whether with the one in which
two hundred plates of Duch metal, or in which there
none, but silver-leaf instead, it is not necessary to
tion.

st Sept. 1809. One leaf of an electrometer made of
ch metal kept flapping to and from the side of the
many times, when connected with the electric rod.

——— The ends of the rod being placed upon two
rometers,—when the top of either of them was touched,
electrometer at the opposite end diverged more imme-
ly.

d Sept. The rod was placed at the bottom of an
rometer; one leaf was attracted to the side and flapped
ral times.—This experiment shows that the electric
er of these piles or columns acts through a portion of
I held the upper part of the electrometer in my hand
ng this experiment.

th Sept. A *small* piece of Dutch metal was attracted
the ball at the zinc pole of the rod, and adhered to it.

h Oct. A very light ivory needle, turning on a point
a magnetic needle), was attracted by the rod; when a
r or a key was placed near one end of the needle,
the ball at the end of the rod also near the same end
he opposite side, the needle vibrated backwards and
ards. The needle was insulated, I believe, by a piece of
er.

——— The needle, after having been touched by the
r-end pole, evidently receded from that pole; or, as it
ommonly called, was repelled, having been charged
with

with the same kind of electricity as that end of the rod possessed :—the same effect was perceived when charged by the zinc pole.

———One leaf of an electrometer (Dutch leaf) moved, when one of the balls on the rod was placed over the top, without being in contact with it.

15th Oct. The ivory needle vibrated between the balls of two rods, one of which was at the zinc pole, the other at the opposite pole.

One column which I have made, consists of about 500 plates, each about ¼th of an inch in diameter. I have put at the zinc end a piece of cork cut like the head of a snake or eel, and at the other end another to resemble a tail. This column may be called an artificial electric eel (*Gymnotus electricus*): it is not inserted in a tube like the others, a silken string runs through the centre of the plates, which may be drawn tight; then wound round a pin which is in the mouth, or may be loosened if desirable. This eel acts powerfully on the electrometers. The power appears to me to vary much more than that of the columns in tubes: provided the outside of these tubes be dry, I do not know that the strength of their electric power changes.

18th Oct. Three rods, each of 500 series, were supported upon insulated stands, and a plate of copper suspended at the silver pole of the combined apparatus; another plate was placed under this, (as in the common electrical experiment of the dancing images,) one very small piece (or more) of tissue-paper was attracted up and fell down, and a little image of the same paper reared up, and once remained suspended to the upper plate, but I could not make it dance up and down.

22d Oct. One ball or both ? of Cavallo's pocket electrometer diverged, when three rods were combined ; the pith-balls are on wires. With these three rods I could not perceive the metallic taste in the mouth, which is so perceptible even with a single piece of zinc and silver placed against the tongue. When the ball or balls ? of the electrometer moved, the opposite end of the apparatus was touched. A small pith-ball, suspended I believe with a *single* thread of a silk-worm, vibrated between two fixed pith-balls, one of which was connected with the apparatus, the other communicated with the table.

23d Oct. A coated jar had a slight charge given to it with one of the electric rods. When the zinc pole charged the inside of the jar, that side gave signs of a *minus* state (as it is called), and the outside a *plus* state. This was shown n electrometer, the leaves of which *diverged* when ex- l amber was holden near it, after it had received elec- y from the inside, and *converged* when electrified from utside of the bottle. From the usual effects of Gal- sm the contrary might have been expected; that is, that zinc end of the column would have produced the *plus* , and the silver end the *minus*.

th Oct. With three rods combined, a small brass suspended by silk between two bells, vibrated between , causing them to ring: the bells were suspended from nds of the apparatus. The next day, 25th Oct. (the ee day), having fixed on glass pillars two bells, and ; by silk a brass ball from the upper part of a piece of . I placed the bells in connexion with the ends of the ined apparatus, by means of bent wires laid on them: pparatus and bells were left for near an hour, during h time the bells kept ringing, at times stopping for a interval, then ringing again; the clapper sometimes een to rest against one of them, then appeared to be gaged by a person moving in the room. Whether the gagement was always owing to some slight shaking of ble, or whether it was sometimes in consequence of all having acquired electricity, and then being repelled, not quite clear. It appears not improbable but that eight of the clapper may be so adapted to the power of pparatus, as to cause small bells to continue ringing for al years without intermission; if so, we shall have a ine which by those who do not consider the subject sophically will be called *a perpetual motion*. How the column will continue to produce the electric cannot at present (or perhaps ever) be determined. rincipal difficulty to be overcome, in order to keep the er in continual motion throughout the different seasons e year, appears to be the want of a very accurate *insula-* f the apparatus; for, if the glass tubes or the pillars h support them are damp, the current of the electric will not pass along in the proper direction for the ex- ent.

th Nov. Five rods, each of 500 series, were combined; these, two small bells kept ringing on and off for more four hours, part of which time I was not in the room, nnot tell how often they might have stopped: the ring- ometimes began again evidently not from any shake, imagine from the clapper having become electrified, then being (as it is usually called) repelled from the against which it rested. I placed three rods of 500 series (insulated,) in a box; and brought wires from the ends of this combined apparatus, which I made communicate with two bells. I placed, on Tuesday 27th February 1810, this apparatus in a closet, where I left it until Sunday 11th March, the bells continuing to ring (as far as my observations went) from the time they were put into the closet until that day; when they ceased. What was the cause of their stopping I do not know, but imagine it was owing to dampness. I cannot ascertain that they rang the whole time without stopping, but have no reason to believe otherwise. I intended some months ago to have sent you the description of the above-mentioned apparatus with experiments, but deferred sending it on account of Mr. De Luc's paper not being published, which he sent to the Royal Society in March, last year, and which contains a description of the *electric column* and its properties. He hopes soon to publish it himself. In the mean time he has permitted me to communicate my account to you. I consider the invention of this column as the most important discovery in the science of electricity since that of the *Voltaic pile*, and do not doubt but that when Mr. De Luc gives his paper to the public, it will prove extremely interesting, and I have reason to believe it may lead to further discoveries which will be considered as very important in this branch of science.

Description of the Figures.

Fig. 1. A column of 500 series of plates about $\frac{5}{8}$ths of an inch diameter, in a glass tube with brass caps, &c.
AA Brass caps, the ends of which screw on and off.
BB Screws which pass through the caps into the tube, having a brass plate at the end for compressing the electric column.
CC Balls at the outer ends of the screws.

Fig. 2. One of the screws with a ball at the outer end, and a brass plate or circle at the other. A small hole is pierced in this plate, through which the silken string connecting the electric column may pass.

Fig. 3. A smaller column (the plates of which are about $\frac{1}{4}$th of an inch diameter,) fitted up with a cork head and tail to represent an eel.

Fig. 4. Size of plates in larger column.

Fig. 5. Ditto of the smaller one.

Fig. 6. A combined apparatus, consisting of three rods resting on insulating stands, and having a wire projecting from each of the outer rods: these

wires are for connecting the apparatus with electrometers, &c.

Fig. 7. A couple of small bells supported on glass pillars, with a brass ball suspended from a wire, used as a clapper to ring the bells.

On Wednesday night, 14th March, I put into a closet a couple of bells, communicating with the three rods above mentioned in a box; they then began to ring, and are now ringing:—how long they will continue so I cannot say, perhaps some change in the weather may soon occasion the clapper to cease vibrating.

I remain, &c.
B. M. Forster.

Walthamstow, Essex, 20th March 1810.

XXXVI. *On the comparative Influence of Male and Female Parents on their Offspring.* By Thomas Andrew Knight, *Esq., F.R.S. In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K.B. P.R.S.*[*]

My Dear Sir, I have been engaged, during many years, in experiments on fruit-trees, of which the object has been to discover the best means of forming new varieties, that may be found better calculated for the climate of Britain than those at present cultivated. In this inquiry my efforts have been always most successful, when I propagated from the males of one variety and the females of another; and I was enabled, by the same means, to ascertain more accurately, than had previously been done, the comparative influence of the male and female parent on the character of the offspring. The analogy that subsists between plants and animals, in almost every thing which respects generation, induced me also to attend very minutely to similar experiments in which I engaged on some species of animals; and as the repetition of such experiments would necessarily require a very considerable space of time, and as the results seem to lead to conclusions that may be of public utility, I have thought the following account sufficiently interesting to induce me to address it to you.

Linnæus conceived, that the character of the male parent predominated in the exterior parts both of plants and animals; and the same opinions have been generally entertained by more modern naturalists. But the Swedish philosopher appears to have been misled, by the striking predominance of the character of the male parent in male animals, and to have drawn his conclusions somewhat too generally: for I have observed that seedling plants, when propagated from male and female parents of distinct characters and permanent habits, generally, though with some few exceptions, inherit much more of the character of the female than of the male parent; and the same remark is applicable, in some respects, to the animal world, as I shall point out in the succeeding narrative.

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[*] Philosophical Transactions for 1810, Part II.

My experiments were made on many different species of fruit-trees; but most extensively, and under the most advantageous circumstances, on the apple-tree: and as the results were all in unison with each other, it will be necessary to trouble you only with an account of some of the experiments which were made on that species of fruit-tree.

The apple, or crab of England, and of Siberia, however dissimilar in habit and character, appear to constitute a single species only; in which much variation has been effected by the influence of climate on successive generations: for the two varieties readily bred together, and the offspring, whether raised from the seeds of the Siberian or British variety, were prolific to a most exuberant extent. But there was a very considerable degree of dissimilarity in the appearance of the offspring; and the leaves, and general habits of each, presented an obvious prevalence of the character of the female parent. The buds of those plants, which had sprung from the seeds of the cultivated apple, did not unfold quite so early in the spring; and their fruits generally exceeded, very considerably, in size, those which were produced by the trees which derived their existence from the seeds of the Siberian crab. There was also a prevalence of the character of the female parent in the form of the fruit; but the same degree of prevalence did not extend to the quality and flavour of the fruit; for the richest apple that I have ever seen, and which afforded expressed juice of much higher specific gravity than any other, sprang from a seed of yellow Siberian crab.

The prevalence of the character of the female parent in the preceding cases, may possibly be suspected to have arisen from some error, or neglect of accuracy in making the experiments; but I do not conceive that any such errors could have existed; for the trees of each variety were trained to walls, where they blossomed much before any others of the same species, and the stamina were always carefully extracted, whilst immature, from every blossom which I intended to afford seeds. The remaining blossoms of the trees were also totally destroyed, and no other blossoms, except those from which the pollen was taken, were ever unfolded in the neighbourhood, in the season when the experiments were made; and I have also invariably declined to draw any conclusion from the appearance of a plant, in which I could not certainly distinguish some portion of the features and character of the supposed male parent.

It is perhaps also proper to state, that the predominance of the character of the female parent, could scarcely have arisen from any defective action of the pollen; for, except in cases where superfœtation took place, I have invariably found the effect of a very large, or a very small quantity of pollen, to be invariably the same, in its influence on the offspring; and in the greater part of the experiments, from which I have drawn the preceding conclusions, more than ten times as much pollen was deposited on the stigmata, as could have been deposited in unmutilated blossoms by the ordinary means employed by nature.

In all attempts to discriminate the different influence of the male and female parent on the offspring of animals many difficulties present themselves, owing to the intermixtures which have been made of the different breeds of domesticated animals of every species, and the consequent absence of all hereditary permanency in the character of each variety. For, under these circumstances, the offspring will be very frequently found to show little resemblance either to its male or female parent, either in form, or stature, or colour. It will therefore be necessary, before I enter on the subject of viviparous animals, to observe that when I apply the terms large and small to the male or female parent, I extend the meaning of those terms to the parentage from which the male and female descend, and not to the size of the individual only, which becomes the immediate parent of the offspring.

Mr. Cline has observed, in a communication to the Board of Agriculture, that if the male and female parent differ considerably in size, the dimensions of the fœtus, at the birth, will be regulated much more by the size of the female than of the male parent; and, if the meaning of the terms large and small be extended to the varieties, as well as to the individuals, his remark is perfectly just. But experience compels me wholly to reject the inference that he has drawn respecting the advantages of propagating from large, in preference to small females.

Nature has given to the offspring of many animals (those of the sheep, the cow, and the mare, afford familiar examples) the power, at an early age, to accompany their parents in flight; and the legs of such animals are very nearly of the same length, at the birth, as when they have attained their perfect growth. When the female parent is large, and the fœtus consequently so, the offspring will be large at its birth, in proportion to the bulk it will ultimately attain, and its legs will thence be long comparatively with the depth of the chest and shoulders. When, on the contrary, the female is small, and the fœtus so, at the birth, the length of the legs of the young animal will be short comparatively with the depth of its chest and shoulders; and an animal in the latter form will be greatly preferable, either for the purposes of labour, or of food to mankind. I have seen this difference in the influence of the male and female parent, on the offspring, very strikingly exemplified, in the result of an attempt to obtain very large mules from the male ass and the mare. The largest females, that could be procured, were selected, and the forms of the offspring, at the birth, were perfectly consistent with the theory of Mr. Cline; they were remarkably large: and I observed, that the length of their legs, when they were only a few days old, very nearly equalled that of the legs of their female parents. I examined the same animals when five years old, and in the depth of their chests and shoulders they very little exceeded their male parent; and they were consequently of little or no value; whilst other mules, which were obtained from the same male parent (a Spanish ass), but from mares of small stature, were perfectly well proportioned. I have never seen the little mule, which is propagated from the female ass and the horse, nor even a delineation or description of its form; but I do not entertain any doubt that its chest and shoulders are excessively deep and strong, comparatively with the length of its legs, and that, on account of this peculiarity in its form, it has been so frequently shown on the Continent, under the name of a jumart, as the pretended offspring of the mare and the bull.

In opposing the theory advanced by Mr. Cline, it is not by any means my intention to enter the lists with him, as a physiologist; but, as a farmer and breeder of animals of different species, I have probably had many advantages, hich he has not possessed; and my conclusions have been awn from very extensive, and, I believe, accurate obser- ition.

There is another respect in which the powers of the fe- ale appear to be prevalent in their influence on the off- ring, and that is relative to its sex. In several species of omesticated, or cultivated animal (I believe in all), parti- ılar females are found to produce a very large majority, ıd sometimes all their offspring, of the same sex; and I ave proved repeatedly, that, by dividing a herd of thirty ows into three equal parts, I could calculate, with confi- ence, upon a large majority or males from one part, of ales from another, and upon arly an equal number of ales and females from the r ainder. I frequently en- eavoured to change these ha by changing the male; ut always without success; I have in some instances bserved the offspring of one t, though obtained from ifferent males, to exceed th the other, in the propor- on of five or six, and eve to one. When, on the ontrary, I have attended to t numerous offspring of a ngle bull, or ram, or horse, I ve never seen any consi- erable difference in the numbe f offspring of either sex. am therefore disposed to bel that the sex of the off- ring is given by the female ent; and the probability f this seems obvious in fi d several other species of nimals which breed in w nd though the evidence fforded by the facts add s not by any means of ufficient weight to decide the question, it probably much xceeds all that can be placed in the opposite scale.

In oviparous animals, I have had reason to think the in- uence of the female parent quite as great as amongst the iviparous tribes, though my observations have been more mited, and less conclusive. In viviparous animals, the ze of the fœtus is affected by the influence of the male arent, and, in some instances, not inconsiderably; but e size and form of the eggs of birds do not appear to be any degree changed or modified by the influence of the ale; and therefore the size of the offspring, at the birth, ust be regulated wholly by the female parent; and this rcumstance permanently affects the form and character f the offspring. The eggs of birds, and those of fishes d insects (if such can properly be called eggs), appear to semble the seeds of plants, in having their forms and bulk holly regulated by the female parent; but nevertheless eir formation appears to depend on very different laws. r the eggs, both of birds and of fishes and insects, attain their perfect size in total independence of the male, and the cicatricula, the vitellus, and the chalazæ have appeared (I believe) to the most accurate observers, to be as well organized in the unimpregnated as in the impregnated egg: in the seed, on the contrary, every thing relative to its internal organization appears dependent on the male parent. Spallanzani has, however, stated, that many plants produced well organized seeds, and even seeds which vegetated perfectly, under circumstances in which it is not easy to conceive how the pollen of the male plant or flower could have been present. But the Italian naturalist appears to have blundered most egregiously in his experiment; or (which I conceive to be more probable) he became the dupe of the refined malice of his countrymen; for, I repeated his experiments under very favourable circumstances, and with the closest attention, but I failed to obtain a single seed. The gourd alone produced apparently perfect fruit, and the seed-coats acquired their natural size and form; and in this respect the growth of its seeds appeared to be, like that of eggs, wholly independent of the influence of the male. But the *seed-coats* of the gourd were perfectly *empty*, and I could not discover, at any period of their growth, the slightest vestige either of cotyledons, or plumule, nor of any thing that appeared to correspond with internal organization of a seed of the same plant, under different circumstances. Spallanzani has not, I believe, mentioned the species of gourd upon which he made his experiments: the common, or orange gourd of our gardens, was the subject of mine.

In comparing the mode of the formation and growth of eggs with the observations I had previously made on the growth of seeds, I have been favoured with the very able assistance of Mr. Carlisle, for which I have on this, as on many other occasions, to acknowledge much obligation.

I am, my dear sir,
with great respect, sincerely yours,
Downton, May 20, 1809. THOMAS AND. KNIGHT.

XXXVII. On the Fossil Bones of Horses and Wild Boars.
By G. A. CUVIER[*].

THE above are the only species of animals which remain to be described, in order to complete the history of quadrupeds with hoofs, which have been found in the fossil ate; and our task will be the easier, as they have been dug from loose soils only, for the most part of recent forpation; and such of their remains as have been collected, annot enable us to distinguish them from living species.

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[*] *Ann. du Muséum*, tome xiv. p. 33.

Article I. *Fossil Bones of Horses.*

These are as common in loose strata as the bones of any ther large animal, and yet little mention has been made f them in works on fossil bones; either because their preence was regarded as a very simple occurrence, not derving of attention, or because they were not recognized s being the bones of horses.

There are various proofs of this last oversight, which would ppear very extraordinary if we were not aware how superially fossils and petrifactions have been examined.

Thus we find in the *Traité des Monstres* of Aldrovandus, ublished by Bernier, p. 37, two horse's teeth given as the eeth of giants, while in the *Museum metallicum* of this uthor, published by Ambrosinus, p. 830, teeth of the same nimal are represented correctly.

In another memoir we have said that Lang, in his *Hisoria Lapidum figuratorum Helvetiæ*, tab. XI. *f.* 1, 2, had aken a horse's tooth for the tooth of a hippopotamus.

We may add that Kundmann has engraved others, without nowing what to make of them (*Rar. Nat. et Art.* tab. II. 4 and 5); and that Walch, who had received them from Quedlimbourg, confines himself to remarking their resemlance with those of Lang and Kundmann, without endeaouring to determine them more precisely (*Monumens de Knorr*, II. sect. II. page 152).

The number of authors who have been more adventurous s very small; such as Bourguet, who quotes a single jawooth found at a depth of 60 feet, on digging a well near Modena (*Traité des Pétrifications*); and Romé de l'Isle, who reckons in the number of the subjects in the Cabinet f Davila, a fossil horse's tooth in its alveolus, near Cantadt. (*Cat. de Davila*, III, page 230).

It is certainly to this silence of most naturalists, with espect to the fossil bones of horses, that we are indebted or the silence preserved by M. Faujas on the same subject n his *Géologie*, although he might have taken great advanage of it to support his favourite opinion respecting the dentity of fossil animals with those of the present time.

In fact, the fossil bones of horses cannot be distinguished rom the bones of living horses; and nevertheless we find them most assuredly in the same strata which contain unknown animals.

We have already said that there were thousands of horses' teeth in the celebrated depôt of bones of elephants, rhinoceroses, tigers, and hyænas, discovered in 1700 near Canstadt in Wirtemberg: their association with the elephants seemed to be a general occurrence.

We have seen with our own eyes hundreds of bones and teeth of horses dug up from the canal of the Ourcq, in the spot from which elephants' bones also had been taken; and among the horses' bones there were some completely petrified.

In the quarry of Fouvent le Prieuré, in the department of the Haute-Saone, from which bones of elephants and of hyænas have been procured, several bones and teeth of horses were at the same time found, which have been also sent to our Museum.

M. de Drée is in possession of a piece of a horse's jaw found at Argenteuil, nearly in the same spot with an elephant's jaw.

M. Fabbroni has sent me drawings of several similar portions dug up in the upper Val d'Arno, with bones of elephants, rhinoceroses, and mastodonti with straight teeth.

Finally, M. Fischer has procured me some drawings of horses' teeth brought from the Bergstrasse, and placed in the Cabinet of Darmstadt.

I am convinced from these observations, that if we have not more frequently heard of horses' bones dug up with those of elephants, it has arisen from the former having been regarded as less interesting.

We shall not repeat what we have said of those which we sometimes find in osseous strata: but it is in recent alluviations that most of them are found, as might reasonably be expected.

There is scarcely any valley into which we can dig in any direction, without finding horses' bones in the depositations made by rivers: the valley of the Seine, that of the Somme, and without doubt several others, are full of them.

M. Traullé sent me several specimens from the banks of the Somme; and I have seen them myself dug up from the foundations of the bridge now constructing opposite the Military School.

These last are not interesting, because they have been deposited since our continents assumed their present form: the former, however, being those which accompany the bones of elephants and tigers, are of an anterior order of things.

ings. Did the horses to which they belong resemble ose of the present day in every respect?

I must confess that comparative anatomy cannot answer s question.

I have carefully compared the skeletons of several vaties of the genus *Equus*, those of the mule, the ass, the bra, and the couagga, without being able to find in them haracter sufficiently fixed to entitle me to hazard a deion. If we could obtain an entire fossil head, we might haps set on foot some comparison; but with the other nes, most of which are mutilated, we can obtain no result.

We may therefore rest assured, that one species of the rse genus served as a constant companion to the *elephants* *mammoths*, and to the other animals of the same æra, bones of which fill our large basins; but it is impossible say in what respects they resembled any of the species own at present.

It only now remains to point out the characters by which may distinguish the bones of horses. As it is with the and the buffalo that they are most liable to be connded, it is with these that we must compare them.

The upper grinders of horses are prismatic, like those of ox and buffalo, and marked in the same way with four scents; but they have besides a fifth, in the midst of the er edge. The lower grinders are more compressed, and e four crescents in the horse as well as in the ox; but tead of being parallel in pairs, they are alternate, the t of the inner edge corresponding to the interval of the of the outer edge.

The shoulder-blade of the horse has its spine more eleed at the upper third part of it, and decreases from nce to the acromion. In the ruminating animals there lso an elevation at the same spot; but it is at the lower remity, and at the acromion, that the spine is most proient.

In the humerus of the ox, the great tuberosity rises far ve the rest of the upper head, and there is only a groove the biceps humeri; in the horse this tuberosity does not more than the rest, and there are two different grooves front.

The camel and other ruminants resemble the horse more n the ox in this respect.

The cubitus of the ox, although attached to the radius, y be distinguished throughout its whole length; that of horse is entirely lost from its superior third part, being y marked afterwards by a kind of thread.

The lower head of the radius of the horse is divided into two facets, by an almost perpendicular ridge; that of the ox is divided into three, by two very oblique ridges.

The ox has one bone less in the carpus than the horse, because its os trapezoides is confounded with the great bone. Every person is acquainted with the difference of their metacarpus and their hoofs.

The ischion of the ox has its tuberosity higher than that of the horse, and the os ileum of the latter, on the contrary, is higher at its upper angle: this occasions the striking difference in the crupper of the two animals.

The femur of the horse has three trochanters; that of the ox has only two, and the great trochanter is less elevated.

The lower head of the tibia of the ox is rectangular, and has at its inner edge a facet for the articulation of the fibula; that of the horse is very oblique, and almost triangular.

The same difference of obliquity is discernible in the astragali: that of the horse, besides, has but a very small facet for the os cuboides; that of the ox rests on this bone nearly the half of its inferior head.

The os scaphoides of the horse is much larger than its cuboides, and remains always distinct from it: in the ox these two bones are equally large, and are always compounded. The horse has only one os cuneiforme, and the ox has two.

The differences of the metatarsus and of the hoofs, which have occasioned those of the tarsus, are known to all naturalists.

By means of these short and simple characters we may easily distinguish the bones of the extremities of the two species.

Each of the vertebræ, separately examined, would also furnish characteristics; but the detail would be endless, and it is very rare that we find vertebræ isolated from other bones. I think I have now furnished geologists with all that is requisite.

Article II. *Of the Fossil Bones of Wild Boars.*

I do not find many indications of the teeth of these animals in authors: all those that I have seen, came from peat mosses, or other recent soils; and I do not know if they ever accompanied the bones of elephants.

Walch mentions the vertebræ of a petrified hog, alluded to by Luid, and after him by Argenville; but we cannot trust to such authors or their descriptions. Gmelin, Wallerius,

ius, and others whom I have consulted, do not speak at of this kind of fossil.

There is nevertheless in the *Museum Beslerianum*, plate XXI, a piece of the fossil tooth of a wild boar, under e singular name of *pseudo-corona-anguina*; and Grew ys that the cabinet of the Royal Society of London has milar specimens: but neither of these authors assigns the gin or the species.

M. Delannay, in his *Mémoire sur l'Origine des Fossiles cidentels des Provinces Belgiques*, p. 36, relates, that in the virons of Alost, on digging into a moss, "they found the seous part of a wild boar unknown in Europe, and they nsidered what must have been the extraordinary size the animal when alive." He adds, that what made the imal be recognized, "were the tusks, of a length in every spect astonishing." It would have been very easy to ve added the length of these tusks, and some figure or scription of this head. But geologists have rarely descended to what they considered as minutiæ, and preferred ending their time in contriving systems, to employing in accurate researches: thus the above fact, which might ve been made interesting, is totally useless.

For my part, I have some teeth of wild boars which em to have remained long in the earth. I have some o stained black by the moss in which they certainly had en immersed; but I am not acquainted with the precise igin of any of them, except of a tusk found on digging e foundations of the bridge of Jena, opposite the Military hool, with several bones of horses, pieces of boats, d other artificial fragments. I have also a piece of a w brought from the mosses in the department of the se, deposited in the cabinet of the School of Mines. Both e therefore of very recent strata, and they do not differ in e least from the living analogy.

Adrian Camper sent [illegible] the drawing of the lower half of e humerus of a hog [illegible] wild boar, which had been transmitted to him from H[illegible], but as to its precise position he ows nothing certain.

The head of the Sus [illegible] is so easily distinguished from y others, that we have [illegible] occasion to give its characters. Its grinders represent [illegible] small scale those of the mastodon, with straight [illegible], having also blunt tubercles nished on their edges [illegible] smaller tubercles.

In the wild boar, [illegible] pigs, Siam pigs, and Madagascar wild boars, the [illegible] and complete number of olars is seven.

The posterior lower grinder has five groups of tubercles; that of the upper has six. The eight which precede them have each four groups ranged in pairs. The fourth on each side has three groups ranged in a triangular form; and the three anterior, having their tubercles on a single line, are almost sharp-edged.

The anterior tooth falls very early in our European pigs; and I never found it in the babiroussa, the number of which should be six, but it is frequently five only, in consequence of the casting of the anterior tooth: I found but six also in two peccaris.

The wild boar of Ethiopia has only three teeth, all composed of cylinders tied together like the laminæ of those of the elephant, and presenting circles at their surface when they are filed down. They are very unequal; for the last has no less than twenty-three circles ranged in three lines.

Every species has its peculiar form of tusks; but all the tusks and all the grinders which I have observed, were similar to those of the common wild boar.

The extremities of the *Sus* genus have a great resemblance to those of the ruminating animals. As it is most likely that the bones of the former may be confounded with those of stags and sheep, it is with these last that we ought to compare them.

The shoulder blade, like that of the horse, has the spine flattened in front, and more prominent in the upper third part, where it forms a hook bent backward.

The great tuberosity of its humerus is very high, as in the sheep; but it becomes broader behind, and is followed by a broad reentering arc.

The cubitus is very broad and distinct throughout its whole length; the greatest part of it in the sheep is attached. In the stag it is at least much more slender.

The carpi have a close resemblance, with this difference, that the trapezoidal bone is distinct in the *Sus* genus, whereas it is close in the ruminating animals; and the unciform bone is narrower, whereas the os scaphoides is broader. The differences in the femurs are almost incapable of description in words; but the tibia may be recognized because it is shorter; its inferior head is square, and does not decrease from back to front, and has no articulation for the fibula. The chief difference of the tarsus depends on the small cuneiform bone, on the sole of the fifth toe, and on the os scaphoides remaining distinct from the cuboides. As to the metacarpi, the metatarsi, and the toes, they cannot be confounded.

XXXVIII. Ex-

XXXVIII. *Extract from a Memoir by M.* Mathieu, *on the Discovery of several Blocks of orbicular Granite recently found in Corsica**.

THE insulated block of orbicular granite which was found
n Corsica in 1785, on the small plain of Talavo, half a
ague from the sea, on the shores of the Gulf of Valin-
o, on the Istria road, not far from a place called la Stan-
ona, and which Messrs. Sionville and Barral were the first
o describe, fixed the attention of mineralogists, from the
ngular form assumed in this rock by the white semi-
ansparent feldspar and the *amphibole* or *horne blende* of a
eep black, a little greenish, arranged in several concentric
rcles, which had given rise to species of round or oval
owls, immersed in a confused mixture of the same two
ineral substances which form the basis of the rock.

This stone, so singular by the system of its formation,
nd by the effect which it produced when polished, made
very much sought-after for cabinets, and it soon became
re and of high price.

In vain did Messrs. Barral, Sionville, and after them Do-
mieu, Besson, and several other mineralogists, make in-
uiries after the rock which had given birth to the insu-
ted and partly smooth block, which some convulsion had
ansported and buried in the small plain of Talavo;—all
eir pains were useless.

Notwithstanding their want of success, a Corsican na-
ralist, M. Rampasse, was more fortunate; because his
nowledge of the language and manners of the inhabitants
the mountains admitted of his pursuing, with a hammer
his hand, the chain of mountains from which he pre-
med that the block of the orbicular rock of Talavo must
ve been torn at a very remote period by the action of the
a.

M. Rampasse has executed this arduous journey, during
hich he collected a fine series of rocks and other mine-
ls; but he was not more fortunate than the rest with re-
ect to the globular granite. His inquiry, however, made
acquainted with the position of a porphyritic rock
owded with globular bodies, in general larger than those
the stone found at Talavo; and the formation of which,
thout being absolutely the same with the latter, never-
eless resembled it considerably; but the rock, of a differ-

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* *Annales du Muséum d'Histoire Naturelle*, tome xiv. p. 82.

ent colour, was softer, and did not receive the brilliant polish of the orbicular granite. M. Rampasse brought to Paris some magnificent specimens of this porphyry with large globules. This variety was not to be found in any cabinets.

It results from these details, that all hopes were given up of other masses of granite similar to that of the plain of Talavo, when a particular circumstance brought to view several other blocks, which are said to exist in their native position, a leage from Talavo, and resting on the same rock which gave birth to them.

It is to M. Mathieu, captain in the imperial artillery, and commanding at Ajaccio, that we owe the first information on this discovery, consigned in a manuscript memoir, accompanied with a topographical plan, and a very excellent specimen of this granite, which is of precisely the same form with that which was formerly discovered.

"This superb production," says M. Mathieu, "is found in considerable masses on the estate of Sartene, the property of M Jean Paul Roccaserra: its present situation is about three fourth parts up a very steep mountain, from which it has been insulated by accident; it is in *blocks smoothed* (arrondis) *in consequence of decomposition,* which blocks are comprised within a space which does not extend beyond four hundred square metres. The base is a granite composed of semitransparent quartz, of amphibole with large crystals, and of mica in a small quantity: sometimes shades are discovered which give a feeble appearance of the globulous system. The rest of the mountain is, like those adjacent, composed of a granite of quartz, feldspar, and mica."

M. Mathieu adds, that the lichens and mosses which covered the blocks of this new orbicular granite and concealed its characters, did not permit those who visited the same mountain to see that the discovery was owing *to the recent separation of two parts of one block.* The distance of the position of the ancient block of Talavo from the Rizenare, a river which washes the foot of the mountain on which the recent discovery has been made, is one myriametre and a half. M. Mathieu does not think that this river has ever been capable of transporting this old block of granite to such a distance; and he is perfectly right: but when he presumes that, "*in very remote ages, this same block has been discovered in the Rizenare, and thence transferred by the care of an architect to the spot where it was to be cut with the chisel,*"—this conjecture does not seem to

rest

st on any solid foundation: and it would be entirely refuted the *smooth blocks,* alluded to by M. Mathieu, discovered ithin the space of 400 square metres, instead of having en *worn or smoothed by the effects of decomposition,* had en so worn by the action of some violent convulsion of e sea at very remote periods, when these blocks as well that at Talavo must have been transported to the places here they were discovered. We see many examples, en on very high mountains, of these accidental transports masses much more numerous and much larger, of grates and other rocks not less hard, all the angles of which ve been flattened by frictions. Besides, those in the environs of Sartene are at a trifling height, and of small size, comparison with the enormous masses mentioned by ussure in his Travels in the Alps, and which he considers the result of what he calls *la grande débacle.*

We are certainly far from wishing to lessen in the small degree the merit of M. Mathieu's Memoir, to whom are under great obligations for communicating the disvery; but we insist the more strongly on a new examinan of the smoothed blocks of Sartene, as the precise owledge which we have of that of Talavo, which has ver been *smoothed* by *decomposition,* but by *friction,* leads by analogy to consider the blocks in question as being e result of a similar cause:—for nothing could have less dency to decomposition than the orbicular granite of lavo, the block of which was very hard and very sound ternally, as well as on the external surfaces, which last l not bear any other signs of destruction than that proed by shocks or by friction.

We must therefore invite M. Mathieu to examine again th attention the state of the blocks which he mentions his Memoir, and to ascertain positively, First, If they lly were smoothed by the *effects of decomposition?* Secondly, If these blocks are adherent, or separated from the nite rock on which they rest? which is not clearly enough scribed in his Memoir. And finally, If semitransparent artz exists, as he says it does, in the new orbicular granite? he old species is devoid of it, and is composed only of [illegible] and amphibole of a greenish black, [illegible] which is only met with rarely and in [illegible]

XXXIX. *Pro-*

XXXIX. *Proceedings of Learned Societies*,

ROYAL SOCIETY.

The meetings on the 1st, 8th, and 15th of March were occupied in reading Dr. Herschel's supplementary remarks and illustrations to his former papers on the nature of coloured concentric rings seen between object-glasses. The details were so unconnected in themselves, and referring so intimately to his preceding observations and the objections which have been made to them, that it is not possible to convey any just idea of the author's experiments in an abstract.

March 22, The right honourable President in the chair. A letter to the president from Mr. Knight was read, on the functions of the leaves of trees. In this short letter Mr. Knight merely confirmed the opinions and observations which he made on this subject several years ago, namely: that the leaves of trees perform the same functions as the lungs of animals; that grafts of trees perish as soon as the parent stock, from the inability of the leaves to perform their office, and support the increased circulation of a young and healthy stock; and that consequently a rich soil, and augmented nourishment to the roots of such trees, only tend to accelerate their decay instead of increasing their vigour, as more sap by such means is propelled to the branches than can be digested by the leaves. This theory of the perishable nature of grafts, which has met with much opposition, Mr. Knight now considers as sufficiently established on the sure basis of physical demonstration.

N. B. The doses of magnesia administered by Mr. Brande to his calculous patients * varied from 15 to 20 grains night and morning.

SOCIETY OF ANTIQUARIES.

An account of the earl of Cumberland's expedition to the Azores in 1589 was read to this society; but it contained no new facts of particular importance, except so far as it proved the early superiority of the British navy.

Part of a very accurate description of Rippon minster was also read, in which it was alleged that at least a wing of this church was built prior to the Norman conquest.

ROYAL INSTITUTION.

On Saturday, the 3d of March, Mr. Professor Davy began his second course of electro-chemical lectures, with an inoductory discourse, in which he explained the principle pon which it is proposed to new-model this institution. he plan appears to be so judiciously adapted to the cir-umstances of the establishment, and holds out so fair a romise of a successful prosecution of scientific researches, nat we entertain no doubt of its meeting with the general ppro bation of the proprietary, and receiving that share of ublic encouragement to which it is so eminently entitled. fter a concise historical survey of the origin and progress f the Institution, and of the important discoveries which ad repaid the liberality of its founders, Mr. Davy pro-ounced a short and eloquent dissertation on the utility of hilosophical inquiries in general, and proceeded to explain ne nature of those improvements, which, he conceived, ere calculated to render the Institution a more efficient in-rument of public advantage.

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* See page 155 of our last Number.

The funds of the Institution, he said, had been inade-uate to the expenses of the establishment; and some in-ry had been sustained from the purchase and sale of the roprietors' shares. It was not to be expected, that those ho could make an interest in a scientific institution, an bject of pecuniary profit, could ever feel a zealous soli-tude for the advancement of science. It was therefore roposed that the shares, which originally conveyed an in-rest in perpetuity, should be converted into shares which onferred merely an unalienable interest for life. But lest ny of the present proprietors should conceive themselves njured by the change, a fund was to be provided, by loan, for their indemnification. Thus none would con-nue to be proprietors, but such as were animated by sen-ments favourable to that description of patronage, from ne beneficial influence of which the objects of the Institu-on would be most effectually cherished and promoted.

It might perhaps be a question, whether proprietors hould then be admitted indefinitely, or restricted to a mited number, and appointed by election. Experience ad shown, that what every one could attain, was scarcely hought desirable by any. But where any difficulty pre-nted itself, it produced a desire to surmount it. Hence, it became an object of some exertion to obtain the di-inction of a proprietor, a class of candidates would pro-ably present themselves for election, consisting of indivi-uals to whom the pursuits of science are of real and in-insic importance; and from such patronage the best re-lts might be expected.

In order that the proprietors and the public might derive

every benefit from the Institution, it was proposed, in addition to the advantages presented by the lectures, the use of the library, and the collection of mineralogy, to put them in possession of the result of all scientific investigation that might be pursued, as well as of the proceedings of the Institution, by a quarterly publication of its labours.

To one class of proprietors, namely, the possessors of landed property, the Institution might prove eminently useful. The value of the mineral productions of their estates might be ascertained, without exposing them to the impositions sometimes practised by adventurers, who, for the selfish purpose of promoting their own interests, recommend the working of mines, without any prospect of advantage to any one but the individuals employed upon the undertaking. Several instances, Mr. Davy observed, might be mentioned of benefit which had already accrued to persons, who, before engaging in expensive enterprises of this kind, had transmitted specimens of the productions of their estates to the Institution, in order that they might be analysed; and the report which was returned to them enabled them to avoid the unprofitable hazards to which such speculations are exposed. One gentleman conceived he had discovered a valuable coal mine on his estate. Upon examining the substance, however, it was at once ascertained that it was destitute of all bituminous properties, and the working of a mine of this quality would have been attended only with expense. Another gentleman supposed he had discovered on his estate a stratum of alumina; but, upon its analysis, it was found to be a clay of inferior value. Instances of this description might, if necessary, be easily multiplied. To landed proprietors, therefore, the Institution might prove eminently beneficial.

It had also its claim upon princes and statesmen for their support. Even the materials of war, which in these times may be of the highest importance to the state, might, from new results of scientific investigation, be essentially and effectively improved. But there is another point of view in which it has still a superior claim to their attention and patronage. With the progressive advancement of sciences and arts, the increasing prosperity of the country is closely and inseparably interwoven. Of this no one was more sensible than the great Colbert. He knew how to appreciate their importance, and cherished them with the utmost assiduity and care. Nor was he disappointed in the expectation of their natural effects; for the prosperity of France, in the reign of Louis the Fourteenth, was most rapidly ac-

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Nor are the minute details of science and of art unfavourle to the cultivation of eloquence. One of the greatest atesmen, and at the same time one of the greatest orators is country ever produced, owed, in a great measure, the riety, the charms, and the force of his eloquence, to the timate knowledge which he had acquired of every branch science and of art. These sources of intelligence supied him with that copiousness of illustration, with which s orations were enriched, and enabled Mr. Burke to colct within the boundaries of his own genius, every thing at could adorn and embellish his elocution.

It has been supposed, said Mr. Davy, that this Instituon may ultimately encroach on the province of some of the cient and venerable establishments of the country, where cient erudition has long been cultivated with success. his opinion, however, is founded in error. The precious mains of antiquity, which enlightened the darkness of the iddle ages, and delivered down to us some of the most instimable treasures of human knowledge, we can never alue too highly. Let them continue to be the guides of ur taste, and the beacons by which our course is to be diected. But let them not be exclusively studied. The ursuits on which such minds as those of Boyle, of Bacon, nd of Newton, were employed, are of a much higher rder of utility, and far more conducive to the interests and e happiness of mankind.

I presume too to hope, continued Mr. Davy, that the ncouragement which the Royal Institution has hitherto received from its female visitors, will not be withheld from under its new modification. It may afford them opportnities of acquiring that knowledge, which will contribute render their elegant acquirements still more interesting.

By increasing the sphere of their intelligence, they will secure not only an addition to their own happiness, but the higher gratification of imparting to their children that useful instruction, which cannot fail to strengthen and to endear the relations of domestic intercourse.

Mr. Davy concluded a very eloquent and comprehensive introductory lecture, by observing, that discoveries in physical science are not to be estimated solely by their conduciveness to general utility. They produce on the human mind the happiest and the most sublime impression, in proportion as they develop the harmony and simplicity which reign throughout the works of that Being, whose infinite power is manifested in every thing that is in the heavens and on the earth.

On Saturday, the 17th of March, Mr. Davy, speaking of the discoveries of

MR. CAVENDISH,

paid the following just tribute to the memory of this distinguished philosopher :

"Of all he philosophers of the present age, Mr. Cavendish was the one who combined, in the highest degree, depth and extent of mathematical knowledge, with delicacy and precision in the methods of experimental research.

"It may be said of him, what perhaps can hardly be said of any other person, that whatever he has done has been perfect at the moment of its production : his processes were all of a finished nature: executed by the hand of a master, they required no correction ; and though many of them were performed in the very infancy of chemical philosophy, yet their accuracy and their beauty have remained unimpaired amidst the progress of discovery ; and their merits have been illustrated by discussion, and exalted by time.

"In general, the most common motives which induce men to study, are the love of distinction, of glory, or the desire of power, and we have no right to object to motives of this kind ; but it ought to be mentioned, in estimating the character of Mr. Cavendish, that his grand stimulus to exertion was evidently the love of truth and of knowledge : —unambitious, unassuming, it was often with difficulty that he was persuaded to bring forward his important discoveries. He disliked notoriety ; he was, as it were, fearful of the voice of fame. His labours consequently are recorded with the greatest dignity and simplicity, and in the fewest possible words, without parade or apology ; and it eemed as if in publication he was performing, not what was duty to himself, but, what was a duty to the public.

“ His life was devoted to science, and his social hours ere passed among a few of his friends, principally mem- ers of the Royal Society : he was reserved to strangers, ut where he was familiar, his conversation was lively, and ull of varied information : upon all subjects of science he as luminous and profound, and in discussion wonderfully cute :—

—“ Even to the very last week of his life, when he was early 79, he retained his activity of body and all his energy nd sagacity of intellect. He was warmly interested in all ew subjects of science ; and several times, in the course of he last year, witnessed or assisted in some experiments that ere carried on in this theatre, or in the laboratory below.

“ Since the death of Newton, (if I might be permitted o give an opinion,) England has sustained no scientific loss o great as that of Cavendish ; but this loss is less to be re- retted, since, like his great predecessor, he died full of years nd of glory : his name will be an object of more venera- ion in future ages than in the present moment :—though t was unknown in the busy scenes of life, or in the popu- ar discussions of the day, it will remain illustrious in the nnals of science, which are as unperishable as that nature o which they belong : and it will be an immortal honour o his house, to his age, and to his country.”

WERNERIAN NATURAL HISTORY SOCIETY.

At the meeting on 3d February, the Rev. Dr. Macknight aid before the society a sketch of the mineralogy of the ighlands of Scotland, from the Pass of Leny to Balahelish. The general rock in this tract is mica slate, with its usual ubordinate beds, such as, of granular limestone, horn- lende slate, &c. It contains also, in some districts, beds nd veins of leadglance, and indications of ironglance. Be- ond Zyndrum, the mica slate approaches to gneiss, till e pass Inverouran, where sienite appears. In the neigh- ourhood of King's House, newer granite, feldspar, por- hyry and hornstone are found ; and the adjacent country, s might be expected from the decomposition of these rocks, resents, for many miles, an unusual aspect of bleakness nd sterility. Glencoe, which is singularly interesting, oth in a picturesque and in a mineralogical point of view, onsists of hornstone and compact feldspar, in beds subor- inate to the primitive rocks, and capped with porphyry. At the bottom of Glencoe, mica slate again appears, and is covered with the formation of clay-slate, which affords the well-known quarries of Balahelish. Thus, according to Dr. Macknight, it appears, that the relative positions of the great formations which occur in the highlands of Scotland, correspond to the principles of the geognosy of Werner.

At the same meeting, Professor Jameson read some observations on the universality of rock and metalliferous formations, preliminary to a short account of some specimens of a particular formation of lead-ore found within fifteen miles of Dunkeld in Perthshire. The formation appeared to be almost the same with that which occurs at Strontian in Argyleshire; and it is therefore possible that it may be a source of wealth to the proprietor.

At this meeting also the secretary read some new and interesting observations on the natural history of the common Greenland whale, by Mr. Willian Scoresby, junior, of Whitby; and exhibited a correct drawing of that animal by the same gentleman, differing materially from the figures hitherto published.

HACKNEY LITERARY AND PHILOSOPHICAL SOCIETY.

We have great pleasure in announcing the formation of institutions calculated to diffuse useful knowledge. Of this description is the Hackney Society. It consists of two classes. First, Ordinary members, who contribute to the funds, enjoy the use of the books, &c. Secondly, Honorary, consisting of such gentlemen whose association may reflect honour on the society, and whose opinion of the labours of its members may be such as to impress them with sentiments of regard for such a mark of the society's respect. Ladies are admissible as members.

The officers of this society consist of a president, two vice-presidents, two secretaries, a treasurer, and six committee, who are to be chosen from the ordinary members by ballot or scroll at every anniversary meeting.

The meetings on Tuesday evenings are to be principally occupied by literary conversations, and reading such papers on scientific or literary subjects as the society may be favoured with.

The subjects for conversation, and books for the library, are to comprehend the mathematics, natural philosophy and history, chemistry, polite literature, antiquities, civil history, biography, questions of general law and policy, commerce, and the arts.

The purchase of philosophical instruments, and patronizing lectures on philosophical subjects, also form a part of e plan of this society.

FRENCH NATIONAL INSTITUTE.

The class of history and ancient literature of the French istitute has proposed the following as the subject of a ize dissertation : " What were the people who inhabited isalpine and Transalpine Gaul, at the different epochs of story anterior to A. D. 410?—Determine the position of e capital cities inhabited by these people, and the extent of rritory which they occupied.—Trace the successive changes at took place in consequence of the divisions of the Gauls to provinces."—The prize will be a gold medal of the lue of 1500 francs. The memoirs may be written in tin or in French, and must be transmitted to the Secreriat of the Institute at Paris on or before the 1st of April, 11.

XL. *Intelligence and Miscellaneous Articles.*

Ir. Brown, the botanist who accompanied Capt. Flinrs in the late voyage of discovery, has just published the st volume of a work on the plants of New Holland, &c. der the following title : " *Prodromus Floræ Novæ Holdiæ et Insulæ Van-Diemen* ; exhibens characteres planum quas annis 1802—1805 per oras utriusque insulæ llegit et descripsit *Robertus Brown* ; insertis passim aliis eciebus auctori hucusque cognitis, seu evulgatis, seu intis, præsertim *Banksianis*, in primo itinere navarchi *ok* detectis."

M. Ebel, of Bavaria, has recently published a geological rk on the structure of the Alps, which is reported by the ntinental journalists to contain much novelty, and to coide entirely with the experiments made by Humboldt. cording to Messrs. Ebel and Humboldt, it is not true t granite is the nucleus of the surface of the earth : on contrary, we find as many strata of granite as of any of other integrant substances of mountains. These strata stones in the mountains have been formed by crystallizan in the Sea of Chaos, and are found in a great measure the same line from Savoy to Hungary. According to se ideas, the earth resembles a prism of crystal, the edges which have been worn away by the flux and reflux of waters, without the ruins of these points having entirely d up the hollows made. These ideas are expected to lead to important results; but they will at the same time discourage those who still hope to find the solid nucleus of the earth. The geologists on the Continent now begin to abandon their own system, in order to embrace that of Humboldt and Ebel.

Mr. Parkinson has withdrawn the *Introduction to the Knowledge of Fossils,* announced at the end of his first volume of *Organic Remains of a former World*, considering its publication as entirely superseded by Mr. Martin's excellent systematic *Outlines* of the same subject. The *third volume* of *Organic Remains* is in considerable forwardness.

In the month of October last, the viceroy of Italy and his consort visited the ruins of the ancient Pompeia, accompanied by chevalier Arditi, superintendant of the Royal Neapolitan Museum. A fresh search having been made for antiquities a few days before by order of their majesties, M. Arditi presented on the above occasion several pieces of ancient pitch, a vessel full of wheat, a piece of coral, several beautiful paintings, and a lamp of baked earth in the form of a leaf, and bearing a Latin inscription. This lamp was covered with a very fine varnish or vitrification, which gave it a silvery or pearly appearance. It seems to be a mistake therefore of some authors, when they inform us that this vitrification was not invented until the fifteenth century by Lue de la Rubria, a Florentine sculptor.

Their majesties having expressed a desire to have some of the ruins dug up under their own inspection, the workmen had the good fortune to find several pieces of money of various denominations: a quantity of bronzes, among which was a very fine vase, and an urn for wine: some articles formed of bones; a great quantity of glasses of various dimensions and shapes; and in particular a great number of vases improperly called Etruscan, on which were Latin inscriptions. On the same occasion, their majesties found some works in marble, and in particular some comic masks: a few small but elegant altars, adorned with bas-reliefs and weights marked with cyphers in the upper part.

Hitherto only a single subterranean apartment had been discovered at Pompeia, improperly called a cantino, but which ought rather to have been named crypto-portico: in the recent diggings one was discovered consisting of several stories. It is remarkable for having a pipe or tube of stucco placed in a corner, and intended for the conveyance of smoke.

ıoke. This discovery seems to set at rest the question so ıg agitated by the learned; namely: whether the ancients re acquainted with the use of vents or chimneys for carng off smoke? In the same apartments were also found 'eral pieces of marble and alabaster, valuable on account the bas-reliefs and inscriptions with which they are ɔrned.

Their majesties afterwards proceeded into a *triclinium* or ıing apartment recently discovered. The walls of this ıgnificent saloon are covered with paintings of the most ɥuisite taste, and representing fishes, birds, and game of kinds. Here there are three couches of mason work in fect preservation, being the places in which the ancients ted during their meals. Adjoining the three beds, there ll exists a marble foot, which must have served as a support for the table on which the dishes were placed.

His majesty on quitting the ruins expressed a most arıt desire that the exertions made to expose them to public w should be continued, and has since issued the necesy orders to his ministers of finance.

The following account of the present state of the unisities and other seminaries of education in the new ıgdom of Westphalia, is published in the foreign jours. The universities of Halle, Gottingen, Helmstadt, ırbourg and Rinteln, contain in all 1207 students. There, also, 52 gymnasia or classical schools in the kingdom, which are educated 6,851 children: the inferior schools, which reading, writing, and arithmetic are taught, ount to 3,600, and are frequented by 253,338 children of h sexes. In each of the two great cities of Brunswick l Magdeburg there are 35 public institutions for every nch of education, besides private seminaries. In the ɔlic schools the hours of teaching are so arranged, that children who attend them are generally able to earn their :lihood at the intervals. In the above two cities alone) scholars are instructed in the sciences. In short, on a derate computation there is a teacher for every 50 chiln throughout the kingdom.

The university of Jena is also described as being in a very ırishing condition. The number of students, which in)7 scarcely exceeded 100, is now quadrupled. The Mineɔgical Society established at Jena a few years ago is in at repute. In September last this society held its anniversary, at which the celebrated M. Goethe was present. This ingenious author now devotes the whole of his time to the study of natural philosophy.

A society for the education of the blind has been lately established at Zurich in Switzerland. The number of pupils is at present 50: and what is singular, the chief master, M. Funke, is blind. He is described as an excellent teacher and an ingenious mechanic.

The calamities experienced at different times in Switzerland from the sudden rolling down of huge fragments of rock and other component parts of the mountains in the Grisons, have suggested to the government the propriety of employing M. Escher, a geologist of Zurich, to survey that district. He has accordingly published the result of his inquiries, and it appears that the valley of Nolla behind the village of Thusis, and the valley of Plesner behind the town of Caire in the Grisons, are threatened with the visitation of avalanches, which can only be averted by the prompt adoption of the measures of precaution which he has suggested.

The following short article on volcanos appears in a recent French journal:—"It has been observed that in the year 1806, at the moment of the dreadful eruption of mount Vesuvius, all the other volcanos with which we are acquainted vomited an increased quantity of flames. Ætna spread terror throughout Sicily, and covered Calabria with lava. Hecla in Iceland was as violent as ever, and the Peak of Teneriffe threw out red hot stones. Volcanos which were thought to be extinguished awoke with new fury.

"The communication of volcanos with each other is not doubtful, But are we acquainted with the conductor of the electrical fluid?—Is this communication effected by subterranean passages, or by the medium of the atmosphere? How does it happen that the above unusual phænomena take place at the same moment?"

CAUTION TO APOTHECARIES AND DRUGGISTS.

For the subjoined information, which we consider ourselves imperiously called upon to circulate as widely as possible, we are indebted to a most respectable manufacturing chemist, Luke Howard, esq., of Plaistow.

A very large quantity of *glass of lead* has, by some means, found its way into the London market, as *glass of antimony*. This criminal *imposition* is sure to be detected, in the operation

ion to which the glass of antimony is chiefly applied, the
king of emetic tartar; but it is highly needful, for the
e of the consumers of smaller quantities, as in the *vi-*
m ceratum, and *vinum antimonii*, that the following *di-*
ictive characters of the two be extensively circulated, in
ler that those, who may have bought the article within
lve or eighteen months past, may assure themselves of
being genuine. *The public health, and even the lives of*
ie patients, may be considered as at stake on the occasion.
Glass of *antimony* has a rich brown or reddish colour,
h the usual transparency of coloured glasses. The glass
lead in question is of a deeper and duller colour against
light, is much less transparent, and even, in some
nples, quite opake.

The specific gravity of the *true* never exceeds 4·95, that
the *spurious*, or *lead* glass, is 6·95: or, in round num-
s, their comparative weights are as 5 to 7.

Let twenty grains be rubbed fine in a glass mortar, add-
half an ounce of good muriatic acid. The *true* dissolves,
h an hepatic smell, the solution is turbid, but has no
iment. The *spurious* turns the acid yellow, giving out
oxymuriatic odour, and leaves much sediment.

Let a little of each solution be separately dropt into wa-
. The *true* deposits oxide of antimony, in a copious
ite coagulum; or, if the water has been previously
ged with sulphuret of ammonia, in a fine *orange* preci-
ate—The *spurious* gives no precipitate in water, and in
other liquid, one of a dark brown or olive colour.

A solution of the *spurious* in distilled vinegar has a sweet
e, together with the other properties of acetate of lead.

A very small mixture of the spurious may be detected,
its debasing, more or less, the bright orange colour of
precipitate, thrown down by sulphuret of ammonia
m the solution in any acid.

The samples of the *spurious*, hitherto detected, are of a
ch thicker and clumsier cast than the *genuine*; but the
earance is not to be trusted, and no specimen should be
wed to pass, without a trial either of the specific gravity,
chemical properties.

John Morison, living at 145, Holborn Bars, who had
misfortune to lose both arms by the discharge of a can-
n, has invented a curious set of instruments, so well
pted for almost every purpose of life, that the want of
se limbs to him is wonderfully supplied; and he under-
es to make artificial legs, arms, and instruments, on a
new

new principle. The Society for the Encouragement of Arts, Manufactures, and Commerce, being convinced of their great utility in affording help and comfort to unfortunate persons in his situation, lately voted him their silver medal and 40 guineas for his ingenuity.

LIST OF PATENTS FOR NEW INVENTIONS.

To Joseph Stephenson, of Mortimer Street, Cavendish Square, in the county of Middlesex, plumber, for a machine for filtering and purifying of water.—Feb. 27, 1810.

To John Justice, of Dundee, in North Britain, ironmonger, for his improvements in the construction of stove-grates calculated to prevent or cure smoky chimneys, and possessing other advantages over the stove-grates in common use.—March 6.

To Thomas Scott, of Holborn, in the county of Middlesex, musical instrument maker, for his improved German flute, clarionette and oboe.—March 12.

To Thomas Robinson, of Roberts-Bridge, in the parish of Salehurst, in the county of Sussex, brewer, for his mashing machine.—March 12.

To John Kent, of Southampton, architect, for his new and expeditious method of moving all kinds of goods or materials to high buildings or from deep places.—Mar. 12.

To Thomas Grant, of Bideford, in the county of Devon, esq., for his method of making paint or varnish from a new discovered fossil, which will be of great public utility in painting of ships and in various manufactories.—Mar. 22.

To Michael Shannon, of Berwick Street, in the county of Middlesex, architect, for certain improvements in the art of brewing, which were communicated to him by a learned foreigner since deceased.—March 22.

To Johann George Deyerlein, of Long Acre, in the county of Middlesex, tool-maker, in consequence of certain inventions by himself, and of communications made to him by a native of Germany, for a machine, new principle, or method of making bricks and tiles, and also by means thereof, and of clay, loam, or similar materials to those commonly used in potteries, to make all sorts of mouldings, beads, tubes, gutters, channels or cylinders to convey water, smoke, or any fluid or soft substance—March 22.

To John Gregory, of Islington, in the county of Middlesex, builder, for his new method of tunning or cleansing ales and beers into casks.—March 22.

ain Table, by the Rev. J. Blanchard, of Nottingham.

Chichester.	London.	Chatsworth, Derbyshire.	Horncastle, Lincolnshire.	Ferriby, Kingston-upon-Hull.	Heath, near Wakefield, Yorkshire.	Manchester.	Lancaster.	Dalton, Lancashire.	Kendal.	Middleshaw, near Kendal.	Nottingham.
8·44	2·91	5 22	3·50	1·57	3·98	2·67	4 ·66	6·58	5·27	4·48	1·80
4·31	1·86	3·29	2·59	2·94	2·58	1·96	3·11	4·53	4·28	5·74	1·69
0·00	0·94	0·44	0·82	0·48	0·43	0·35	0·53	1·13	0·72	0·62	0·75
3·95	3·46	1·70	2·10	3·05	2·11	0·96	1·59	2·20	1·69	1·50	2·15
1·07	0·86	1·83	1·59	0·45	2·96	3·42	3·39	3·85	4·55	2·66	1·80
2·38	1·20	2·06	2·24	3·24	2·01	2·45	3·10	4·26	3·24	3·79	2·45
3·45	3·58	2·00	2·87	2·38	2·28	1·79	4·00	3·45	3·28	3·08	1·44
3·70	2·64	4·38	4·53	5 88	4·61	3·85	6·12	7·25	9·73	9.59	4·50
3·34	2·90	4·13	3·90	3·10	4·29	4·22	4·75	5·57	6·36	6·11	3.13
0·60	0·22	0·28	0·75	0·56	1·41	0·61	0·87	1·66	1·16	1·10	0·31
1·30	1·38	1·91	1·70	1·90	2·25	2·14	3·87	2·80	4·11	2·56	1·18
5·53	3·00	2·67	1·79	2·42	2·74	4·68	5·74	7·08	8·02	8·85	1·81
38·07	24·95	29·91	28·38	27·97	31·65	29·10	41·73	50·36	52·41	50·08	23·01

leteorological Table, by Dr. Clarke, of Nottingham.

hermometer.			Barometer.				Weather.		Winds.			
Minimum.	Medium.	Greatest Range in Twenty-four Hours.	Maximum.	Minimum.	Medium.	Greatest Range in Twenty four Hours.	Fair.	Wet.	N. and N.E.	E. and S.E.	S. and S.W.	W. and N.W.
17	35·29	14	30·05	28·65	29·44	1·13	18	13	14	18	9	5
30	43·00	14	30·33	28·68	29·62	0·74	18	10	2	3	33	16
30	44·00	10	30·38	29·00	29·99	0·40	26	5	22	6	9	15
28	42·76	13	30·36	28·97	29·73	0·54	13	17	16	1	14	18
33	57·61	9	30·26	29·23	29·81	0·43	24	7	10	13	19	13
45	57·71	18	30·45	29·27	29·84	0·62	21	9	11	7	15	14
46	59·64	10	30·12	29·39	29·88	0·24	17	14	24	4	10	18
48	60·69	8	29·97	29·23	29·64	0·45	12	19	3	8	31	8
34	50·46	12	29·87	29·05	29·46	0·63	25	5	2	5	11	17
30	52·00	10	30·25	29·77	30·09	0·38	25	6	13	6	17	4
26	42·10	11	30·41	29·03	29·89	0·77	18	12	12	2	6	22
33	40·12	13	30·00	28.25	29·45	0·90	17	14	1	6	23	15
	48·78				29·74		234	131	130	79	197	165

[illegible]

ANNUAL [illegible].

THERMOMETER.

Highest Observation, July 27th - - - - - - - [illegible]
Lowest Observation, January 22d - - - - - [illegible]
Greatest Variation, in twenty-four hours, [illegible]
Annual Mean - - - - - - - - - - - - - - - [illegible]

BAROMETER.

Highest Observation, June 25th - - - - - - - - - - - [illegible]
Lowest Observation, December 17th - - - - - - - - - [illegible]
Greatest Variation in twenty-four hours, January [illegible]
Annual Mean - - - - - - - - - - - - - - - - - - [illegible]

RAIN.	INCHES.
Greatest Quantity in August - - - - -	[illegible]
Smallest ditto in October - - - - - -	·31
Total Quantity for the Year - - - - -	23·01

WEATHER.	DAYS.	WIND.	TIMES.
Fair - -	234	N. & NE. -	190
Wet - -	131	E. & SE. -	79
		S. & SW. -	197
	365	W. & NW	165
			571

REMARKS.—On the 22d of January, the Thermometer, within two miles of Nottingham, stood at 14°. April 19th, Snow had fallen to the depth of one foot. May 2d, Snow fell this morning. August 6th, the Rain that fell from 9 A.M. to 5 P.M. amounted to 1·72. Loud peals of thunder at noon, increased at 4 P.M., when the lightning became extremely vivid—the thunder tremendous—the rain descending in torrents, and continuing so most part of the night. December 17th, the Barometer at 11 P.M. stood at 28·25f. The following are comparative observations on the fall of the mercury:—

BAROMETER.

	Keswick.	*Kendal.*	*London.*	*Nottingham.*
January 1788 - -	28·55	28·38	28·89	
January 1789 - -	28·09	28·12	28·58	
December 1809 - - - - - - - - - - - - - - - - -				28·25

N. B. The Barometer is firmly fixed to a standard wall, over a stair-case, on a level of 130 feet above the sea. The Pluviameter is placed in a garden, on an elevation of 140 feet from the level of the sea.

Quantity of Rain in 1808, *at the following Places in Scotland:*

1808.	*Dalkeith.* Inches.	*Bothwell Castle.* Inches.	*Glasgow.* Inches.	*Gordon Castle.* Inches.	*Largs.* Inches.
January - -	1·29	1·278	1·246	1·27	
February -	1·905	1·461	·778	1·62	
March - -	·452	1·26	·032	·71	
April - -	2·805	1·923	1·535	2·97	
May - - -	2·22	1·867	1·371	·78	
June - - -	2·288	1·579	1·811	1·62	
July - - -	4·12	1·83	3·118	2·91	
August - -	3·451	5·827	5·597	2·92	
September -	2·651	·655	·616	5·31	1·452
October - -	2·393	3·418	2·171	3·21	8·033
November -	1·48	1·993	2·135	1·68	3·664
December -	2·44	1·507	1·342	2·21	5·488
	27·995	24·598	21·795	27·21	18·637

Meteorological

METEOROLOGICAL TABLE,

By Mr. Carey, of the Strand,

For March 1810.

ays of the Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Feb. 26	38	47°	47°	29·89	23	Fair
27	47	53	44	·65	19	Cloudy
28	40	52	48		32	Fair
arch 1	47	53	47		16	Cloudy
2	48	54	49		10	Small rain
3	49	51	41		0	Rain
4	41	50	40		18	Cloudy
5	40	45	39		19	Cloudy
6	35	38	41		0	Snow
7	40	45	42		0	Rain
8	45	49	46		15	Cloudy
9	52	54	49		10	Stormy
10	50	54	50		36	Fair
11	50	54	50		30	Cloudy
12	52	52	46		10	Stormy
3	40	42	40		32	Cloudy
14	39	41	38		25	Cloudy
15	38	38	35		20	Cloudy
16	35	39	34	·55	10	Cloudy
17	35	40	33	·65	30	Fair
18	32	44	36	·90	22	Fair
19	29	44	43	30·00	19	Fair
20	34	50	40	29·95	33	Fair
21	35	49	41	·65	21	Cloudy
22	34	42	34	·98	19	Fair
23	32	49	36	·72	34	Fair
24	33	47	37	·72	27	Fair
25	32	42	37	·81	25	Fair
26	35	42	39	·85	27	Fair

N. B. The Barometer's height is taken at one o'clock.

XLI. *On the Torpidity of Animals.* *By* Benjamin Smith Barton, *of Philadelphia, M.D.*

To Mr. Tilloch.

Sir, I lately purchased, and have just finished the reading of, "An Essay on the Torpidity of Animals, by Henry Reeve, M.D." The work has afforded me much amusement, and some instruction; and may, doubtless, be read with great satisfaction and advantage by the younger class of naturalists. It is, however, I think, less replete with new facts and experiments, and with original and enlarged views of the nature and phænomena of torpid life, than might have been expected, considering the respectable author's opportunities of acquiring information, and the length of time that he has had the subject under his consideration.

Having myself, for several years, been engaged in inquiries relative to the same subject, in various classes of animals, but especially in the Mammalia, the Birds, and the Reptilia (*Amphibia* of Linnæus), I hope to be able, at no very distant period, to publish the full result of my researches and experiments. I shall then, with that candour, which, I trust, will never forsake me in my inquiries as a naturalist, point out some of the errors (as I now conceive them to be) of Dr. Reeve's work; and, in particular, I shall state, at length, the facts, the actual experiments, and the observations, which compel me to differ from him on some very material questions. At present, I have no other object in view, than to draw your attention, and that of your philosophical readers, to that part of Dr. Reeve's *Essay* which relates to the real or supposed torpidity of birds. This part of his subject, the intelligent author does not seem to have examined with his accustomed ability.

In treating of the "Migration of Birds," Dr. Reeve has the following words: "Here a curious question arises respecting the disappearance of birds. It is singular that this subject should still admit of doubt, when it seems so easy to be decided; yet every month we see queries and answers about the migration of swallows, and every year our curiosity is tempted to be amused with marvellous histories of a party of these birds diving under water in some remote quarter of America. No species of birds, except the swallow, the cuckow, and the woodcock, have been supposed to remain torpid during the winter months. And what is the evidence in favour of so strange and montrous a supposition ? Nothing but the most vague testinonies, and histories repugnant to reason and experience*."

It appears somewhat surprising to me, that an author vho has so long had the subject of the torpidity of animals inder his consideration, should have hazarded the asertion contained in the preceding paragraph. Dr. Reeve ias, certainly, read of other birds, besides the swallow, the uckow, and the woodcock, which are said to have been ound in a torpid state. And ought he not to have *menioned* these birds ?

In my *Fragments of the Natural History of Pennsylania*, which Dr. Reeve, if I do not mistake, has seen, for e has referred to the work in his Inaugural Dissertation ublished in 1803, I have mentioned the common humming-bird (*trochilus colubris*) as one of those American irds which do occasionally become torpid. I have partiular reasons for quoting the passage, as it occurs in the *ragments*. " I have not been able to learn, that the umming-bird winters in any, not even in the warmest, arts of the United States. I cannot hesitate to consider t as a bird of passage. A gentleman, however, whose ame I do not recollect, wrote a little paper to prove, that hese birds continue with us all the winter: why? beause one of them was, one frosty day, in the month of October, found a good deal benumbed in a church, in some art of New England, I think in Connecticut†."

In the same work, speaking of the *caprimulgus virginianus*, or whip-poor-Will of the Americans, I have said: " I have been informed, that some of these birds have been found in a torpid state, in hollow trees, in New-Jersey. But I cannot entirely depend upon the fact; and I have little hesitation in saying, that this bird, as well as the swallows, to which it is allied, is a bird of passage‡."

Here, then, there are two American birds, besides those enumerated by Dr. Reeve, which are supposed, by some persons, to become torpid in the winter season. Nor do these complete the list. It is the opinion of many well-informed persons, in the United States (but I by no means vouch for the verity of the story), that the Virginia corn-crake, or rail (*rallus virginianus*), becomes torpid, and remains among the mud and grasses of our meadows, &c., during the winter-season. It is asserted, by many other

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[*] An Essay, &c. section ii. pages 39, 40.

[†] Fragments of the Natural History of Pennsylvania, Part First. Appendix I. pages 18 and 19. Philadelphia, 1799.

[‡] Fragments, &c. Appendix I. page 18.

persons, that whole flocks of the Carolina parrot, or parakeet, (*psittacus carolinensis,*) continue in a torpid state, in the hollows of trees, in the state of North Carolina, and in some other parts of the American Union. *I believe entire dependence may be placed upon this statement*; though it would not be difficult to show, that these birds are often seen abroad, and pretty active, when the ground is whitened by snow. I could mention not a few other birds, the torpid state of which has been spoken of by naturalists and others; and these birds I shall mention in my "Facts, Experiments, and Observations, relative to the Torpidity of Animals."

But "what" (says Dr. Reeve) "is the evidence in favour of so strange and monstrous a supposition? Nothing but the most vague testimonies, and histories repugnant to reason and experience."

This, surely, is not the proper language to be employed in the investigation and discussion of physiological questions. Authorities are facts in natural, as well as in civil, history. And in favour of the torpidity of some of the birds which I have mentioned, the authorities are, sometimes at least, highly respectable: nor are they few in number. In regard to the swallows, I shall say but little at present. I have, at this time, in the press, a memoir on the migration and torpidity of these birds. I am confident that I shall be able to convince every candid philosopher, that great numbers of swallows, of different species, do *occasionally* pass into a state of torpidity, more or less profound, not merely "in some remote quarter of America," but in the vicinity of our capital cities, where there are some men of genuine observation and inquiry, and who are as little propense to believe the marvellous in natural history, as any philosophers elsewhere.

I do not suppose, that *all* the swallows of North America become torpid. It is my present opinion, and it was my opinion when I published the "Fragments" in 1799, that the swallows, *in general*, are migratory birds*. But subsequent and very extensive inquiries have convinced me, that the instances of torpid swallows are much more frequent than I formerly supposed they were; and that there are two species of the genus *Hirundo*, which are peculiarly disposed to pass the brumal season in the cavities of rocks, in the hollows of trees, and in other similar situitions, where they have often been found in a *soporose* state. These species are the *hirundo riparia*, or sand-swallow, commonly called, in the United States, bank-swallow and bank-martin; and the *hirundo pelasgia*, or aculeated swallow, which we call chimney-bird and chimney-swallow. *There is no fact in ornithology better established, than the fact of the occasional torpidity of these two species of Hirundo.*

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* See Fragments, &c. Appendix I. page 16. See, also, Introduction to this work, pages xii and xiii. § xxiv, xxv, xxvi.

I say nothing of the torpidity of swallows "under water." But I do not wholly deny *this* fact. And I take much pleasure in referring Dr. Reeve to a short paper, in the *Transactions of the American Philosophical Society*, vol. vi. part i., relative to the di [illegible] of a torpid swallow under a quantity of mud and [illegible] The author of that paper was a most worthy and [illegible] le man; and a man so religiously attached to trut [illegible] believe him to have been incapable of uttering a [illegible] l. He was, moreover, a man of nice observation, [illegible] ilosophical turn of mind.

I do not wish to urg [illegible] of the swallow's history any further. I have n [illegible] say in support of the "swallow song." But [illegible] page 44, Dr. Reeve asserts, that no swallows " [illegible] r found in all the rivers and lakes of England, [illegible] land, Scotland, or Switzerland, although fisherm [illegible] constantly employed on these their supposed hidi [illegible]," does he mean to say, that it has never been as [illegible] any of his countrymen, that swallows have bee [illegible] torpid, under water, *in England?* Swallows a [illegible] have been found torpid "in the river Thames;" a [illegible] fact seems to have been credited by some illustrious Englishmen in the 17th century; and among others, if I do not mistake, by the immortal William Harvey*.

But I will take my leave of the swallows.—Since I published my *Fragments*, I have obtained much information relative to the torpidity of the humming-bird. I have hinted at this subject, and have, indeed, most pointedly admitted the fact, in my letter to Mons. la Cepede, published in your Philosophical Magazine. I am now fully persuaded, that instances of the torpidity of the *trochilus* are by no means uncommon in the United States: and I regret my having treated with so little respect, the opinion of the Connecticut gentleman already alluded to. It is certain, at least, that the *trochilus*, like the generality of the swallows, is very impatient of cold; and that it sometimes, even in our houses, very suddenly passes into a profound slumber, from which, however, it awakes, to enjoy all the privileges of its life.—I say this is *certain*. And this, so far as his sentiments may be collected from his *Essay*, is more than Dr. Reeve is willing to admit of *any* species in the great class of Birds.

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* In Dr. Birch's *History of the Royal Society*, vol. iv., there are some curious notices about swallows. The following may not be deemed wholly unworthy of Dr. Reeve's attention. "Sir John Hoskyns proposed, that it might be duly examined, what becomes of the swallows, and in what state they are during the winter. In answer to which Mr. Henshaw affirmed, that the chancellor of Denmark told him, as an undoubted truth, that in Iceland, there had been taken out of the ice swallows, which being afterwards brought into a warm stove recovered and flew about the room."—Mr. Henshaw observed, "that he had an account like the former concerning swallows from our watermen, viz. that they have found them in the river Thames;

The fact of the torpidity of the *trochilus* was not unknown above two centuries ago. It is related by the Spanish historians Herrera, Ximenes, and several others, though it must be confessed that these writers have mixed with the truth, some *fable*. I have lately conversed with an intelligent gentleman, who was born, and has long resided, in the kingdom of Mexico. He assures me, that the fact of the torpidity of the *trochilus* is known to every one in that country, and in the adjacent provinces. He added, that he had himself seen one of these little birds in its brumal sleep, in a tree.—I shall discuss this subject at length, and shall illustrate it by actual experiments, in my work on the torpid state of animals, to which I have already alluded. In the mean while, I flatter myself that the following lines, a part of which immediately relates to the *somnus* of the *trochilus*, will not be wholly unacceptable to some of your readers. The author is Raphael Landivar, a native of Guatimala; and his poem, entitled *Rusticatio Mexicana*, in fifteen books, besides an Appendix, in verse also, deserves to be much better known than it appears to be. It is, indeed, well worthy of an English translation; and I sincerely wish that the elegant Mr Sotheby, whose translation of the *Georgics* of Virgil has so deservedly procured him a high reputation, could be induced to

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Thames; and that towards the end of the year they assemble in great numbers on the little islands of the river, and then submerge themselves in the water."—"Upon reading the minutes of the last meeting, Mr. Henshaw remarked, that Dr. Harvey had considered the state of swallows in the winter, and had dissected some of them, which had been found under water, and could not observe that there was either warmth or motion in them."—"Mr. Chetwynd, of Ingstree, being present (at a meeting of the Royal Society), observed, that during the time that the swallows are laid up for the winter, they moult, and return in the spring with all new feathers." The History of the Royal Society of London, &c. &c. By Thomas Birch, D.D. Secretary to the Royal Society, vol. iv. pages 533, 534. 537.

ndertake the task. My copy of Landivar's work, which , I believe, a very rare one, would be at his service. The ublic pulse might be tried, by the publication of a version f one or two of the books.

In his 13th book the author treats of Birds. And here is that he speaks of the humming-bird, its manners, its eep, &c.

"Nil tamen exiguo novit præstantius orbis
Colibrio dulcis spoliato murmure vocis*,
Sed claro tenues pennâ radiante per artus.
Exiguum corpus, forsan non pollice majus,
(Quod rostro natura parens munivit acuto
Atque artus ferme totos æquante volucris.)
Induit aurato viridantes lumine plumas,
Et varios miscet tractos a sole colores.
Ille volat rapidum Zephyrum superante volatu,
Et raucum pennâ tollit stridente susurrum.
Roscida si vero fragranti educère flore
Mella velit rostro, viresque reducere membris,
(Quippe aliâ quacumque negat se pascere mensâ)
Sistitur in medio concussis aëre pennis,
Nectareum donec tereti trahat ore liquorem.
Ast adeo prompte subtiles concutit alas,
Ut vigiles fugiant oculos, ludantque citatæ;
Suspensamque putes volucrem super æthera filo.
Sin autem sylvis borealis bruma propinquet,
Plusque vagus solito frigescat Jupiter imbre,
Frigida præcipiti linquit *Colibrius* arva
Nostra fugâ, linquitque levi viridaria pennâ,
Et longùm montis nigris absconditus umbris
Indulget placido, ceu Progne arguta, sopori,
Dum luces Aries stellatis noctibus æquet,
Verque novum pratis antiquum reddat hororem."
Rusticatio Mexicana, lib. xiii. v. 217—242.

All this, Dr. Reeve will perhaps say, may do very well poetry: but something more positive on the subject of e "placidus sopor" of the colibri is required. Some cts, and therefore something more positive, I have already entioned: and many additional facts, with experiments, promise to give in another place. At present I will ly add, that Mr. Landivar mentions the torpidity of the umming-bird, not as a fable, but as an established truth. or in the short *Monitum* prefixed to his interesting work,

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* "Avicula hæc *Colibri* in America Meridionali, in Septentrionali vero *hupa-mirto* dicitur." Note by Landivar.

he says, " In hoc autem opusculo nullus erit fictioni locus, eam si excipias, quæ ad lacum Mexicanum canentes poetas inducit. Quæ vidi refero, quæque mihi testes oculati, cæteroquin veracissimi, retulere. Præterea curæ mihi fuit oculatorum testium auctoritate subscripta, quæ rariora sunt, confirmare." I am, sir, with much respect,

Your obedient servant, &c.,

Philadelphia, Nov. 1, 1809. **Benjamin Smith Barton.**

XLII. *Description of a Hydro-pneumatic Table for collecting and transferring Gases, and for experimenting on permanently elastic Fluids.* By Fredrick Accum, *Operative Chemist, Lecturer on Chemistry and on Mineralogy and Pharmacy, M.R.I.A., F.L.S., &c.*[*]

There is but one sure road to arrive at truth in all departments of experimental knowledge, namely, to consult nature herself by operative experiment; and this becomes the more necessary as the subjects to be studied are more complicated and extended.

In mathematics, the propositions necessary to be known for the acquisition of knowledge, are self-evident, and admitted as soon as announced, and from their ready admission the student is led to the explanation of the most complicated truth. In the system of chemical science it is otherwise. This branch of inquiry, it may be said, opens with a detail of various processes which are by no means self-evident, nor easily to be repeated by those who have not yet a claim to the title of a chemist.

To awaken the ardour of chemical research, as well as to give facility to the practical acquisition of knowledge at the least possible cost, and in the most advantageous manner, must therefore be an object of some importance to the advancement of science; for to the abridgement of labour, and the superior aid of modern instruments of research, does the science of chemistry owe its rapid strides towards perfection.

It is well known that before the invention of the thermometer, men were accustomed to judge of the different degrees of heat and cold, by the sensations produced on their organs of feeling, and the estimates must have been often highly exaggerated, and always vague and fallacious. It must be acknowledged that a number of valuable facts

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[*] Communicated by Mr. Accum.

would

ould still be wanting, chiefly with regard to our know-
edge of the constitution of mineral bodies, if Bergman
ad not pointed out the effects of the blowpipe, an instru-
nent of the greatest value to the mineralogical chemist.

What a deal of trouble and expense, as well as time,
that most inestimable of all the *desiderata* of experimental
esearch,) is saved by the form which Nicholson has given
o the gravimeter, an instrument which renders the modes
f ascertaining the specific gravities of solids and fluids,
t once easy, expeditious, and accurate, to the fifth figure
f the decimal, water being taken as unity *.

What a vast field of [illegible] has been opened to the
hemical philosopher by [illegible] simple modification of
he air-thermometer of Lesl[illegible] instrument of uncom-
non delicacy, employed by [illegible] ith the greatest advantage
n his important researches [illegible] ning the mensuration of
he force and density of ligh[illegible] ith what ease, expedition
nd œconomy are at presen[illegible] rmed (even in the closet)
hose operations which in [illegible] imes demanded a regular
boratory! The moveable [illegible] of Dr. Black †, the blast
rnace of Aikin, and the l[illegible] nace of Guiton, are now
onsidered as sufficient for [illegible] g on, in the small way,
ll those operations of the [illegible] e, in which either a very
ntense or a very low heat is [illegible] ssary. These apparatus
lone, it may be said, have [illegible] ed from the laboratory a
umber of unwieldy and co[illegible] ntrivances, which served
nly to excite embarrassme[illegible] confusion. Many tedious
rocesses in the practice [illegible] science have thus been
nade easy by the help of modern instruments, hazardous
nes are become safe, expensive ones cheap, and the means
f experimenting have been brought to every door. The
ultivators of chemical science who have no access to the
boratory of the operative chemist, are too well acquainted

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* It does not appear that any better instrument for finding specific gra-
ties for the use of the chemist need be wished for. The gravimeter of
icholson is susceptible of correction for the variation of temperature, and
e impurity of the water in which it is to be immersed, which for practice
sometimes more convenient. It requires no address in using it, and the
ice at which the instrument may be purchased is far below that of the
ost ordinary kind of hydrostatic balance.

† This furnace has been considerably improved by others, since its
rst introduction into practice. It is very substantial, durable, not liable to
e easily injured by external blows, and capable of producing a degree of
eat more than sufficient to melt iron. It is perfectly safe in a room; and
e thickness of the walls, composed of fire bricks, with which it is lined,
events the operator from being molested by the heat when the furnace is
action. See "Manual of nalytical Mineralogy, intended to facilitate
e practical Analysis of Minerals," by F. Accum, 2d edit. page 38.

with the utility of the instruments before named, to render any further observations concerning them necessary.

It is in consequence of such reflections, and the invitations I have received from others, whose judgement I respect, that I take the freedom to lay before the public the annexed sketch of a pneumatic table, which in the routine of my profession I have found extremely useful in operating on gases, which I flatter myself will be found an acquisition among the apparatus to the laboratoty.

The discovery of the gases, and their great importance in the researches of modern chemistry, have occasioned, as is well known, the necessity of some peculiar instruments, by means of which these bodies may be caught, collected, transferred, and submitted to the action of other bodies. Among these the very simple and ingenious reservoir, invented by Dr. Priestley, and named by him the pneumatic trough, is the most indispensable. Several alterations have been proposed in the structure of this vessel, to render it more convenient for use; but these, it may be said, relate either to its form only, or to its neatness and general appearance,and not to its principles, or application and utility, as connected with the operations of pneumatic chemistry.

Fig. 1. (Plate VII.) represents a wooden table three feet six inches high, two feet ten inches long, and one foot eight inches broad. At each end of this table, and at a depth of ten inches from its upper edge, is a moveable board or platform *d d* measuring 18 inches by 16½. These platforms are supported horizontally by swing-brackets *e e*, which may be turned aside to allow the platforms *d d* to fall down between the inner sides of the legs of the table. The brackets *e e* are then concealed from view. The platforms *d d* serve to support table furnaces, retorts, and stands, or other apparatus employed in the production of gases. Fig. 2. is an œconomical lamp furnace with its retort in action, to show the use of the platforms. The upper part of the pneumatic table is surrounded by a broad rim or border *x x* two inches and a half deep, so as to form a shallow tray. It is of rather larger dimensions than the table, projecting over the frame of it about three fourths of an inch. This tray (as I shall call it) is divided into two unequal compartments, namely, a cistern *a a* occupying one end or side, and a stage or shallow plain *b* occupying the other. The cistern *a a* is 16 inches deep, 19 wide, and 16 broad; the stage or plain *b* occupies the remaining part of the tray *b*. When the table is intended to be e used, the tray *x x x*, together with its cistern *a a*, and age or plain *b*, is to be filled with water, so as to stand least three fourths of an inch deep over the shallow ain or stage *b* in every direction. Across the cistern *a* is a shelf, marked *c* in the drawing; it is five inches oad, and perfectly level with the stage *b*. This shelf ides backwards and forwards horizontally within the stern *a*, so that it may be placed at any convenient di- ance from the lamp furnace, fig. 2, or other vessel om which gas proceeds. It is provided on its foremost lge with a row of holes, into which from underneath, oad short-necked funnels are fixed; it serves to sup- ort the receivers standing on the shelf *c*, with their pen ends turned downwards upon the before-mentioned bles, through which afterwards the gas conveyed by e funnels is made to pass into the jars destined to receive . As this shelf is level with the stage, and may, by s sliding motion, be brought close to the stage *b* where terminates in the cistern, the jars placed upon this shelf, hen filled with gas, may be slided along without further ouble to a distant part on the adjoining plain *b*, whilst her vessels previously filled with water in the cistern *a a*, id standing also on the plain *b*, may in return instantly e moved on the shelf *c*; and thus the trouble of removing e jars into and out of the cistern is totally avoided.

From this statement it will be seen that the advantages the pneumatic table are considerable. It enables the perator to fill jars or vessels with the utmost neatness, onvenience and expedition. The necessity of trans- rring them when filled with gas, out of the trough, avoided, and the danger of suffering part of the gas escape, as well as the almost unavoidable spilling of ater whilst the vessels are removed, is completely guarded gainst. It enables the operator also to fill jars of almost ny size, which cannot be done in the common trough, ithout rendering it unwieldy and very cumbersome, or ithout being at least obliged to immerse the jars pre- iously into a deeper trough, and thence conveying them pon a plate or saucer into the reservoir in which they e to be filled with gas. Thus the constant danger of aving either too much water, so as to overflow the trough, r too little, so as to admit common air to enter into e jars when the water sinks below the shelf, is effec- ially remedied. In the common trough the vessels are also ceedingly liable to be overset by the pressure of the water, the height of which rapidly increases when large jars are filled, and require continual removing.

When large quantities of gases are required, as in public lectures, this pneumatic table has been found exceedingly useful. The operator not being restrained for room in the management of his experiments, nor being obliged to transfer the vessels continually in and out of the apparatus, and being enabled to perform his operations within a confined space and without encroaching upon any surrounding furniture, are likewise no small advantages, which entitle it to the attention of those who have no access to the laboratory of the operative chemist. The shelf *f f* is very serviceable as a receptacle for holding whatever utensils may be deemed necessary to be near at hand during the experiment.

Compton Street, Soho, March 12, 1810. FREDRICK ACCUM.

XLIII. *On Salmon-Fry: in Answer to a Correspondent whose Communication appeared in our last Number. By* JOHN CARR, *Esq., of Manchester*[*].

To Mr. Tilloch.

SIR, A LIBERAL discussion of subjects in British natural history certainly affords a very amusing variety amongst the more abstruse branches of scientific research, which obtain so deserved a preference in your select journal; and it is with this view that I submit my present observations, rather than from a wish to attain any triumph over the brief and illusory remarks on Salmon-leaps, in your Magazine for last month. What I formerly stated, and shall now offer, is the result of my own personal observations in the Tweed, Coquet, Tyne, Eden, Esk, and several other northern rivers which swarm with salmon, and I mention this only as increasing the responsibility for what I shall advance.

Were it really a fact, as contended for by the writer I am opposing, that salmon-fry actually continue upwards of twelve months in the same river where they are spawned; then during the summer months, (when the streams are half dried up, and the water transparent,) however diminutive might be their size, such are their incredible multitudes that they could no more remain concealed than the pebbles over which the streams flow; and yet during this season, so favourable to exposure and discovery, I believe not one solitary individual of the preceding spawning season can be met with.

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[*] Mr. Carr is respectfully informed that, if we have not misunderstood his allusion, he is mistaken respecting the author of the paper to which he has sent this as an answer. We have therefore suppressed his introductory paragraph.—EDIT.

In the Tweed, more than two hundred thousand salmon are annually caught; and these, astonishing as the number may seem, probably bear but a moderate proportion to the whole quantity that enters the river. Every female deposits many thousands of ova; hence it is obvious what myriads of progeny must be the result; and accordingly n the months of March and April the amount of fry in he river fully corresponds with these data; but after the vernal floods, no vestige remains of the true salmon-fry, he whole of the immense body being swept down into the ocean.

The writer in your last Number seems to be unacquainted vith the spawning months, which are not the two which he conceives, but the last four months of the year. The fish y no means all spawn at or near the same time, there eing considerable diversity in that respect, some comnencing and even finishing two or three months before thers. This diversity probably has two very different circumstances for its cause,—the different ages of the fish, nd the different periods of their entrance into the fresh vaters. The weather, and state of the rivers as to floods, ave also an evident influence. It is in the months of June and July that the old fish, recovered from the great xpenditure of health and substance in the preceding pawning season, first begin to ascend the rivers. What re called fresh fish, indeed, come into the rivers even in ecember, and in all the following months, but these are ish which have not spawned in the last season. In Sepember the females are all exceedingly distended with ova, nd a few of them even begin spawning in that month, nore in that following, but November and December are he principal spawning months; and that the ova then deosited are the germs of the same fry which appear so bundantly in the spring, is a truth as well known, and scertained, as that the fish themselves have heads and tails.

The very rapid growth of the fry I specially noticed, as n extraordinary circumstance; but we shall advance a very hort way indeed into natural research, if the character of xtraordinary is to beat us back. The average weight of the fry in spring will not amount to one ounce; and the writer who has made it necessary for me to offer these observations, admits that the spring fry in the following July and August attain a weight of from 14 to 20 ounces. This latter fact I know to be much greater than stated; and surely the writer who can cavil at the acquisition of the first ounce, and immediately admit its multiple by twenty in little more than the same period, must have very incongruous notions on the subject. His paper was professedly written to exhibit what he calls a more rational and credible account of the matter than I had given; and its own contradiction and improbability are strikingly manifest, as will be seen by contrasting the following two passages: "The fact is, the young fry do not descend the rivers with the old salmon in the spring after they are spawned, for in the October following they are no bigger than a minnow."—"In the months of June and July they (the fry) are about five or six inches in length: *this I know to be fact.*"

These paragraphs contain the horns of a piercing dilemma, and I offer the writer his choice of being gored with that which he may prefer. Either the same fry which were five and six inches long in June and July have, by some strange means, been wasted down to the size of a minnow in the following October, or the fry then no bigger than a minnow continue in the river till the following June and July: and in this latter case, whence have those shoals of fry originated which the same writer sends down to the sea in spring?

The fish which he speaks of in the months of June and July, is an abundant and well-known visitor in our rivers at that season; but it is a perfectly distinct species of tself, and by no means the salmon-fry, with which, however, it has long been, and with the ill-informed it still is, confounded. *It is the samlet.* Few animated productions of nature have had a more contested origin, or been subjected to more wild and extravagant assumptions. Most generally it was considered as a backward and spurious brood of the salmon, that always remained dwarf; never bred; and even all the individuals were absurdly deemed males. Its natural history is now, however, well ascertained, and, excepting in size, it certainly does approximate, in all respects, very nearly to the salmon. Like the latter, it ascends our rivers, from the ocean, in summer, continues in great abundance through the autumn, afterwards spawns, and in the first two months of the year again descends to the sea.

ea. It probably never exceeds nine, but averages only six nches in length. Though every where the same precise pecies, its dubious history has assigned it a number of rovincial names. In Scotland they are called *pars*, in Cumberland *bremlins*, in Northumberland *wrack-riders* (from their lying on the wrack or river weed), in the Severn *samsons*, and in the Wye *skirlings* or *lasprings*.

Mr. Pennant, whose industrious research after facts has o seldom been equalled, in combating the notion of their eing salmon-fry makes the following observations: "The amlet is the least of the trout kind. It is by several imagined to be the fry of the salmon: but our reasons for dissenting from that opinion are these. First, it is well known hat the salmon-fry never continue in fresh water the whole year: but, as numerous as they appear on their first escape rom the spawn, all vanish on the first vernal flood that happens, which sweeps them into the sea, and scarce leaves one behind. Secondly, the growth of the salmon-fry is o quick and so considerable as suddenly to exceed the bulk of the largest samlet: for example, the fry that have quitted he fresh water, in the spring, not larger than gudgeons, return into it again a foot or more in length. Thirdly, the almon attain a considerable bulk before they begin to breed: the samlets, on the contrary, are found, male and female (distinguished by the milt and roe), of their common size. Fourthly, they are found in the fresh waters in ll times of the year."

These observations of Mr. Pennant I can fully confirm from my own experience, excepting the last, which I must directly contradict; for I believe it would be absolutely impossible to find a single samlet in any of our rivers in the months of March and April, the very period when the waters are teeming with the proper salmon-fry. This little nteresting fish, the samlet, in natural habitudes, figure, and even oily fatness, is truly a salmon in miniature; but, nevertheless, it does contain several discriminative marks,—in so much that were a samlet and salmon-fry, of equal size, placed together, the most cursory eye would readily recognise a difference. The samlet is thicker and fuller made han the fry, so much so as to be one third heavier in equal engths: but what would instantly distinguish the one from he other, are several broad light blue bars crossing the sides from the gills to the tail. These the samlet never is without, nor does the fry ever possess them, though similar blue marks may often be seen on small trout. The back and fins of the fry are also more dusky, and the few red spots along the lateral lines far more dull and obscure than in the samlet. The latter are of all fish one of the easiest prey to the angle. They are incessantly on the feed; and an expert angler with artificial flies, in some of our rivers, will take eight or ten dozen in a few hours.

Besides the salmon and samlet, the bulltrout, seatrout, and whitling, all very distinct species of the same genus, also quit the ocean and ascend our rivers, to spawn, in great numbers, and their fry are frequently mistaken, by incautious observers, for the true salmon-fry; from which, however, they exhibit several specific differences.

I shall now drop the subject, having, I presume, sufficiently shown that my former account of the salmon was strictly correct. Even the writer who has drawn from me the present remarks, will, I trust, by this time have discovered how much more easy it is to derange and embarrass than to elucidate an obscure case.

It is worth observing, that the papers on the breeding of fish, published in the Philosophical Magazine for October and November, very satisfactorily illustrate an obscure fact in the natural history of the trout. It is well and very generally known, that trout, when confined in ponds and lakes, attain a size very far beyond what they ever arrive at in rivers and brooks, and that in these confined situations they never breed. This is well accounted for by the proof that they can only spawn in a swift-running current on gravel, and that there only the spawn will attain animation: and doubtless the fish not being exhausted with breeding, in situations where no sufficient currents exist, is a principal cause of their extraordinary growth. These considerations are very encouraging for stocking large pools with trout-fry procured in the easy way pointed out in the papers I have alluded to, and by which trout of a very superior size may be obtained.

A most singular anomaly in the history of British fishes has lately occurred, by a very splendid and meritorious work, under that title, having issued from the press, wherein two species of our most abundant and interesting fish, the salmon and common eel, are purposely disregarded, under the inadequate excuse of making room for a few scarce, obscure, and immaterial species. My own cursory remarks on the two proscribed species will sufficiently establish, that, to an industrious and intelligent inquirer, they yet offer much of new and interesting matter. The most, I was about to say the only, valuable portions of natural history consist in a knowledge of the singular and various habitudes

les and peculiar modes of existence in each distinct
ss,—a sort of animal biography, wherein the species is
'sonified in the diversified details of the individual: and
ılying this to the case under consideration, surely the
imals which almost daily appear upon our tables have a
ramount claim to our consideration, over those which can
ver extend beyond administering to, certainly a laudable
t yet, a mere curiosity. I am, sir,

Your most obedient humble servant,
acess Street, Manchester, **JOHN CARR.**
April 6, 1810.

.IV. *On the supposed Fresh- r Origin of the Gypsum Strata in the Environs of P on the Geological Characters and Relations of the n Shales on the Northern Coasts of Yorkshire: and on the Orbicular Blocks of Sienite on Mount-Sorrel in Leicestershire.* By Mr. JOHN FAREY.

To Mr. Til h.

ı, IT has given me great pleasu e to find, that the doubts
ich I ventured to express i 's 134 and 139 of your
ilosophical and Geological zine, as to the accuracy
Messrs. Cuvier and Brogniar opinions, on the *fresh-*
ter origin of the lower part of the series of strata de-
ibed in the IVth article of t memoir on the basin of
'is*, are in the way of being ed up, by the researches
Messrs. Desmarets, the elde d younger, and M. Pre-
t, who have found numerous *sea-shells* in the third or
'est gypsum mass, notwithstanding M. Cuvier and his
ociate had said (page 50 of your translation) "we are not
uainted with any fossil in this mass, which is the third
he quarries:" what therefore must become of the opinion
M. Lamanon, adopted by them and others, as to the
osums of Montmartre and the other hillocks of the basin
Paris, having been crystallized in fresh-water lakes? or,
ught of the unphilosophical expression used on the oc-
ion, that *one fresh-water shell* therein, was "sufficient
lemonstrate the truth of the opinion of M. Lamanon?"
a calcareous marl, below what is called the little plaster
l in this mass, the fossils appear numerous, and are,
pullariæ, calyptræ, cerites, cithereæ, cockles, corbulæ,
b's shells and claws, echini spatangus†, glosopetræ,

Page 49 of the present volume.

At Grignon lime quarries (p. 118) our authors observed echini cypeastræ.

murices, solens, tellinæ, turritellæ, vertebræ of fish, volutes, &c. Below the above, three small beds of plaster and some strata of marle occur, without shells; and lower, a bed of calcareous marle, in the midst of which is a plaster or gypsum bed, which contains cerites of the genera petricolum and terebrale; many of which *sea shells* are perfectly similar to those in coarse limestone at Grignon, and other places, not only in marles here, lying between beds of gypsum, but in the gypsum itself, although several naturalists have questioned the reality of such an occurrence. The above particulars I have learnt, from a notice in Mr. Nicholson's Journal of the present month, which contains other interesting particulars. Should M. Desmarets and Prevost have presented any memoir on the above subject, you will not, I hope, fail to translate it at length, on its reaching this country.

In the respectable journal above mentioned, Mr. Richard Winter has given an interesting account of the manufacture of *alum* in the neighbourhood of Whitby, and of the strata whence the raw material or alum-shale for making this useful article is procured; of which geological particulars I beg to present a brief sketch, for the purpose of soliciting further information, through the medium of your Magazine; such that may enable us to fix the place of these strata in the British series.

Alum-shale, a blueish gray argillaceous schistus, occupies the coast of Yorkshire for about ten miles southward, and 18 miles northward of the town of Whitby, and extends inland a great distance; cliffs of this shale appear on the shore which are from 100 to 750 feet high, and on which the sea is continuing to make its inroads. A ferruginous sand-stone (perhaps the same with that of Woburn), iron-stone, and shale, or clay of its decomposition, covers the alum-shale stratum, having an easy dip to the southwest. The sand-stone appears from 4 to 50 or more yards thick, producing ochrey springs at its bottom; it hardens by exposure in buildings: a thin bad seam of coal or jet, (probably bituminized wood,) seldom exceeding two inches in thickness, is sometimes found under or in the lower part of the sand-stone: the slines or length-way joints of the strata of stone and shale range N. and S., and the end or cross joints E. and W.

The alum shale contains several species of *ammonitæ*, abundance of *belemnitæ*, *bones* of animals, *nautili* of two or three species, *shells* of various kinds, *trochitæ*, and *vertebræ* of fishes or animals. It likewise holds nodules of indurated

ndurated clay, which contain *native alum*; geodes of iron-tone that contain *naphtha*; pieces of petrified, charred and ituminated *wood*, or *jet*: red *iron-stone*, in strata of a few nches to two feet thick, four or five in number, at about 00 feet from the top of the shale: *calcareous spar*, about alf an inch thick, in the vertical slines and end-joints of the hale above mentioned: *whin-dykes* are said also to traverse hese strata, and to have charred the coals in their vicinity? he specific gravity of the alum-shale is about 2·48; it ecomposes into clay on long exposure: its upper part is ichest in sulphur, and produces about one ton of alum om 130 tons of shale, or 00·77 per cent.: the lower part of e shale is very bituminous, hard and slaty; a spontaneous nd slow combustion ensues when the alum-shale is mixed nd sprinkled with sea-water.

Such are the interesting [illegible]logical facts, which Mr. Winter has laid before the pu[illegible] respecting the aluminous hale strata of Whitby; con[illegible]ing which, it is further esirable to know, the names o[illegible] the different species of imedded shells and other organic remains, with reference to ublished descriptions and plates of them, or drawings and escriptions of such as have not yet been described by naralists; and an account of the strata that underlay the lum-shale, as well as of those which overlay the ferruinous sand-stone. To ascertain these last particulars, it ay be necessary to examine the extremities of the alum-hale on the sea-shore, and to trace its edges or boundaries or some miles inland; carefully distinguishing between he *alluvial* matters, properly so called, consisting of or ontaining *rounded stones* and broken and heterogeneous echanical mixtures, and the regular stratified matters, hich will somewhere be found in a regular range, covering he ferruginous rock and other strata which Mr. Winter as described; the nature and peculiar fossils of which trata, it will be very desirable to obtain an account of.

In Derbyshire, some are of opinion, that the great or imestone Shale which there covers the mineral limestone nd basaltic strata, as shown in Plate II of your thirty-first olume*, (and of which *Mam Tor* is composed,) is the ame soil or assemblage of strata, which produces alum at

Whitby

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[*] And is described by Mr. Whitehurst in his "Inquiry concerning the arth," 2d edit. p. 188; where however he is wrong in saying, that this shale ontains no vegetable impressions, since such occur at Shaw-Engine mine ere mentioned, and numerous other places. Mr. Mawe, in his "Mineraogy of Derbyshire," p. 24, has copied this error, and added a still greater ne, viz. that this shale "is not stratified."

Whitby and the adjacent parts of Yorkshire; and at Shaw-Engine mine in particular, near Eyam N. (where this stratum has been penetrated 360 feet deep to come at the vein of lead ore in the first limestone) alum is said to form a crust on the parts of the shale exposed. I cannot however consider this shale, and that at Whitby, as belonging to the same soil, but believe the latter to correspond with the great or clunch Clay of Bedfordshire, Cambridgeshire, Lincolnshire, &c., underlaying the Woburn Sand and ferruginous sand-stone strata, as hinted above: which clay abounds with selenite and pyrites, and at its exit from the island on the south coast in Dorsetshire, if I mistake not, takes fire on contact with the sea-water, as at Whitby; and whose highly bituminous shales, towards its lower part, have occasioned the useless expenditure of such immense sums of money in search of coals in this and past ages, in all its course through the English counties. At the east end of Bolinbroke town in Lincolnshire, in 1807, I observed these bituminous shale beds, and within a few miles, an expensive boring was at that time making, in search of coal-seams, which existed only in the ideas of the adventurer. Ludi Helmontii of various sizes and shapes, some long and cylindrical in shape, (of which sir Joseph Banks has specimens at his seat at Revesby) with thin shells investing them, which seem to prove their *animal origin*, are found at Bolinbroke, and elsewhere: Quere,—Are ludi found in the Whitby shales? or large anomia gryphus, perforated, or worm-eaten as it is called, near to the sand-stone?

Mr. Winter mentions the Whitby strata, as passing into the interior of Yorkshire and into Lancashire; but is it not more probable, that the ancient alum-works in Lancashire and the western parts of Yorkshire, which he alludes to, were situate on the shale of Castleton Mam Tor, above mentioned? and not on the Whitby shales; since the former contains sulphur sufficient for the production of the sulphuric acid, necessary in the production of alum. An alphabetical list of all the places, exactly describing their situation, where alum has at any time been made in the north of England*, which perhaps Mr. Winter by the assistance of his friends could furnish, would prove of considerable use towards deciding the geological questions above stated, as to the number and relations of the rich aluminous soils, in the British series of strata. The Whitby strata I have never had an opportunity of seeing, or any other where alum is manufactured; nor do I recollect what Mr. William Smith has ascertained, relative to the Whitby and other alum-shales. The valuable information which Mr. Winter has already given on this subject, is, as I hope, but the prelude to further details, which would greatly oblige,

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* Benjamin Martin's Natural History of England, vol. ii. p. 229, mentions alum as among the mineral products of Derbyshire; but I found no works, nor heard of any, in my recent examination of that county.

Sir, your obedient servant,
JOHN FAREY.

No. 12, Upper Crown Street,
Westminster, April 5, 1810.

P. S. The perusal of an extract in your last Number (p. 222), respecting the orbicular blocks of granite of Talavo and Sartene, and the doubts expressed, whether the latter received their present form by the effects of attrition or decomposition, induce me to state, that in a cursory examination of Charnwood Forest in Leicestershire, in August 1807, previous to entering on my Derbyshire survey, I found on Mount-Sorrel common, on the north and north-west of the windmill, several isolated blocks of sienite, from one to several feet of solid content each, which appeared at first sight like rounded stones, almost half imbedded in the soil; but on a closer examination I found them to be perfectly similar in kind to other blocks nearly cubical, which were lying about on the grass, of which blocks seats are made at the doors of many of the houses in Mount-Sorrel town, which most travellers that way must have observed. Seeing reasons for referring the rounding of the blocks in question to decomposition rather than attrition, I attempted, and succeeded in two or three instances, in turning over these blocks, by which it appeared evident to me, that attrition had had no share in giving them this semi-orbicular form; the bottom in contact with the soil, presenting a perfect face of the cubical form peculiar to these blocks, and no appearance of breaking or other violence: in a further search on this common, I found such a series of these blocks in all stages of their decomposition, as to leave no doubt on my mind on the subject.

In the progress of my subsequent survey, I have found numerous instances of round masses of granite, of very different species apparently, some very large, but none of these boulders admitted of any hesitation in referring their form to attrition, or in considering them as the alluvial ruins of very distant regions. One of these granitic boulders I found in the Buts pasture at Ashover, on the limestone, again in Bretby Park, on the red marle, some on Macclesfield

field common, Cheshire, on the coal-measures, some in alluvial marle on Werneth-Low Hill in Cheshire, (as mentioned vol. xxxiv. p. 50,) and in vast numbers in the vale of the Goyte from Whaley-bridge downwards, and in its various branches in Derbyshire and Cheshire: in numerous other places, single blocks are found. What I wish to impress on geological observers is, the necessity of stricter attention to alluvial matters, and not hastily to conclude whether round stones are boulders, or were formed by decomposition.—J. F.

XLV. *On Crystallography. By M.* Hauy. *Translated from the last Paris Edition of his* Traité de Minéralogie.

[Continued from p. 201.]

ON THE NOMENCLATURE OF MINERALS.

Mineralogy and the other natural sciences have been cultivated during a long course of years, without men of science being aware of the great influence possessed by words, the signs of our ideas, in facilitating the acquisition of their ideas themselves. The language of the sciences was not submitted to any fixed rule; the caprice of the nomenclators themselves decided both the choice and the number of the words which composed each denomination; and these words, frequently improper, or even calculated to lead to a fallacious interpretation, presented the double inconvenience of injuring the operation of the memory and of obscuring intellectual perceptions.

At length Linnæus undertook to make natural history speak a rational and methodical language, by reducing every denomination to two words, one of which should be common to the species denominated, with all those which belonged to the same genus, and the other should serve as the distinguishing sign of this species. The example of this illustrious man has been followed by all those who have since cultivated the study of nature with most success; and the authors of modern chemistry have adopted a similar precision in the idiom of that science, in which it is conjoined with a peculiar advantage originating from the very basis of the subject. It consists in this, that here, to name and to define are one and the same thing; and a mere collection of the names, such as fluate of lime, sulphate of barytes, &c. presents an abridged treatise on the science.

We have adopted this nomenclature wherever the knowledge we have acquired would admit of it; and amidst a crowd of examples which we could quote, in order to prove how much mineralogy has gained by this adoption, we shall confine ourselves to what is furnished by the word *spar*. Originally several species of minerals which had a lamellous and opal-like (*chatoyant*) texture were united under this head. Thus there were *calcareous spars, heavy spars, fluor spars, shining spars*, &c. At the time when the different bodies designated by this word composed a single genus, as seems to have been the case at the origin of the science, it was the method that was more in fault than the nomenclature, by identify ecies essentially different from each other. Subseque owever, these same bodies, having been better known, separated from each other and placed in different gen even in different classes, and nevertheless they we allowed to preserve their common denomination o and the inevitable alternative occurred, either to out a genus in order to disperse its members, whi ntrary to all the principles of the method, or to co under one and the same name, genera which in ot ects had nothing common, which is not less opposite principles of a good nomenclature. And as if t not enough of confusion occasioned by the spars ancient mineralogy, the abuse of this word has, e, multiplied in modern denominations; and henc ve boracic spars, adamantine spars, &c. The lang f modern chemistry, by suppressing the word *spar* i cidiferous substances, has given a kind of signal-post xtending the same reform to some of the earthy subst which still remained obscured by this erroneous appell on*.

As to the names of these last substances, they ought to be founded, at least for the present, on considerations foreign to the chemical nature of bodies; and it is even to be presumed that we shall not be yet able to refer them to the results of analysis, always supposing that we are not stopped by the prolixity of those which should be applied to substances composed of three or four earths intimately combined with each other. Whatever may be the case, names are wanted which could serve during an indefinite time, and this was one reason for also making in this part of the language of the science all the changes which should not produce too many inconveniences.

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* We have preserved this word in the denomination of *feldspar* only, which has been now too generally used not to be respected, and about which here can be no ambiguity, because it is no longer employed on any other occasion.

But in order the better to show the advantages of those which I have fixed upon, it will not be useless to explain, before any thing else, the principles to which, in my opinion, the formation of names independent of analysis ought to be subjected.

For a long time, persons have been in the habit of giving to mineral substances, names borrowed from those of the places where they were discovered. It seems to me that this is to invert the use of these names, which ought only to serve for designating individuals or particular bodies, as, when speaking of an *idiocrasis* the locality of which we wish to point out, we say that it is an idiocrasis from Vesuvius, or an idiocrasis from Siberia. If we substitute instead of *idiocrasis* the term *vesuvian,* which is adopted in Germany, the former expression will have the air of a pleonasm, and the second will appear contradictory.

Others derive the new denominations from the colour under which the substance is presented to the first observers. This is to transfer to the species the name of the variety. For example, *yanolite* (violet stone) is the substance which we call *axinite.* But there are crystals of this substance which are green, and in this case the name of green *yanolite* merely expresses an imaginary substance.

We ought also to avoid confounding the name of one mineral with that of another, with a different inflection; as when persons called *hyacinthine* the substance which we call *idiocrasis,* without doubt in order to recall the appearances ascribed to it in common with the hyacinth (zircon in the present system), with which it had been at first united. The truth is, that it differs from it very sensibly, either by its component principles or its structure, or even by the angles of its crystals; and it is sufficient to regard it for a moment in order to decide with Romé de l'Isle, that they ought to be separated from each other. So far therefore from indicating by the resemblance of the names a pretended connexion, which to good eyes does not exist, we ought rather to mark between the two substances a distinct line of separation, by a new name which could have nothing in common with the first, and which would make us forget, if possible, an error which mineralogists ought never to have committed.

With respect to insignificant names to which several naturalists give the preference, there is nothing to prevent their adoption. Of this number are the names derived from fable,

ich as titanium, uranium, &c. The ideas which they
esent are so far from carrying us to the objects which they
rve to designate, that they can neither occasion mistake
r confusion; so that they are in the same situation as
they were purely imaginary. Sometimes also persons
cribe to a natural production the name of the person who
scovered it; but it would be rather too severe to condemn
tirely this way of paying a kind of homage for a favour
nferred on science.

It appears to me that there is more advantage in em-
loying significant words, which recall some characteristic
roperty of the mineral to be denominated, or some cir-
umstance relative to its his [illegible] But because this mineral
frequently distinguished fr [illegible] thers only by its general
haracters, we ought not to r [illegible] that the name, which can
ear upon a single character [illegible] should make the object it
esignates apparent without [illegible] guity. Moreover, if we
onsider that the characters [illegible] inerals are susceptible of
ariation, we must admit t [illegible] e nomenclator may here
llow himself considerable [illegible], and it is sufficient that
ach word should rest o [illegible] idea connected with a
nowledge of the object. [illegible] ut this latitude, it would
e almost impossible to [illegible] nificant, *i. e.* reasonable,
ames. In a subject so fra [illegible] ith difficulty, every thing
admissible, except what [illegible] usable.

Now it must be confess [illegible] the French language is
ot well adapted to furnish [illegible] ficant words without the
elp of periphrases, whic [illegible] eed the narrow bounds
ithin which true names ought to be confined. Let
his language display in the descriptions of objects the
learness and precision which characterize it; but let
he specific names be furnished by the Greek, which
as the eminent advantage of moulding several words
nto one, so as to paint in miniature the object which
t serves to denominate. In this way a multitude of
ords have been formed which are used in the arts and
ciences. Every day these words are multiplied; the in-
trument which transmits ideas to a distance in the twink-
ing of an eye, is the *telegraph*; the art of writing written
ords with rapidity is called *stenography*, &c. Wherefore
hen should we banish the Greek language from the coun-
ry of the sciences, where it has been so long naturalized,
nd where every new expression introduced by necessity,
inds itself as it were in the same family with a thousand
thers which preceded it?

It is from the same source that I have derived the names
which

which I have added to the nomenclature of mineralogy. Different motives suggested their formation; and there were two circumstances in particular, in which it was indispensable to compose new names: viz. when a species hitherto unknown was the subject of description, and when several different species had been confounded. In the latter case I generally left to one of the species the name which they had borne in common, and I designated the rest by particular denominations.

I was almost confined to these changes from absolute necessity, in the extract which formerly appeared of this treatise; and besides, I had allowed all the names already printed, to remain, however improper they might be. But, since, it has been observed to me that it would be proper to reform in the same manner several names which I had omitted, such as *leucite* and *leucolite*, one of which signifies a *white body*, and the other a *white stone*, *smaragdite*, which is nearly synonymous with *emerald*, *oisanite*, *andreolite*, *thallite*, and some others borrowed from the localities or from the colours. These names were found vicious in a twofold degree, either from their impropriety when considered in an isolated point of view, or from the monotony of their terminations when they happen to resemble each other closely. Besides all this, they were very few in number, and are only found in very modern works. In a word, it was thought that the interests of science, which had determined the first changes, would also suggest those which were proposed to me. I hesitated no longer from the moment I found myself supported by men of science whose reasons seemed to me to be decisive, and whose authorities alone have the weight of reasons; and I am the more anxious thus publicly to declare the motives which actuated me, because it would give me pain to be accused of allowing myself to be led away by neologism. I certainly think there is a great difference in every respect between making new names and advancing new theories. The one is the result of a mere technical labour, which interferes with the dictionary of science only; the other presupposes views which tend to aggrandize the edifice. A truth newly made known is instantly adopted, because it insinuates itself into the mind by means of persuasion. But the novelty alone of words which reach the ear for the first time, throws a shade of disapprobation over them: he who proposes them seems to assume an authoritative air; they are rejected without reflection and without examination, or they are censured perhaps, while at the same moment all are

greed as to the utility of a change. But naturalists, who
ter long consideration undertake a task so painful, so
stidious, and so little calculated to indemnify them for
ieir trouble, ought to have nothing but science in view,
siring no advantage but that of being useful, and dreading
ie reproach alone of not having done all that a true re-
ird for science demands of them.

To conclude:—Those who would still cherish a predi-
ction for the suppressed denominations, will find them in
e same line with those which I have substituted, and may
ill continue to use them. But I hope that beginners, on
mparing both nomenclatures, will store their memory, ac-
rding to my plan, with names so constructed as to en-
hten their minds. I have taken care to add to these
mes their etymologies, and I have done the same with
the rest, whether new or old ienever there was a pos-
ility of ascertaining their or

OF THE NOMENCL OF CRYSTALS.

If the language of minera s been so long defective,
m the bad choice of spec ies, the almost total de-
iency of names with respe he varieties of crystalli-
tion has left a void, whic o less an inconvenience.
iere was no exception, ith respect to a small
mber of these varieties, rms of which were so
nple that they would s s if of themselves the
ithets of *cubical*, *octahe* *decahedral*, &c., which
ght to be added to the na the species. The more
mpound forms were indi by definitions, the length
which was in some measure proportional to the number
the facets; or, if it was wanted to abridge these defini-
ns, by borrowing them from a resemblance between the
stal and some familiar object*, this was done with so
le rationality, that it would have been desirable for the
nour of the comparison if such names were less known.
Convinced of the necessity of introducing the utmost
cision into this part of mineralogical language, so much
glected hitherto, I have attempted to designate the va-
us crystalline forms by simple and significant names,
en from the characters of these forms, or from the pro-
ties which result from their structure, and from the
s of decrement on which they depend. I shall here
sent my readers with the series of these names, under
form of a methodical system. I hope that those who
use it with attention will find an assistant for engraving

The following are examples of this kind: *nail-headed calcareous spar*,
toothed calcareous spar, &c.

these names on their memory, by connecting them with considerations which are easily classified in the mind. They will perceive that, by a kind of œconomy of language, extremely useful in such cases, the same name is frequently applicable to varieties taken in different species. It is true that on one hand the word which serves to designate such a variety might also serve another variety of the same species. For example: I denominate *binary*, a form which depends on a decrement by two ranges. Now supposing this decrement to take place on the edges, it is possible that another variety of the same substance may be owing to a decrement which takes place by two ranges on the angles. But in this case the system will present for the latter another name borrowed from a different consideration. The inconvenience just mentioned is common to all nomenclatures, and seems unavoidable. Thus, in the language of botany, one variety will bear the name of *crassifolia*, or of *rotundifolia*, while another variety of the same species shares with the first the character which has served to distinguish it. The essential requisite is, that the method should be copious enough to furnish at least to all the known wants of science. I even expect that, by means of the labour I have performed, a great part of the forms which shall be discovered in future will be found to have been named beforehand; and as to those which require new names, we shall have at least a system from which to designate them. In all descriptions of researches, it becomes easier to go forward when the route is traced.

Principles of the nomenclature.—The primitive form of any given substance is always designated by the word *primitive* added to the name of the species. Examples:—*primitive zircon*, *primitive carbonated lime*, *primitive sulphurated lime*, &c.

We may consider secondary forms:—

1. With respect to the modifications of the primitive form, when the faces of the latter are combined with those which result from the laws of decrement.
2. By themselves, and as purely geometrical forms.
3. With respect to certain facets or certain ridges remarkable by their assortment or their positions.
4. With respect to the laws of decrements on which they depend.
5. With respect to the geometrical properties which they present.
6. Finally, with respect to certain particular accidents.

Secondary forms considered with respect to the modifications which they present of the primitive form.

The crystal is called,

Pyramidated (*pyramidé*), when the primitive form being a prism, has on each of its bases a pyramid which has as many faces as the prism has panes. Example: Pyramidated phosphated lime.

Prismated (*prismé*), when the primitive form being composed of two pyramids joined at their bases, these pyramids are separated by a prism. Ex. Prismated zircon, prismated quartz.

Semi-prismated, when there is only the half of the number of ridges situated around the common base, which are intercepted by panes. Ex. Semi-prismated sulphated lead.

Based (*basé*), when, the primitive form being a rhomboid, or an assemblage of two pyramids, the summits are intercepted by facets perpendicular to the axis, and performing the function of bases. Ex. Based carbonated lime, based sulphur.

Pointed (*epointé*), when all the solid angles of the primitive form are intercepted by solitary facets. Ex. Pointed mesotype.

We shall also use the terms *bi-pointed* (*bisépointé*), tripointed (*triépointé*), quadripointed (*quadriépointé*), according as each solid angle may be intercepted by two, three, or four facets. Ex. Tripointed analcime, quadripointed sulphurated iron.

Marginated (*émarginé*), when all the ridges of the primitive form are each of them intercepted by a facet. Ex. Marginated garnet.

We shall also use the term *bi-marginated*, *tri-marginated*, as each ridge is intercepted by two or three facets. Ex. Trimarginated garnet.

Peri-hexahedral, *peri-octahedral*, *peri-decahedral*, *peri-dodecahedral*, when the primitive form being a prism with four panes, is changed by the effect of decrements into a hexahedral, octahedral, decahedral, or dodecahedral prism. We also denominate *peri-dodecahedron* a crystal the nucleus of which being a regular hexahedral prism, has its six longitudinal ridges intercepted by as many facets. Ex. Peri-hexahedral sulphated copper, peri-dodecahedral emerald.

[illegible] (*[illegible]*), when the primitive form being a prism with rhombic bases, the longitudinal ridges contiguous to the grand diagonal are intercepted by two facets, which make it appear diminished in the direction of its length. Ex. Recurved sulphated barytes.

h. *Retreated* (*rétréci*), when the primitive form being a prism with rhombic bases, the longitudinal ridges contiguous to the small diagonal are intercepted by two facets which make it appear diminished in the direction of its breadth. Ex. Retreated sulphated barytes.

2. *Secondary forms considered in themselves, and as being purely geometrical.*

The crystal is called,

a. *Cubical*, when it presents the form of the cube, which in this case is always secondary. Ex. Cubical fluated lime.

b. *Cuboidal*, when its form differs a little from the cube. Ex. Cuboidal carbonated lime.

c. *Tetrahedral*, when it presents the form of the regular tetrahedron, as a secondary form. Ex. Tetrahedral sulphurated zinc.

d. *Octahedral*, when it presents the form of this solid, as secondary. Ex. Octahedral muriated soda.

e. *Prismatic*, when it has the form of a straight or oblique prism, the panes of which are inclined 120° among each other. Ex. Prismatic carbonated lime, prismatic feldspar.

f. *Dodecahedral*, when its surface is composed of twelve triangular, quadrangular, or pentagonal faces, all equal and similar, or solely of two measurements of different angles. Ex. Dodecahedral quartz, dodecahedral zircon, dodecahedral sulphurated iron.

If the dodecahedron had not all its faces of the same number of sides, it would be sufficient to bring them to this aspect in imagination, by varying its dimensions.

g. *Icosahedral*, when its surface is composed of twenty triangles, of which twelve are isosceles and eight equilateral. Ex. Icosahedral sulphurated iron.

h. *Trapezoidal*, when its surface is composed of twenty-four equal and similar trapezoids. Ex. Trapezoidal garnet.

i. *Triacontahedral*, when its surface is composed of thirty rhombuses. Ex. Triacontahedral sulphurated iron.

k. *Enneacontahedral*, when its surface is composed of 90 faces. Ex. Enneacontahedral idiocrasis.

l. *Birhomboidal*, when its surface is composed of twelve faces, which being taken by sixes, and lengthened in imagination until they intersect, would form two different rhomboids. Ex. Birhomboidal carbonated lime.

We say *trirhomboidal* in the same manner. Ex. Trirhomboidal carbonated lime.

Biform, triform, when it contains a combination of two or three remarkable forms; such as the cube, the rhomboid, the octahedron, the regular hexahedral prism, &c. Ex. Triform sulphated alumine.

ubo-octahedral, cubo-dodecahedral, cubo-tetrahedral, &c., when it contains a combination of the two forms indicated by these expressions. Ex. Cubo-octahedral fluated lime, cubo-dodecahedral sulphurated iron, cubo-tetrahedral gray copper.

rapezian, when its lateral surface is composed of trapezia situated on two rows between two bases. Ex. Trapezian sulphated barytes.

Ditetrahedral, i. e. twice tetrahedral, when its form is that of a tetrahedral prism with dihedral summits. Ex. Ditetrahedral grammatite.

Dihexahedral, when it forms a hexahedral prism with trihedral summits. Ex. Dihexahedral feldspar.

We say in the same manner, *dioctahedral, didecahedral, didodecahedral*. Ex. Dioctahedral topaz, didecahedral feldspar, didodecahedral phosphated lime.

rihexahedral, tetrahexahedr pentahexahedral, heptahexahedral, when its surf is composed of three, four, five, seven rows of faces disposed in sixes the one above the other. Trihexahedral nitrated potash, pentahexahedral z, heptahexahedral nitrated potash.

We also say in the same manner, *tridodecahedral*. Ex. Tridodecahedral sulphurated antimoniated silver. —*Trioctahedral*. Ex. Trioctahedral sulphated lead.

Bigeminated, when it presents a combination of four forms, which, taken by twos, are of the same species. Ex. Bigeminated carbonated lime.

Amphihexahedral, i.e. *hexahedral* in two ways, when by taking the faces according to two different directions, we have two hexahedral contours. Ex. Amphihexahedral axinite.

Sexdecimal, when the faces which belong to the prism or to the middle part, and those which belong to the two summits, are the former six in number, and the latter ten in number, or *vice versâ*. Ex. Sexdecimal feldspar.

In the same manner we say *octodecimal*. Ex. Octodecimal feldspar. *Sexduodecimal*. Ex. Sexduodecimal carbonated lime. *Octoduodecimal*. Ex. Octoduodecimal sulphated copper. *Deciduodecimal.* Ex. Deciduodecimal feldspar.

x. *Peripolygonal,* when the prism has a great number of panes. Ex. Peripolygonal tourmaline.

y. *Supercomposite,* when the form is very much compounded. Ex. Supercomposite tourmaline.

z. *Antienneahedral,* i. e. having nine faces on two opposite sides, is a name peculiar to a variety of the tourmaline, in which the two summits are of nine faces, and the prism of twelve panes; whereas, generally, the prism is enneahedral.

a a. *Prosenneahedral,* i. e. having nine faces on two adjacent parts, is another variety of the tourmaline, in which the prism and one of the two summits have each nine faces.

b b. *Recurrent,* when, on taking the faces of the crystal by annular rows, from one extremity to the other, we have two numbers, which succeed several times, as 4, 8, 4, 8, 4. Ex. Recurrent oxidated tin.

c c. *Equidifferent,* when the numbers which designate the faces of the prism and those of the two summits, which in this case differ from each other, form the commencement of an arithmetical series, as 6, 4, 2. Ex. Equidifferent amphibolus.

d d. *Convergent,* when in the foregoing case the series is sensibly convergent, as 15, 9, 3. Ex. Convergent tourmaline.

e e. *Unequal* (*impair*), when the numbers which designate the panes of the prism and the faces of the two summits, considered as different from each other, are all three unequal, without being in other respects in progression. Ex. Unequal tourmaline.

f f. *Hyper-oxidated,* i. e. *acute to excess,* is a variety of carbonated lime, which contains the combination of two rhomboids; the one acute, which is the inverse; the other incomparably more acute.

g g. *Spheroidal,* is said of the diamond with 48 bombated faces.

h h. *Plano-convex,* is the diamond with some plane and some curvilinear faces.

3. *Secondary forms considered relatively to certain facets, or certain ridges, remarkable for their arrangement or position.*

The crystal is called,

a. *Alternate,* when it has on its two parts, the one superior and the other inferior, faces which alternate among each other, but which correspond on both sides. Ex. Alternate quartz.

Bisalternate, when in the foregoing case the alternation takes place, not only among the faces of one and the same part, but also among those of the two parts. Ex. Bisalternate carbonated lime, bisalternate quartz.

Bibisalternate, when there are on both sides two orders of bisalternate facets. Ex. Bibisalternate sulphurated mercury.

Annulary, when a hexahedral prism has six marginal facets ranged in form of a ring around each base. Ex. Annulary emerald.

We say the same of an hedral prism with eight marginal facets around b Ex. Annular oxidated tin.

Monostic, when a prism of given number of panes has, in the contour of base, a row of facets in number different from the panes, and which may be all marginal, or marginal and others angular. Ex. Monostic t

Distic, when in the sam e there are two rows of facets around each base Distic topaz.

Subdistic, when among ets arranged on one and the same row around e e, two are surmounted each by a new facet, w as it were the rudiment of a second row. Ex. stic peridot.

Plagihedral, when the cry has facets situated in a slanting direction. Ex. Plagihedral quartz, plagihedral zircon.

Dissimilar, when two rows of facets, situated the one above the other, towards each summit, have a defect in symmetry. Ex. Dissimilar topaz.

Squared (*encadré*), when it has facets which form kinds of squares around faces of a simpler-form already existing in the same species. Ex. Squared fluated lime.

Slightly prominent (*prominule*), when it has ridges which form a very slight eminence. Ex. Slightly prominent sulphated lime.

Zonary, when it has around its middle part a row of facets, which form a kind of zone. Ex. Zonary carbonated lime.

Apophanous, i.e. *manifest*, when certain facets of certain ridges present some indication useful for ascertaining the position of the nucleus, which would otherwise be difficult to find out, or even to determine, either in point of direction or the measurement of the decrements. Ex. Apophanous feldspar, apophanous sulphurated antimoniated silver, apophanous gray copper.

l. *Blunted* (*emoussé*), when it has facets which intercept, and render as if blunted, some parts which would otherwise be sharper than the rest. Ex. Blunted axinite, blunted carbonated lime.

m. *Contracted,* is a dodecahedral variety of carbonated lime, in which the bases of the extreme pentagons undergo a kind of contraction, in consequence of the inclination of the lateral faces.

n. *Dilated,* is said of another dodecahedral variety of carbonated lime, in which the bases of the extreme pentagons undergo a kind of dilatation, in consequence of the inclination of the lateral faces.

o. *Acuteangled,* is a variety of carbonated lime in a hexahedral prism, the solid angles of which are intercepted by very sharp triangular facets.

p. *Defective,* is a variety of borated magnesia, in which four solid angles of the primitive cube are intercepted by facets, while the opposite angles remaining untouched, are subject to a kind of defect.

q. *Superabundant,* is another variety of borated magnesia, in which the solid angles which were untouched in the defective variety, are intercepted each by four facets, in such a way as to make a superabundance where there was a defect.

4. *Secondary forms considered relatively to the laws of decrement on which they depend.*

The crystal is called,

a. *Unitary,* when it undergoes only a single decrement by one row. Ex. Unitary telesia. If there are two, three, four decrements by one row, we say bisunitary, triunitary, quadriunitary. Ex. Triunitary peridot, bisunitary carbonated lime.

b. *Binary, bibinary, tribinary,* &c. in the case of one, two, and three decrements by two rows. Ex. Binary oligistous iron, bibinary feldspar.

c. *Ternary, biternary,* &c. in the case of one, two decrements, &c. by three rows.

d. *Unibinary,* if there are two decrements, the one by one row, the other by two; *uniternary,* if there is one by one row, and the other by three; *binoternary,* if there is one by two, and the other by three, &c. Ex. Uniternary carbonated lime, binoternary carbonated lime.

The

The nomenclature in all the foregoing expressions, as well as in those which follow, makes an abstraction of the faces parallel to those of the nucleus, which exist most frequently in the secondary crystal. Among the forms in which the nucleus is entirely disguised, some have names borrowed from different considerations; and those which remain are so few in number, that I thought it unnecessary to complicate the language by employing a particular designation for them.

In order to avoid confounding the words which express the decrements with those which indicate the number of the faces, we may remark, that the former have their termination in *dral*, as dodecahedral, or in *al*, as octodecagonal, wh eas the others end in *ary*.

e. *Equivalent*, when the par isible (*exposant*) which indicates a decrement i to the sum of those which indicate the others. quivalent sulphated iron.

f. *Subtractive*, when the visible relative to a decrement is less by unity he sum of those which indicate the others. Ex. btractive pyroxene.

g. *Additive*, when the part ble relative to a decrement exceeds by unity the f those which indicate the others. Ex. Additi hated copper.

h. *Progressive*, when the visible form a commencement of arithmetic gression; as 1, 2, 3. Ex. Progressive tourmali

i. *Disjointed*, when the ents form an abrupt leap, as from 1 to 4 or to 6. Disjointed sulphurated antimoniated silver.

k. *Partial*, when there is some part which remains without decrements, while the other parts similarly situated undergo them. Ex. Partial sulphurated cobalt.

l. *Subdouble*, when the part visible relative to a decrement is the half of the sum of the other parts visible. Ex. Subdouble topaz.

We say *subtriple*, *subquadruple*, &c. in the same way. Ex. Subtriple sulphated copper.

The three parts visible (*exposans*) which compose the indication of an intermediary decrement, count as one only, which is equal to their sum.

m. *Doubling*, *tripling*, *quadrupling*, when one of the visible parts is repeated twice, thrice, or four times in one series which would otherwise be regular. Ex. Doubling peridot, quadrupling peridot.

n. *Identical*, when the parts visible of the simple decrements, to the number of two, are equal to the terms of th

fractio

fraction relative to a third decrement which is mixed. Ex. Identical gray copper.

o. *Isonomous,* i. e. *equality of laws,* when the parts visible which indicate the decrements on the edges being equal, those which express the decrements on the angles are equal also. Ex. Isonomous sulphated copper.

p. *Mixed,* when the form results from a single mixed decrement. Ex. Mixed telesia.

q. *Pantogenous,* i. e. *deriving its origin from all the parts,* when each ridge and each solid angle undergoes a decrement. Ex. Pantogenous sulphated barytes.

r. *Biferous,* i. e. *which carries twice,* when every ridge and every solid angle undergoes two decrements. Ex. Biferous gray copper.

s. *Surrounded,* (*entouré*) when the decrements take place on all the ridges and on all the solid angles around the base of a prismatic nucleus. Ex. Surrounded sulphated barytes.

t. *Opposite,* when a decrement is made by one row, and another is intermediary. Ex. Opposite oxidated tin.

u. *Synoptic,* when the laws of decrement present as it were the picture of those which take place with respect to the whole of the other crystals, or at least with respect to the greatest part. Ex. Synoptic feldspar.

x. *Retrograde,* is a variety of carbonated lime, the expression of which contains two mixed decrements, which are such that the faces resulting from them seem to retrograde, by throwing themselves backward, on the side of the axis opposite to that which looks towards the face on which they originate.

y. *Ascending,* when all the laws of decrement have an ascending course, setting out from the angles or lower edges of a rhomboidal nucleus. Ex. Ascending carbonated lime.

5. *Secondary forms considered relatively to the geometrical properties which they present.*

The crystal is called,

a. *Isogonous,* i. e. *equality of angles,* when the faces which are on parts differently situated form equal angles between each other. Ex. Isogonous cymophane.

b. *Anamorphic,* i. e. *form turned upside down,* when we cannot give it the position most natural to it, without that of the nucleus being as it were turned upside down. Ex. Anamorphic stilbite.

c. *Rhombiferous,* when certain facets are true rhombuses, although, from the manner in which they are cut by the adjoining faces, they do not appear at the first glance to be of a symmetrical figure. Ex. Rhombiferous quartz.

Equiaxis, when it has the form of a rhomboid the axis of which equals that of the primitive rhomboid. Ex. Equiaxis carbonated lime.

Inverse, when it has the form of a rhomboid the salient angles of which are equal to the plane angles of the primitive rhomboid, and *vice versâ*. Ex. Inverse carbonated lime.

Metastatic, i. e. *transferred*, when it has plane angles and solid angles equal to those of the nucleus which are thus transferred to the secondary form. Ex. Metastatic carbonated lime.

Contrasting, when it has the form of a very acute rhomboid, in which an inversion of angles similar to that which takes place in the ir rse (letter *e*) presents a kind of contrast, in so far as it resembles in another part a very obtuse rhomboid. Ex. Contrasting carbonated lime.

Persisting, is a variety of car iated lime in which certain faces are cut by the ad ining faces, so that they preserve the same measurements of angles which they would have had without that, except that these angles have other respective positic is. Ex. Persisting carbonated lime.

Analogic, when its forn ents several remarkable analogies. Ex. Analog onated lime.

Paradoxal, when its structure presents singular and unexpected results. Ex. Paradoxal carbonated lime.

Complex, when its structure is complicated by laws not very common, as when it is produced by decrements some mixed and others intermediary. Ex. Complex carbonated lime.

6. *Secondary forms considered relatively to certain particular accidents.*

The crystal is called,

Transposed, when it is composed of two halves of an octahedron, or of two portions of another crystal, one of which seems to have turned upon the other in a quantity equal to a sixth of its circmference. Ex. Transposed spinel, transposed sulphurated zinc.

Hemi-trope, i. e. *one-half reversed*, when it is composed of two halves of one and the same crystal, one of which seems to be reversed. Ex. Hemi-trope feldspar.

- *c*. *Rectangular*, a particular name given to the staurotide composed of two prisms which cross at right angles.
- *d*. *Obliqueangled*, a particular name given to the staurotide composed of two prisms which cross at an angle of 60°.
- *e*. *Sexradiated*, a name given to the staurotide composed of three prisms which cross so as to represent the six radii of a regular hexagon.
- *f*. *Cruciform*, a name given to the harmotome composed of two crystals which form a kind of cross.
- *g*. *Triglyphous*, when the striæ considered on three faces united around one and the same solid angle, are in three directions perpendicular to each other. Ex. Triglyphous sulphurated iron.
- *h*. *Geniculated*, when it is composed of two prisms which unite by one extremity forming a kind of knee. Ex. Geniculated oxidated titanium.

In the descriptions of the species we shall meet with a small number of denominations which we have here omitted. But their signification will be immediately obvious, or will resolve itself into that of some of the foregoing denominations.

[To be continued.]

XLVI. *On a Method of examining the Divisions of astronomical Instruments.* *By the Rev.* WILLIAM LAX, *A.M., F.R.S.,* Lowndes's *Professor of Astronomy in the University of Cambridge. In a Letter to the Rev. Dr.* MASKELYNE, *F.R.S. Astronomer Royal*[*].

St. Ibbs, August 27, 1808.

DEAR SIR, I AM persuaded that you must feel, in common with myself, how unpleasant it is to make use of an instrument in astronomical observations requiring extreme accuracy, whose exactness you have no adequate means of ascertaining, but are obliged to depend for it in a great measure upon the abilities and integrity of the artist. It is in vain that we observe with so much nicety, and read-off with so much precision, if we are still uncertain whether there may not be an error in the instrument itself of much greater magnitude than those which we are endeavouring to prevent; and that our best instruments must be liable to such errors, no person can possibly doubt, who has paid due attention to the sources from whence they may arise. I have estimated, as accurately as I could, the amount to which they may accumulate in Bird's method of dividing by continual bisections, and have satisfied myself that they are much more considerable than is generally appre-

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[*] From the Philosophical Transactions for 1809, Part II.

prehended: but as I cannot obtain such precise informa-
n as I could wish, respecting the exactness with which
isection can be performed, or a length taken from the
ale of equal parts and laid upon the instrument, I will
t trouble you with the deduction which I have made.
is understood, indeed, that Bird's method is now generally
d aside, and that each artist employs one, which he con-
ers in many respects as peculiar to himself; but I pre-
med that there would still be such a connexion betwixt
rd's method and those which have been substituted in its
ad, as to render them in some degree liable to the same
ors to which it was subject the reports which I have
iformly received from person ho have had an oppor-
nity of examining some of th dern instruments, have
ly convinced me that my o n was just. But what-
er may be the nature of th hods which are now in
e, or whatever their advan er Bird's, I never could
rsuade myself that it woul fe to trust to an instru-
ent, without a previous exa tion. To discover the
eans of accomplishing th ct, is what I have for
me time been anxious to and though I fear my
deavours have not been v cessful, I will neverthe-
s take the liberty of prese ou with the result.
You are aware, I believe use a circular instru-
ent for observing both i de and azimuth, which
s made for me by Mr. C he Strand; that the ra-
is of both the altitude an muth circle is one foot,
d that each is divided into ontaining ten minutes.
e construction of this instrument does not differ ma-
ially from that of other similar instruments, with which
u are well acquainted, and I shall not therefore waste your
e by giving you a particular description of it. For the
rpose of examining the divisions upon the two circles, I
ocured an apparatus to be prepared by Mr. Carey, which
ll be very easily explained. To the face of the rim which
rrounds the azimuth circle, and with its left end close
the stand which supports the micrometer on the east
e, an arc of brass, concentric with the circle itself, and
ittle more than 90° in length, an inch in breadth, and
e eighth of an inch in thickness, is firmly fixed by
ews, so as to have the plane parallel to the plane of the
cle, and a small portion of its lower surface resting upon
extreme part of the rim. The screws pass through a
ss arc, which is fastened to this at right angles, and lies
h its broad side against the face of the rim. Upon the
t-mentioned arc, a strong upright piece of brass, about

six inches in length, is made to slide, the lower part of it embracing the arc as a groove, and having a clamping screw underneath, for the purpose of fixing it firmly to the arc at any point required. To the top of the upright piece of brass is attached a microscope, with a moveable wire in its focus, pointing down to the division upon the circle, not directly, however, but with an inclination to the left of about 30°. This inclination is given to it, in order to make it point to the same division upon the circle, which is immediately under the micrometer itself, when it has been moved up as near to the micrometer as it is capable of approaching. The microscope has attached to it a small graduated circle of brass, and an index, by which the seconds, and parts of a second, moved over by the wire are determined.

To the vertical circle there is likewise an arc applied, of the same length and breadth as the former, but somewhat thicker, and of a radius exceeding that of the circle by about two inches. This greater thickness is given to it, on account of its being supported in a manner which renders additional strength necessary. It is fixed with its broad convex side downwards upon two brass pillars, screwed fast to the plane of the azimuth circle, and standing in a line parallel to the plane of the vertical circle at the distance of about four inches from it, and on the right side of the pillars which support the micrometers belonging to this circle. The pillar, to which the left end of the arc is fastened, is placed close to the lower micrometer of the vertical circle, and the other contiguous to the elevated rim, in which the divisions of the azimuth circle are cut. The right end of the arc reaches beyond this pillar about ten inches. The pillars are of such a height, and so proportioned to each other, that whilst the left end of the arc, which lies horizontally, is raised to within about two inches of the height at which the lowest point of the vertical circle is placed, the whole arc runs parallel to the circle through an extent of something more than 90°. Upon the arc a microscope, with a moveable wire in the focus, is made to slide as in the former case, and to point to the divisions upon the vertical circle, not directly, but with an inclination of about 30° to the left, in order that the same division (which is the lowest upon the circle) may be seen through it and through the lower micrometer at the same time.

I will now proceed to show you in what manner the examination of the divisions upon either circle may be

rformed. The process is precisely the same in both
d will of course be described in the same words.

The first point to be examined is that of 180°,
ust be done in the usual way, by bringing the poi
and 180° to the moveable wires of the opposite n
eters, and then turning the circle half-way round
secting the points again with the moveable wires
stly, taking half the difference betwixt the distan
e wires in the two positions of the circle for the e
e point of 180°. Having now bisected the point o
ith the moveable wire of the micrometer, which
nded to be used in the rest of the process (for w
ave no further occasion for both), we must slide th
oscope along the arc, till by moving the wire a lit
n bisect the point of 90°, and then the micrometer
firmly clamped to the arc. The circle must t
rned till the point of 180° is brought to the micro
d that of 90° to the micrometer, so that we may b
bisect each by a slight motion of their respective
his being done, we must observe, from the positi
e wires, how much the interval betwixt them h
eased or decreased in the measurement of the ne
d this increase or decrease must be noted down
- or — accordingly. In the same manner we mus
ed through the remaining two arcs of 90°, observin
oting down the difference betwixt each and the o
c.

The point of *zero* must now be brought again
icrometer, and bisected by the moveable wire, a
icroscope be made to slide back along the arc,
oving the wire a little we can bisect the point of 60
hen this is done, the microscope must be clamped.
ust then measure the arc of 60° against every succ
c of 60° in the circle, precisely in the same way t
easured the first arc of 90° against the other three.
c of 45° is next to be measured against every succ
c of 45°, and this will complete all that is neces
done in the early part of the morning before the h
e sun can have affected the temperature of the i
ent. The rest may be performed at our leisure.

You will immediately perceive the object of thi
f measurement. It enables us to determine, wit
egree of accuracy that may be required, the prop
hich the first and every succeeding arc of the circle
ined betwixt the micrometer and the microscope,
the whole circle, and of course the absolute len

the arcs themselves. Let a denote the real length of the first of these, and $\pm a'$, $\pm a''$, $\pm a'''$, &c., the difference betwixt the first and second, the first and third, &c. respectively; let A represent any other arc whose length is known, and which is a multiple of a, as marked upon the instrument, and let this multiple be expressed by n. Then will $a + (a + a') + (a + a'') + (a + a''') + \&c. \ldots (a + a'''^{\ldots\overline{n-1}}) = A$, and $a = \frac{A - a' - a'' - \ldots a'''^{\ldots\overline{n-1}}}{n}$. Hence it is evident, that if there is no error committed in the measurement of any of these arcs, we shall have the value of a, and consequently of $a + a'$, $a + a''$, $a + a'''$, &c., and of any arc, comprehending any number of these, accurately determined. But if there be an error of e in the measurement of the first, of e', e'', e''', &c., in the measurement of the second, third, &c., respectively, then we shall have the following equation for determining a, viz. $a + (a + a' + e + e') + (a + a'' + e + e'') + \&c. \ldots (a + a'''^{\ldots\overline{n-1}} + e + e'''^{\ldots\overline{n-1}}) = A$, and consequently a will appear to be equal to $\frac{A - a' - a'' - \ldots a'''^{\ldots\overline{n-1}} - \overline{n-1}\,e - e' - e'' - \ldots e'''^{\ldots\overline{n-1}}}{n}$, which differs from its true value by $\frac{\overline{n-1}\,e + e' + e'' + \ldots e'''^{\ldots\overline{n-1}}}{n}$. Hence it follows, that the value of the p^{th} arc (p being greater than unity), as deduced by this process, will differ from its true value by $\frac{\overline{n-1}.e + e' + e'' + \ldots e'''^{\ldots\overline{p-1}} + e'''^{\ldots p} + \ldots e'''^{\ldots\overline{n-1}}}{n} - e - e'''^{\ldots\overline{p-1}}$, and that if we add any number p of these arcs together, in order to determine the value of the arc which is equal to their sum, we shall have an error in this value (and the expression holds when p is unity, or the first arc only is taken) equal to $p\,\frac{\overline{n-1}.e + e' + e'' + \ldots e'''^{\ldots\overline{p-1}} + e'''^{\ldots p} + \ldots e'''^{\ldots\overline{n-1}}}{n} - \overline{p-1}.e - e' - e'' - \ldots e'''^{\ldots\overline{p-1}} = \frac{\overline{n-p}.\overline{e - e' - e'' - \ldots e'''^{\ldots\overline{p-1}}} + p.e'''^{\ldots p} + e'''^{\ldots\overline{p+1}} + \ldots e'''^{\ldots\overline{n-1}}}{n}$. Now, if we suppose e to be the greatest error to which we are liable in the measurement of any arc, and each of the succeeding errors to be equal to it, and likewise that e', e'', $\ldots e'''$

$e'''^{\cdots\overline{p-1}}$ are all negative, then it will appear that $\frac{n-p}{n}\times$
e will be the greatest error that can be committed in de-
mining the value of any arc. by adding together the values
the (p) smaller arcs of which it is compounded. For
tance, if the interval betwixt the micrometer and the
croscope comprehends an arc of 60°, as marked upon
instrument, and this arc is measured against every suc-
ding arc of 60° in the whole circle, we shall have the
atest error that can be committed in deducing the arc
120° from the addition of the two first arcs of 60°,
ıal to $\frac{6-2}{6}\times 2\times 2e = 2{\cdot}66\,e$. After these remarks,
may proceed to consider ıe remaining divisions
on the circle may be exam vith the least probable
or, and to ascertain the am f the greatest to which
process can in any case l .
Let the arc of 30° be now red against every suc-
ding arc of 30° in the first, , fourth, and sixth arcs
60°, and let the length of be determined from a
arate comparison with the of 60°, in which it is
nprehended, and not from ral comparison with all
four. The arc of 15° m n be measured against
ry succeeding arc of 15° in st, third, fourth, sixth,
enth, ninth, tenth, and t arcs of 30°, and the
ue of each deduced from parison with the arc of
, in which it is contai hen this is done, we
ll have determined the len very succeeding arc of
, of the first arcs of 30, 45, 60, 75 (= 60 + 15), 90,
(= 90 + 15), 120 (= 60 + 60), 135 (= 90 + 45), 150
120 + 30), 165 (= 150 + 15), and 180° in each semi-
le.

We must next measure the arc of 5° against every suc-
ding arc of 5° in the whole circle, and deduce the values
the first, and of the sum of the first and second, in each
ceeding arc of 15°, from a comparison with the arc of
in which they are contained. We must then proceed
letermine the values of the first arc of 3° in each 15°,
of its multiples the arcs of 6, 9, and 12°. We must
put down the value of the last arc of 3° in each arc
15°, and then deduce the values of the first and last
of 1° in each arc of 15°, from a comparison with the
of 3° in which they are respectively contained.

We shall now have measured in each arc of 15° the first
of 1, 3, 5, 6, 9, 10, 12°, and by taking the last arc of
degree, which has likewise been determined, from the
arc

arc of 15°, we shall obtain the first arc of 14°. The first 7° of this arc being measured against the second, we ascertain the value of the first 7°; and then, by measuring the first 4° of the remaining arc of 8° against the second, we shall get the value of the first 4°, which added to the arc of 7°, before determined, will give us the length of the first arc of 11°. The first 2° of the remaining arc of 4° must then be measured against the second, and we shall get the value of the first 2°, and by adding this arc to the arc of 11°, we shall obtain the value of the arc of 13°. By taking away the first arc of 1° from the arc of 15°, we get the remaining arc of 14°; and then having determined the length of the first 7° of this arc, by measuring them against the second, we must add it to the arc of 1°, and we shall obtain the arc of 8°. The length of the first 4° of this arc will then be easily known, by measuring them against the second, as will afterwards that of the first 2° in the arc of 4° itself, by measuring them against the second in the same arc.

We have still to ascertain the lengths of all the first arcs of 10, 20, 30, 40, and 50 minutes contained in each degree, for I shall only consider the case in which the circle is divided into parts of 10 minutes. Now the length of the first arc of 30′ will be obtained by measuring it against the second, and the lengths of the first and second arcs of 20′ (whose sum will give the arc of 40′) by measuring the first against each of the remaining arcs. The length of the third arc of 20′ must likewise be put down, and then the first arc of 10′ being measured against the second of the arc of 20′, in which it is included, and also against the two arcs of 10′ contained in the last arc of 20′, its own value, and that of the last 10′ in the degree will be determined from a comparison with the arcs of 20′, in which they are respectively comprehended. The length of this last arc of 10′ being taken from that of the whole degree, will give us the length of the first 50′, and complete the operation.

In order to ascertain the greatest possible error to which we are liable in the examination, let ε denote in parts of a second the greatest that can be committed in bisecting any point upon the limb; then, since this error may occur at each end of the arc, it is evident that e in the expression deduced above $\left(\frac{n-p}{n} \times 2pe\right)$ will become 2ε, and the expression itself $\frac{n-p}{n} \times 4p\varepsilon$. Hence the possible error will be

$\frac{2-1}{2}\,4\varepsilon = 2\varepsilon$ at 180°; $\frac{2\varepsilon}{2} + \frac{2-1}{2} \times 4\varepsilon = 3\varepsilon$ at $+\frac{3-1}{3} \times 4\varepsilon = 3{\cdot}33\varepsilon$ at 60°; $\frac{2}{3} \times 2\varepsilon + \frac{3-2}{3} \times 4 \times$ $= 4\varepsilon$ at 120°. The greatest error must therefore lie :wixt 90 and 120°, and nearer to the extremity of the ter than of the former arc. At 105° it will be 5·50ε; 111° it will be $5{\cdot}50\varepsilon - \frac{2}{5}.1{,}5\varepsilon + \frac{5-2}{5} \times 4 \times 2\varepsilon =$ 'Oε; and at 111° 10′ it will be $9{,}70\varepsilon - \frac{1}{6}.1{,}04\varepsilon$ (the :ess of the error at 111° above that at 112°) $+ 3{\cdot}33\varepsilon =$ ·86ε, which will be found to be the greatest error betwixt 5 and 120°, and of course the reatest in the first semicle. In the other semi-circl , the process being the ne, the possible errors must necessarily be the same at : same distances from the first oint, reckoning the conry way upon the circle.

The magnitude of the quantity ε will of course vary upon cles of the same radius, according to the excellence of : glass employed, and the accuracy of the examiner's :. It will seldom, however, exceed one second upon a cle whose radius is one foot; and in general it will not iount to so much. I find that I can read off, to a certainty, thin less than three fourths of a second; and hence I nclude, that I could examine the divisions of my circle thout being liable to a grea error than 9·63 seconds, d those of a circle of three fe adius without the risk of greater error than 3·21 seconds.

To those people who are accustomed to entertain such alted notions of the accuracy with which astronomical struments can with a certainty be divided, this error, I re say, will appear very considerable; but for my part, am perfectly satisfied that it bears but a small prortion to the accumulated error which may take place, in ite of the utmost vigilance of the artist, in an instrument vided according to any method which has hitherto been ade public. I need not, however, remark upon the very eat improbability that the error of examination should er attain, or approach, to its extreme limit, as this must sufficiently obvious to any person who is in the least gree conversant with the doctrine of chances; but it may proper to observe, that we have it in our power (and in is respect the examiner possesses a most important adntage over the divider of an instrument) to diminish its obable amount, as much as we please, by bringing the oveable wires of the micrometer and microscope several times to bisect their respective points in the measurement of every arc, and taking a mean of the different *readings-off* for the true position of the wire at the real bisection of the point. The wire may be moved in this manner eight or ten times at each point (if such a degree of caution should be thought necessary) and the mean taken in little more than a minute; so that the time of performing the work will not be so much increased as might perhaps have been apprehended; and when it is completed, we may reasonably presume that the distance of every point from zero (whilst the temperature of the circle continues uniform) will have been determined with sufficient exactness for every practical purpose.

Of the time necessary for the examination, a pretty correct idea may be formed by considering how many measurements are required, and allowing about a minute and a half for each; *i. e.* a quarter of a minute for bringing the extreme points of the arc to the micrometer and the microscope, and a minute and a quarter for making the several bisections. Now, in dividing the whole circle into arcs of 15° each, it will appear that forty-four measurements must be performed; and to examine every point in each arc of 15°, there will be 161 required, making in all 3908 measurements; and consequently the time necessary for completing the whole work will be 5862 minutes, or about 98 hours.

The time and labour required for this examination are, no doubt, very considerable; but it ought to be recollected, that it will render any great degree of precision, in dividing the instrument, totally unnecessary. Whoever indeed employs this method of examination, will be virtually the divider of his own instrument; and all that he will ask of the artist, is to make him a point about the end of every five or ten minutes, whose distance from zero he will determine for himself, and enter in his book to be referred to when wanted. We may likewise observe, that by this examination we shall not only be secured against the errors of division, but against those which arise from bad centering, and from the imperfect figure of the circle, and which in general are of too great a magnitude to be neglected.

It will, I dare say, have occurred to you, that whenever we are desirous that an observation should be particularly exact, we may guard it against the effects of unequal expansion or contraction in the metal, by means of the apparatus which I have described: for we have only to measure the arc which has been determined by the observation against

gainst the whole circle, or against the multiple of it, which pproaches nearest to the circle, and from thence to deduce ts value in the manner explained above, and we shall either ave entirely excluded the error which we apprehended, or ave rendered it too small to be of any importance. Supose, for instance, that the arc determined by the observaion was 48°; then by measuring it against the whole cirumference increased by an arc of 24°, we shall obtain a esult free from any greater error of unequal temperature, han one eighth of the increase or decrease of this arc of 4° beyond a due proportion to that of the circle itself.

This expedient gives us all the advantages of the French ircle of repetition, without the inconvenience arising from eing obliged to turn the instrument, and move the telecope, so many times in the course of the observation. Nay, I am persuaded that the result may be made more ccurate in this way than by the French method, because ot only can the object be more frequently observed, but he contacts or bisections, it may be presumed, will be nore exact when the observer is not disturbed by the hurry ttendant upon the use of the repeating circle; and with espect to any error in the instrument, from whatever cause t may arise, it will be as effectually excluded by the process which I recommend, as by moving the telescope round the ircle. Besides, this method is applicable either to the zimuth or altitude circle, or indeed to any circle which urns upon its own axis; whereas the French method can ever be applied to the azimuth circle, nor to any other ircle which does not turn both upon its own axis and pon one which is perpendicular to it.

After all, however, it is possible that the process which have been explaining to you may be no new discovery, nd that you may be already acquainted with it. If this hould be the case, you will be kind enough to inform me. At any rate, indeed, I should esteem myself greatly obliged, f you would favour me with your sentiments upon the ubject, as soon as you can do it with perfect convenience o yourself.

I am, dear sir, yours, &c.

WILLIAM LAX.

XLVII. *On*

XLVII. *On Azimuthal Refraction.*

To Mr. Tilloch.

Sir, Having recently directed my attention to the subject of refraction in azimuth, and having mentioned the subject to Mr. Williams, of Wells (at present residing in Islington), I have received the following letter, stating phænomena observed by him on the 28th ult. depending on azimuthal as well as vertical refraction. Similar effects were observed on the same day in the north environs of London, by Messrs. Whites of Finsbury-square, and myself; but being at that time in the prosecution of other inquiries, we could not attend particularly to the subject. Mr. W. however, between one and three o'clock, observed three sets of angles (in which the churches of St. Paul, St. Mary Islington, Hampstead, and Stoke Newington formed principal objects), without having any reason to suspect error arising from azimuthal refraction, the fog by this time being more uniformly dispersed, forming a general haziness in the horizon, but not reaching so high as the tops of the above spires.

I am, sir,
Your obedient servant,
26, Garlick-Hill, March 10, 1810. Jos. Steevens.

Mr. Steevens,—Sir,

Although I stated to you at our last interview, that in a very great number of observations, (probably a hundred,) made on Harrow, Hampstead, and another church lying S.W. of Primrose Hill, distance about four miles, I could discover no azimuthal refraction as you term it; yet I have since, viz. on Fast-day, observed it twice in a very striking manner, and must do you the justice, before I quit town, to say I am quite satisfied as to the fact.

On the above day about nine A.M., in my way to Kilburn crossing some high fields to the south of Primrose Hill, I observed, from the interposition of the fog, that several objects put on a strange distorted appearance, and that the spire of Islington church appeared crooked (see *firist appearance*, Pl. VII); and in less than a minute, about 20 feet of the upper part appeared insulated and not immediately over the under part, but as in the margin. (See *second appearance.*)

I immediately screwed my telescope into the post of a fence which was near me; but before I could adjust it, the spire

re became indistinct, being now wholly enveloped in the
. I now directed the telescope to Hampstead (Harrow
ng invisible), the upper part of which was tolerably di-
ict, although there was a thick fog at the bottom of the
. As soon as I had a tolerably good intersection, I
tted hold of the telescope, and found it remarkably
dy, it being quite calm; I soon observed the tower to
rease in its height, and shortly after the upper part ap-
red separated from the lower; the lower part retiring a
e to the left, while the upper part was stationary in
nuth, but increased in altitude; in three or four minutes
top was considerably elevated, but still coincided with
vertical wire, and began to become indistinct as if
ng into a cloud; but before it was quite obscured, I
ught I discovered it to be a little to the left of the wire;
bottom part was now hazy, and still more to the left,
arently about $\frac{1}{6}$ of the whole diameter of the tower; in
ch situation it disappeared.

slington spire was now just visible: it appeared upright,
as far as I could judge by intervening objects was much
ated. The telescope remained unaltered until the fog
iciently cleared up for further observation on Hamp-
d, which was near three-quarters of an hour. I now
nd the top and bottom joined, and nearly in the same
ation where the bottom appeared just before its obscu-
on, viz. considerably to the left of the wire, where it
ained until the telescope was removed. It would thence
ear, that when I first made the intersection the tower
acquired its greatest elongation, and perhaps might
n then have been on its return.

learnt from a gentleman in the course of the day, that
end of a row of houses near Holloway appeared to him
much higher and afterwards much lower than the
er, although he did not change his situation:—and
n another, that the top of Primrose Hill, with some per-
s on it, appeared to him separated from the bottom and
ting in the air, and that he had seen a similar effect on
er hills several times before.

I am, sir,
Your obedient servant,
CHARLES WILLIAMS.

ner Street, Islington,
March 5, 1810.

XLVIII. *Second*

XLVIII. *Second Vindication of Dr. Herschel's Theory of Coloured Rings, in Answer to an anonymous Reviewer.*

To Mr. Tilloch.

Sir, The members of the club who formerly addressed you, have lately seen, in the twenty-first Number of the work called The Retrospect, some strictures on their vindication of Dr. Herschel's Essay on the Newtonian concentric coloured rings, which you honoured with a place in your Magazine for November last*.

Our main object in that communication, was to assert the validity of Dr. Herschel's important experiment described in the thirty-first article of his Essay. This experiment, the retrospectors, in their thirteenth number, attempted to set aside as nugatory, by affirming that the wedge of air described by Dr. Herchel was much too thick for exhibiting the coloured streaks which, according to the Newtonian doctrine of the fits of easy reflection and easy transmission of the rays, would have been seen in it, had it been sufficiently thin. In our former paper we showed, on the authority of sir Isaac Newton himself, that the retrospectors had asserted what was not just; and that Dr. Herschel's wedge, according to his measures minutely stated, was sufficiently thin for exhibiting the coloured streaks, if the Newtonian fits had a real existence; and that, according to this hypothesis, the author had a right to expect such coloured streaks; which failing, or not appearing, he had a right, as he contended for, to conclude that these fits are imaginary.

One proof concerning the competent thickness of his wedge being so complete, and so much held up to view by appearing in your excellent philosophical miscellany, the retrospectors have thought it necessary, as it would appear, to strike to it, by saying in number twenty-one, page 403, "We now come to the last and the most plausible objection that Dr. Herschel's friends have advanced to our remarks; and here we grant, that such a wedge as they have described ought to have produced the effects which Dr. Herschel expected from it." Now we must observe that the wedge of air we described was no other than the wedge described by Dr. Herschel: and what he expected from it, and what every body else must have expected, was this: namely, *coloured streaks*,—provided the Newtonian fits really existed, but *no* coloured streaks if such fits were imaginary. The experiment was most carefully made, as appears by the thirty-first article, and no symptoms whatever of such coloured streaks were perceivable. Hence the author considered it as an *experimentum crucis*, disproving the reality of the Newtonian fits.

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* Philosophical Magazine, vol. xxxiv. p. 359.

After this result of the experiment, and the admission of the retrospectors as above quoted, it might have been expected that they would no longer have resisted such evidence, or the just conclusion from it:—but no such thing; we now find them disputing the point as much as ever, by *new assertions* and contradictions, as totally groundless as that whose fallacy we have convicted them of. They immediately after the above quotation, now allege, that on account of corpuscular repulsions, &c., the wedge of air described by Dr. Herschel could not have been so thin as his measures, which before they did not challenge, show it to be; or so thin as to produce streaks according to the Newtonian doctrine of the fits: than which nothing can be more wide of the fact.

But, in another place, we find something prodigiously inconsistent with all this contention for extreme thinness of the wedge, before it is capable of showing streaks. In page 410, where the retrospectors want to set aside Dr. Herschel's explanation of the *bow-streaks* which he has shown to be occasioned by the application of a plain reflecting surface under the base of the prism, how do they do so? Still by resorting to the Newtonian doctrine of the fits. And now they would have these bow-streaks to be produced by the plate of air between the base of the prism and the reflecting surface applied to it, in consequence, they say, of the *great thickness* of this very plate, occasioned by their corpuscular repulsions. We shall say no more of such flagrant contradictions. In the same spirit, in the concluding paragraph, page 412, they deny that what Dr. Herschel calls the *critical separation* is capable of producing the bows; not perceiving that sir Isaac Newton has explained the formation of his blue bow on this very principle, as Dr. Herschel has explained his red bow.

In short, both attacks of the retrospectors abound with similar inconsistences and contradictions, which we think wholly undeserving of notice; as with such opponents we consider further discussions to be vain.

XLIX. On

XLIX. *Report made to the French Institute on a Memoir of* M. Delaroche *on the Air-bladder of Fishes.* By G. Cuvier.

The mathematical and physical class instructed Messrs. Lacepede, Vauquelin, and myself, to render an account of a memoir by M. Francis Delaroche on the air-bladder of fishes.

As several naturalists have been of late employed in directing their attention to the organ which is the object of this memoir, and to its functions, we do not think it will be improper to preface our report by a historical view of what has been said on the subject; a recapitulation for which M. Delaroche himself has furnished us with ample materials.

The air-vessel of fishes is too remarkable, it strikes the eye too forcibly on the first opening of the animal, and differs too much from every other organ, not to awaken the attention of naturalists; but, like most objects in comparative anatomy, it has long produced more conjectures and hypotheses than exact observations and experimental researches.

Rondelet * confined himself to the observation, that it existed more constantly in fresh than in salt-water fishes, and that it probably serves to assist them in swimming.

Marcus Aurelius Severinus risks an opinion that the air of this vessel was produced along with the animal; which proves that he had never perceived any communication with it outwards.

Gauthier Needham (in 1668) was the first who entered into more detailed inquiries, and inserted them in a book, where no one would expect to find them; namely, *De formato fœtu* †. Adopting the general idea of the utility of this bladder for swimming, he explained how flat fish are enabled to do without it; he described the two tunics of this organ, as well as the varieties of its form, and the origin of the canal of communication. He shows that the vessels are more abundant than are requisite for its own nutrition; that it is probable that some organic function is exercised by them, and that the blood contained in them has some connexion with the air: but judging that it would be difficult for the air to penetrate into it from without, in certain fishes, through substances which fill the

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* *Hist. Pisc.* 1554, pp. 26 and 73.

† *Biblioth. de Manget.* ii. pp. 713 and 714.

:omach, he conjectured that this fluid is secreted there,
nd that it proceeds from thence into the stomach, where
. assists in the process of digestion: he even points out
ıe red bodies which operate this secretion in the snake.

Borelli explained in detail, in 1676, the method in which
ıe bladder is used in swimming. He observed that fishes,
'hose air-bladders burst, remain at the bottom of the wa-
:r, as well as most of those which are naturally deprived
f it; and concludes that it is intended to render the body
f the fish sufficiently light to be in equilibrium with the
'ater: he added, that by compressing the bladder, or by
ɔandoning the air which it contains to its elasticity, the
sh can augment or dimi ıtal specific gravity, and
ısist it in its ascent or d He supposed, that the
ınal which establishes in fishes a communication
:tween the air-bladder an stomach, must be a me-
ıod of varying or renewi ıantity of air [*].

To conclude: he has ne ribed the varieties of the
ructure of the bladder, rmined in what fishes it
ıists, and those in which i ıting.

Redi resumes the observ Needham. He added
ıme details on those fishe have no air-bladder, and
ı the red bodies in the inte several of these organs.
e also stated, that he had sought for the canal of
ımmunication in certain s; but he thought that
was his fault, and this o f the generality of the
istence of the canal has gned to the present time
nong some others. The ks of Redi are still to be
und in a book entitled, *Obser ons sur les Animaux vi-
ns contenus dans les Animaux vivans.* Florence, 1684.

Ray and Willoughby, without making fresh inquiries,
ıd without deciding on the manner in which the air is
troduced into the bladder, disputed the idea of this air
ing used in digestion, and reduced the bladder to its em-
oyment in swimming, according to the ideas of Borelli.
ıey insisted on the muscles peculiar to certain vessels,
ıd mistook for them the red bodies in the interior of some
hers [†].

The same opinion on the use of the bladder was sup-
ırted by Preston [‡], by Perrault [§], and by Petit [‖]. Per-
ult made the important observation, that there are fishes

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[*] *De Mot. Animal.* cap. 23. *De Natatu.*

Willoughby, *Hist. Pisc.* 1686, pp. 12 et seq.

Phil. Trans. xix. p. 499.

Mécanique des Animaux, part II. ch. iii. vol ii. p. 383 of his works, 1721.

Mem. de l'Acad. 1733.

without any canal, and that it is in the latter that the red bodies are found, which are intended for the separation of the air. He added, that in those which have a canal, the air does not issue from the bladder, although it be compressed; a remark too much generalized.

Petit, on the contrary, thought he had discovered in the canal of the carp, valvuli which admit of the air escaping, but not of returning.

Notwithstanding the observation of Perrault, Artedi still ascribed to all bladders a canal destined, according to him, for the introduction of air: but, with the exception of Borelli's [*], there is no opinion given respecting their use.

It is the same case with Gouan, Bloch, and a variety of other authors, who add nothing in other respects to the details previously acquired.

But, admitting in its fullest extent this chief employment of the air-bladder, we might still suppose it to have accessory uses, and in particular we must defer giving any opinion as to the origin of the air which it contains.

This was the conduct pursued by Vicq d'Azyr in 1773 [‡]. He imagined that the air originated in the stomach, from whence it entered, charged with nutritive particles, into the air-bladder, in order to be absorbed by the vascular system. He was followed by Broussonnet in this idea [§] under some modifications.

Erxleben entertained the same idea respecting the propagation of the air; but as to its uses he followed the common opinion [‖].

These three anatomists seem to have been ignorant that the communication between the stomach and the air-bladder is frequently wanting.

This is strongly insisted upon by Kœhlreuter [¶] in an anatomical description of the lotus. After having ascertained the defect in the canal, and that a number of other fishes are also without it, and after having described the organization of the red bodies, he maintained that the air is separated from the blood in the bladder. He thought his system was new, not having read the writings of Perrault and Needham.

Leske adopted the opinion of Kœhlreuter [**].

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[*] *Partes Pisc.* 1738, p. 36.

[†] *Hist. des Poissons*, 1770, p. 81.

[‡] *Mémoires presentés*, tome vii.; and his Physiological Works, tome ii. p. 203.

[§] *Var. Posit. circ. Respir.* sect. v.

[‖] 1776, in a memoir on the subject; and also in his Natural History, edit. 1797, p. 279.

[¶] *Nov. Comm. Petropolit.* tome xix. 1775.

[**] *Hist. Nat.* 1724, p. 390.

Monro, who in his work on fishes ought to have thrown a great deal of light on this subject, has added but little to what was known before on this subject. He made the same distinction with Perrault between bladders with secretory red bodies which have no canal, and those which have a canal and want these bodies; but he does not mention any French anatomist; perhaps because he had never read any of their works on the subject.

He remarked that the genus anguilla formed an exception to the rule, from having the canal and red bodies. With respect to the other parts of the question, he did not decide upon the use of the bladder; and merely inquired, f fishes could not, in swallowing, distinguish the bubbles of air from the mass of water, and make them pass in preference into this organ.

M. Fischer, now professor at Moscow, published in 1795, at Leipsic, a particnlar dissertation on this subject; in which after having given an extract of the writings of his predecessors, and having communicated his own observations on the carp and the tench, he hazarded the opinion, that he air-bladder, independent of its uses for motion, is also a supplementary organ of respiration, destined to absorb the oxygen from the atmospheric air contained in waer, as the gills are destined, according to him, to absorb the oxygen of the water itself, by decomposing it.

M. de Lacepede supposes, that certain fishes may at east fill their bladder with the gases resulting from the decompositions which their respiration occasions. He thought that it was frequently hydrogen with which it was filled, and he mentioned tenches in which he had collected precisely this kind of gas.

Finally, M. Duvernoy, editor of that part of Cuvier's comparative anatomy which has for its object the air-bladder of fishes, adopted, in common with M. Cuvier, he opinion of Needham and Kœhlreuter, that the air is produced in the bladder by secretion. He also described some of the organs of this secretion in fishes not before observed; but, from too much precipitation, he forgot to advance the principal argument, founded on the absence of all canal of communication in many species. He concludes, from the absence of the vessel itself in fishes belonging indiscriminately to all descriptions of families, and even to genera the other species of which are furnished with it, that its functions cannot be very essential to life. By comparing its proportional volume with the nature of the movements of every fish, and by examining the supplementary means granted to those who have it not, and the various effects of those means, he arrives at the conclusion that it is essentially an organ connected with loco-motion.

He expresses his astonishment at the discordance between the analysis hitherto given of the air contained in this bladder; some, like M. Fourcroy, having found hardly any thing but azote; others, like M. Configliati, having found so much as 40·0 of oxygen; while others, like Mr. Broadbelt, found the quantity variable in the same kind of fish according to circumstances. M. Duvernoy concludes with suggesting that chemists should inquire into the causes and limits of these variations; a precise knowledge of which could alone decide a great number of the questions in dispute.

Messrs. Geoffroy and Vauquelin on one hand, and M. Biot on the other, have recently made a great part of the experiments which were pointed out as requisite by M. Duvernoy.

M. Biot, in his first voyage to Ivica, examined the air in the bladder of several fishes of the Mediterranean, and found that it varied from pure azote up to 87·0 of oxygen, with very little carbonic acid, and without any hydrogen, and that in general the oxygen is the more abundant, in comparison to the azote, as the fish comes from a greater depth, although the water at these great depths does not contain a purer air than that which is at the surface.

He also made the curious observation, that in fishes suddenly drawn from a great depth, the air-bladder ceasing to be compressed by the enormous column of water which bore upon it, is dilated so suddenly that it tears the intestines, and is ejected from the mouth. As to the origin of the air contained in it, he seems to think it has been secreted.

The experiments of Messrs. Vauquelin and Geoffroy, published by M. Biot, confirm his own on the subject, so far as the fishes on which they were made, living in our fresh waters and at very small depths, gave but very little oxygen. They agree also with other more ancient experiments of M. Fourcroy, who had found nothing in the bladder of the carp but azote almost pure, and with the analysis made by M. Humboldt of the air in the bladder of the gymnotus electricus, which consisted of 96·0 of azote and 40·0 of oxygen.

Such was the whole of our knowledge of the air-bladder of fishes when M. Delaroche read his memoir to the Institute. But in order to complete the series of facts which are

ecessary to guide us in forming an opinion of his theory,
e think it right to say a few words upon two memoirs pub-
shed since.

One of these, by M. Geoffroy, refers to an earlier memoir,
 which he develops, anatomically, the means by which
e fish compresses or relaxes its bladder, in order to descend
r ascend. Indeed, he says at the same time, in the intro-
uction of his memoir, that the bladder is by no means an
rgan of motion by itself; but this is because he thought
at those who regarded it as such, suppose that it is dilated
y the increase of the air which it contains, and *vice versâ*,
n opinion which no person seems to have entertained; for it
 always by the action of the muscles that it has been made
 be compressed or dilated: on this subject, therefore,
I. Geoffroy is really of the opinion of Borelli, which is
e commonly received idea.

The other memoir to which we have alluded, is by Messrs.
Iumboldt and Provençal, and has for its chief object the
espiration of fishes; but these authors have naturally been
d to examine the air in the swimming-bladder.

They operated upon river fishes, and found the air vari-
ble in composition from 99·0 of azote to 87·0. They have
bserved as much as 5·0 of carbonic acid. They made
ome tench respire hydrogen, and yet their air-bladders
hen examined exhibited none: by keeping them in oxygen,
owever, the proportion of the oxygen in the bladder was
omewhat increased. On removing the bladder from them,
ey were not prevented from producing by their respiration
e ordinary effects upon the atmosphere; they were even
ble to raise themselves in the water, although they gene-
lly remained at the bottom of the vessel.

Thus, in the numerous works we have analysed, almost
very possible hypothesis has been proposed, attacked, or
efended, and examples have been given of almost all
e combinations of organization that could be devised.
I. Delaroche had only therefore to examine these organiza-
ons a little further, in order to reduce them to general
ules, and to weigh over again the arguments advanced for
r against every hypothesis.

Let us see how he has acquitted himself of this task.—

His residence at Ivica, Formentero, and on the coast of
pain, with Messrs. Biot and Arrago, having furnished
im with opportunities of examining a great number of
Iediterranean fishes not to be seen any where else, and
eir air-bladders having chiefly occupied his attention, he
ontinued his inquiries after his return, on our common
fresh-

fresh- and salt-water fishes: hence he has furnished upwards of fifty particular descriptions of the air-bladders of as many species of fish, several of which have not hitherto been described. These descriptions, added to those which former authors had given of some species which M. Delaroche could not find, form the materials of his present memoir; and he has placed his own at the end of the work, as so many proofs of the general propositions which he lays down.

In the body of the memoir he treats successively of the anatomical structure of the air-bladder, of the nature of the sources of the air which it contains, and of the functions which it exercises.

He speaks in the first place of its existence, and gives a list of those fishes which have it, and of those in which it is wanting. The results of this list, which adds several species to those which had already been adduced with respect to this subject, are nearly the same which had been already drawn; namely, that the existence or non-existence of the bladder does not correspond with the other affinities of organization which connect fishes with each other.

He afterwards speaks of the various situations of the bladder, of its variation in size, and in the configuration of its tunics, (an article in which he compares the internal membrane to the serous membranes); and finally, of the particular muscles which it has in several fishes; and he gives a more detailed description of these muscles than is to be found in the comparative anatomy of M. Cuvier.

What he says on the subject of the canal of communication also presents a great number of novelties. On this head he has made some very acute remarks, and has ascertained that this canal is wanting in the greater part of sea fishes. He did not find it in any of the jugular or thoracic classes, which compose nearly three-fourths of the total species of fishes with which we are acquainted. The lectures on comparative anatomy had assigned this canal to the *uranoscope*, which is a jugular; but according to M. Delaroche, the authors of this work have made new inquiries, and found that they were mistaken.

M. Delaroche has studied in a particular manner the red bodies with which certain bladders are furnished. He found them, like Perrault and Monro, in all those which want the canal of communication, and in the anguilla genus although furnished with this canal.

Our author gives a very detailed description of these bodies, in the *gadi*, the *trigli*, the perches, some *labri* and *holocentres*, as well as in the *atherina rhepsetus*, the *blennius physis*,

hysis, the *orphius* or *esox belonus*, and lastly in the eel and
ie conger.

We have verified that part of the descriptions which re-
rs to the species with which we are familiar, or could pro-
ure, and have found them generally correct.

It appears to us, however, that M. Delaroche grants too
reat a homogeneity to the inner texture of these bodies.
)ne of our number, who, along with M. Duvernoy, re-
ently made some inquiries in order to verify this point of
natomy, found these bodies in the larger fishes formed of
bes flattened like ribands, placed almost parallel on each
ther, very distinct from one another by clearly marked in-
rvals, and proceeding obliquely in various directions from
ie proper membrane to the internal membrane of the
adder.

The distribution given by M. Delaroche of the vessels
hich issue from the red bodies of the eel, and from those
hich return to it, has also been verified, and found cor-
ct; but he passes rather too hastily over the red body it-
lf, which is also divided into flakes, separated by intervals,
hich are frequently found filled with blood.

In short, Messrs. Cuvier and Duvernoy think they have
und strongly marked relations in the red bodies of fishes
ith the cavernous bodies; but their inquiries posterior to
ie memoir of M. Delaroche, are only brought forward
ere that the Institute may not be ignorant of what has
een done on this interesting subject. A full account of
ieir experiments will shortly appear.

The author of the present Memoir speaks only from the
ctures in comparative anatomy as to certain branching
ir-bladders, entirely peculiar to one species of fish.
I. Cuvier, who had described them when on the sea coast,
here he had no books from which to determine the spe-
ies of the fish, thought it was the *perca labrax*; but other
aturalists, besides himself, have since sought for it in
ain in the fish so called in the systems of ichthyology.
y unexpected good fortune, the true fish which was the
ubject of observation was brought to Paris some time
go, and proved to belong to the rare species denominated
y M. Lacepede *cheilodiptera*, or sea eaglet, but which ought
be placed among the centropommes, beside the *labrax*.

The bladder of this fish, unique of its kind, will be pre-
nted to the Institute along with a description by M. Du-
ernoy, and which will be more minute than any hitherto
iven under less advantageous circumstances.

In his analysis of the air contained in the bladder, M.
Delaroche

Delaroche confirms in general the experiments of M. Biot; adding, that besides the various degrees of depth at which fishes live, there are other causes which concur to vary the proportions of the gases in their air-bladders. Thus, of two fishes caught on the same spot, one has given 50·0 and the other scarcely 40 of oxygen. M. Delaroche also rectifies the idea that M. Biot had given of the eruption of the bladder from the mouth, in fishes drawn up suddenly from great depths, when he says that a rupture of the bladder then takes place, and that it is the air which forces up the stomach to the mouth. As to the source of this air, our author (like Needham, Perrault, Monro, Kœhlreuter, Duvernoy and Cuvier,) thinks it is produced in the interior of the bladder by a secretion of an unknown nature, of which the red bodies seem to be the organs in such fishes as have these bodies.

It is unnecessary to ask for a proof of this opinion in fishes which have no exterior canal, for in them it is demonstrated by itself. We might also fairly extend it to those which have a canal and red bodies, like the eel.

But in those which want the red bodies, as we must admit a new kind of exhalation, the analogy no longer takes place completely; and perhaps many persons would be equally willing to have recourse to the aërial canal, in so much as it always exists in this description of fishes. As fishes of the same family frequently have the air-bladder, and others want it, it is probable that its functions may be supplied by different means.

M. Delaroche, without considering that question as at all decided, nevertheless supports the argument of analogy, from the difficulty which any given gas would have in many species, in penetrating into the bladder by the canal; from the still greater difficulty which it would have of arriving pure, particularly when it was requisite for it to pass through the substances contained in the stomach; and, lastly, from the difficulty of knowing, from whence, or by what mechanism, the fish could procure it from nature, in order to introduce it into its bladder at great depths, where it is so frequently and so long retained.

The habit in which physiologists are of seeing matters of every kind come out of the blood by secretions, renders them on the contrary very easy as to this kind of production; and in fact there is no real difficulty on the subject, since azote and oxygen, which compose the air in the bladder, exist abundantly in the blood.

But it may be asked; If the gas be exhaled or separated from the blood, wherefore does it vary so much when the greater part of the other secretions are so constant in their nature? Above all, how can the animal body, so greedy of oxygen in general, exhale it so precisely at depths where it has the fewest methods of getting it from the external medium? M. Delaroche, who puts these questions, admits that it is difficult to answer them satisfactorily.

He afterwards proceeds to the uses of the air-bladder.

From its absence in many fishes taken indiscriminately from all classes, he concludes, with the authors of the comparative anatomy, that it cannot hold an important place n the vital functions; and this makes him reject all necessary connexion between the air-bladder and respiration.

He would have even been inclined to conclude, from its solute stoppage in the greater number of fishes that are furnished with it, that it could not in general be employed in he absorption of any useful matter, in the excretion of ny injurious substance, nor even in the production of a substance to be employed in some other part of the body; but that it is solely by itself as the air-vessel, and in its quality of considerable capacity, filled with a light elastic substance that it may be useful to the fish.

Now in this respect it can only have a mechanical use, ither with respect to its station or movement.

M. Delaroche in the first place ascertains its use in the station, and admits that it serves to render the whole fish specifically lighter, and to place it in equilibrium with the vater in which it is suspended.

This is one part of the most generally received opinion; out it is clear that the necessity of the bladder for this sole urpose is any thing but demonstrated. Nature would ather have made all fishes of the same gravity as the water, s she has done with those fishes that have no bladders: hus, the common opinion is also composed of two other ntegrant parts equally necessary with the former. The ne is, that the fish can compress as it pleases, to a certain xtent, its bladder, or dilate it; which we prove by the peculiar muscles with which the bladder is furnished in cerain fishes, and by the mediate action which the sides and he muscles of the abdomen exercise on it in all those vhich have it.

M. Delaroche also adopts this second part of the common pinion.

He thinks even that it is in this way the fish supplies, vhen it rises, the pressure exercised on its bladder in deep vater by the column of water above it. Were it otherwise, the air, which would be no longer compressed, would be too much dilated, and would render the fish too light, or even produce some rupture, as happens to fishes drawn suddenly from great depths.

But who is there who is not aware, that this, on the part of nature, would be correcting very clumsily a defect which she might have refrained from introducing at all into her work? She had only to give no air bladder at all to fishes; and we have seen that she need not have done so to place them in equilibrium with the water: in that case she would no longer have required the apparatus of compression, which has been supposed as serving only to correct the inconvenience of an useless bladder.

Thus we are of opinion that the third, and the chief part of the commonly received opinion, in reality resolves the problem: we mean that part of it which says, that the bladder is placed there to assist the fish in ascending and descending, according as it is compressed and dilated; and we confess that we do not see why M. Delaroche should reject this use of the bladder, to which the two others are, in our opinion, merely accessaries.

That the fish has strength sufficient to enable it to descend, clearly results from what M. Delaroche himself admits; for if the fish, which ascends 30 feet for example (and it is difficult not to believe that many fishes can ascend that height without any accident), if, we say, such a fish has sufficient strength to compress its bladder, by means of its muscles, to the same degree that the 30 feet of water formerly did, it is evident that a similar fish, supposed to be in equilibrium at the height to which the former ascended, will also have sufficient strength to compress its bladder, as much as would the addition of a weight of 30 feet, and that there would result from such a compression or diminution of volume more than sufficient to force it to descend.

M. Delaroche, against this most essential part of the vulgar opinion, advances only a single objection, which he borrows from M. Fischer: this is, that the variation of specific gravity which may result, with respect to the total body of fishes, from the variations of the volume of the bladder being very small, the ascents or descents, which are the consequence of it, could not but be very slow: but, besides, the circumstance of these variations never having been yet measured, no person has ever said that the bladder cannot be aided in this function by other organs. Those fishes which have no bladder, ascend and descend,

escend very well, although, other circumstances considered, atherm ore tardily. Now those which have a bladder have, n addition, all the organs employed by those which have one, and they can use them like the others.

One difficulty which we have sometimes heard started, is o ask how a fish, when it wishes to ascend from the bottom f the sea, can find strength to raise up by means of its sides, r generally of its integuments, the enormous column of vater which lies upon it, in order to permit its bladder to ilate. But as this vessel is already, by its compression, in quilibrium with the water which presses it, the least effort s sufficient; and even this effort, however small, is neverheless necessary, that the fish may only rise a few feet by he means which are common to it with the fishes without ladder: instantly its bladder, being less compressed, will be oo much dilated; and, according to M. Delaroche's own xperiments, it will carry it precipitately upwards, and burst ts entrails if it does not speedily close it. This second bjection is refuted therefore like the former.

Thus we think we ought to adhere to the ideas of Borelli, s to the use of the air-vessel of fishes; but although we liffer in opinion from M. Delaroche on this point, we do ot the less regard his Memoir as worthy of approbation, rom the great number of new and correct observations vhich it contains, as to the anatomical structure of the ladder, as well as upon the nature and sources of the air vhich it contains; and we have the honour to recommend hat it should be printed among the Memoirs of *Savans Etrangers*. Signed,

LACEPEDE, VAUQUELIN, CUVIER.

L. *Notices respecting New Books.*

MR. ACCUM has just published a "*Manual of a Course of Lectures on Experimental Chemistry and on Mineralogy; containing, in the Form of Axioms and concise Outlines, the Elements and fundamental Truths of chemical and mineralogical Science: intended to illustrate the Lectures on these Branches of Knowledge in the Theatre of the Surry Institution; with an Account of the Action of chemical Tests and Modes of applying them in the Practice of the Science.*"

THE very full title of this little work (about 230 pages small 12mo.) sufficiently explains its object. The author states that in writing it he has endeavoured to exhibit, to the learner, those "important facts and fundamental doctrines which constitute the theory of the science. The laws of chemical action, which befall all bodies comprehended under the chemical domain of nature, have been detailed in the synthetic form of propositions—the natural history of the most characteristic bodies stated—the processes employed in the laboratory for obtaining them, described—and their physical and chemical attributes pointed out, so as to interest the mind, and fix the doctrines in the memory."

LI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

MARCH 28.—The President in the chair. A letter from Mr. Groombridge to the Astronomer Royal was read on the refraction of light. The author determines the mean astronomical refractions from the observations of fifty circumpolar stars; as also from the zenith distance of the sun at the solstices. The quantity he assumes at 45° is $56\frac{1}{2}''$; and this is corrected by the difference of the errors of the co-latitude thence found. The mean refraction at 45° the author states to be $58{\cdot}107'' \times$ tang. zenith distance, $-$ 3 times the refraction. He then corrects the formula of Bradley, by comparing the mean refraction of the pole star with stars at low altitudes; whence he determines the refraction from the zenith to the horizon to be $58{\cdot}119'' \times \text{tang}\ \overline{z - 3{\cdot}36\, r}$, and which he shows to agree with the new French tables, by a more simple formula than that of M. de la Place. The author also proposes a correction for the thermometer, drawn from his observations; something differing from that of Dr. Bradley.

April 4.—A letter from Mr. Brinckley of Dublin to the Astronomer Royal was read, stating the discovery of the parallax of the annual orbit of 13 circumpolar stars, which he has found to be $2\frac{1}{2}$ seconds: he also ascertained their refraction somewhat similar to that of Mr. Groombridge. The letter concluded by saying, that the writer is pursuing his researches in order to verify the discovery here announced.

Some observations on the gizzards of swans, geese, and herbivorous fowls, compared with those of turkeys, were communicated by Mr. Home. They consisted of a brief description of the process of mastication and digestion in ruminating animals, by chewing their food slightly, then swallowing it afterwards, bringing it up in round balls, mi-

xing them with saliva, and finally passing them to the
urth stomach, there to be digested. The process of di-
stion in geese and swans is extremely slow, as it is ef-
cted by the action solely of the muscles of their stomachs.
r. Home examined the structure of their stomachs by
ing them with plaister of Paris, and boiling them; when
ey appeared composed of straight muscles united by fila-
nts, as observed by Spallanzani.

April 11.—A part of a curious paper by Mr. Macartney
luminous animals was read. Mr. M. took a brief review
the different creatures in the animal kingdom which
it light, whether in or out of the sea, as the lampyris,
gora, &c. He examined minutely the assertions of some
ench naturalists, who maintain that common earth worms
ve occasionally appeared luminous, and proved that they
unfounded. The luminous appearances in the sea, which
ve been so often noticed, and yet so imperfectly explained,
re next discussed; and also how far it is probable that
emission of light depends on the will of the insect; and
ether nature has given this faculty to females in order
attract the males to them, as has been alleged.

The reading of the remainder of this paper was postponed
next meeting, and the society adjourned till the 3d of
ay.

SOCIETY OF ANTIQUARIES.

Monday, April 23, being St. George's Day, the Society of
tiquaries met at their apartments in Somerset-place, in
rsuance of their statutes and charter of incorporation,
elect a president, council, and officers, of the society for
year ensuing: whereupon

e most noble George, marquis of Townshend and earl
of Leicester,

F. A. Barnard, esq.
W. Bray, esq.
Nich. Carlisle, esq.
Sir H. C. Englefield, bart.
Anthony Hamilton, D. D.
William Hamilton, esq.
Samuel Lysons, esq.
Craven Ord, esq.
Matthew Raper, esq.
Joseph Windham, esq.

even of the council were re-chosen of the new council:
and

arles Baratty, esq.
ght Hon. P. R. Carew,
illiam, lord bishop of Cloyne,
nry Ellis, esq.
nry viscount Harberton,
Edward Jerningham, esq.
Charles duke of Norfolk,
John Towneley, esq.
Brig.-gen. T. H. Turner,
Rev. Stephen Weston,

Ten of the other members of the society, were chosen of the new council, and they were severally declared to be the council for the year ensuing; and on a report made of the officers of the society, it appeared that the most noble George marquis of Townshend and earl of Leicester was elected president, William Bray, esq., treasurer, Matthew Raper, esq, director, rev. T. W. Wrighte, A. M. secretary, and Nicholas Carlisle, esq. secretary for the year ensuing.

The society afterwards dined together at the Crown and Anchor tavern in the Strand, according to annual custom.

SOCIETY FOR THE ENCOURAGEMENT OF ARTS, &c.

At a late meeting of this society a premium of fifty guineas was awarded to Mr. John Davis of John Street, Spital Fields, for a most ingenious fire-escape, which promises to be of singular use in lessening the number of personal accidents which occur so frequently in this great city in cases of fire. This contrivance consists of a most curious yet simply constructed ladder, or rather three ladders so combined as to admit of their being slid out, like the tubes of a pocket telescope, to the height of from forty to fifty feet if required; carrying up at the same time a box to receive females, or children, or small valuables, (while the less timid can descend by the ladder). This box, by means of a chain and pulley worked by the people below, descends to the ground; where being instantly unhooked, another box is sent up while the first is emptying. All this is performed in about two minutes. This apparatus is erected on a carriage with four wheels, 9 feet long and 5 feet wide, furnished with the usual apparatus and harness for yoking a horse to it, for the more speedy removal to the scene of danger.

The fire offices, much to their credit, assisted the inventor with money to construct a machine of this kind for real use, after having inspected his model. We have examined the machine, witnessed the facility with which it performs the destined operation, and we have no hesitation in recommending to every parish in London to provide one of them to be kept with their fire ladders and engines.

This new fire-escape may be seen at Mr. John Bevan's, carpenter, City Road, near Finsbury Square.

WERNERIAN NATURAL HISTORY SOCIETY.

At the meeting of this society on the 10th March, the rev. Dr. Macknight read a paper on the mineralogy of

Strontian and Ben Nevis. The rocks which compose the districts of Strontian are mica slate, gneiss, and granite; and the lead-glance, which occurs in gneiss, is associated with iron pyrites, cross-stone, calc-spar, foliated zeolite, strontian, and heavy spar. Ben Nevis is an overlying massive formation, which rests on gneiss and mica-slate, approaching in some places to clay-slate. In this formation compact feldspar is the leading ingredient. The inferior mass consists of sienite, passing from the simple-granular to the granular porphyritic; and the upper portion of the mountain, comprehending the summit, with about 1400 feet of the perpendicular height below it, is composed of a dark-coloured rock, whicl he most part, is porphyritic, and seems to be inti allied in its characters to compact feldspar. This a om the gradual transition of the one substance into .. er, which is distinctly observed under the tremend ipice of Ben Nevis to the NE. and demonstrates t.. ity and continuity of the whole formation. The ig matter appears to be hornblende intimately n ith the substance of the rock. At first view, the iass might be considered as a formation of clinks d porphyry-slate. But a more minute investigati vers many oryctognostic characters of distinction fr se substances, which are less crystalline, and belon nore recent æra of formation.

At the same meeting, hur Edmonstone read an account of the peculiarit.. e Zetland sheep; with remarks on their diseases. e secretary read a communication from lieutenant-colonel Imrie, describing a vein of greenstone, which occurs in Glencoe, and which appears to have been overlooked in the mineralogical descriptions of that district.

FRENCH NATIONAL INSTITUTE.

Analysis of the Labours of the Class of Mathematical and Physical Sciences of the French Institute, for the Year 1809[*].

All the sciences which are founded upon facts have this distinguished advantage, namely, that every experiment and every observation contribute to their progress. Indeed, properly speaking, there are no discoveries made in vain, so far as the physical sciences are concerned. Whatever may be the consequences to which they lead, whatever are the results obtained, provided they be just, the instant they assume a character of novelty, they become useful: every fact has a determinate place, which can be held by itself alone, and we ought to consider the edifice of the sciences as that of nature: every thing is infinite, every thing necessary. We may go further: in short, the progress of truth is not essentially retarded because those who devote their talents to philosophical subjects occasionally fall into erroneous roads. The most useful discoveries have sprung from the greatest errors. We find the proof of this in the labours which have been undertaken to overturn modern chemistry, and to support the old theory of combustion. The complication of the phænomena in this latter science will even be the cause that the proofs of this description will still continue to be multiplied: facts do not always present themselves under the same characters; they are studied under other points of view, they are seen with different eyes, and the results to which they lead are not similar. This is precisely the case at present with respect to the discussions which have arisen between Mr. Davy and associates Messrs. Gay Lussac and Thenard.

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[*] Translated from the original, distributed at the public sitting of the lass, 2d Jan. 1810.

CHEMISTRY.

In former reports we have given an account of the discovery of Mr. Davy, as to the changes that potash and soda undergo by the action of the Voltaic-pile, and of the processes by which Messrs. Gay Lussac and Thenard produced these changes without the help of the above instrument.

Mr. Davy thought that in these experiments the potash and soda were subjected to a deoxygenation, and that a true metal resulted from it, which was particularly distinguished from other substances of this kind by an extreme affinity for oxygen. He called these two metals *potassium* and *sodium*. Messrs. Gay Lussac and Thenard, established on the contrary, by several experiments, but particularly by the products obtained on analysing the combination of the potassium with ammonia, were of opinion that the changes of potash and of soda were owing to a particular combination of these alkalis with hydrogen. Mr. Davy, having repeated the experiments on which this opinion is founded, has not obtained results conformable to those which had been announced by the French chemists; this has given rise to some observations by Messrs. Gay Lussac and Thenard, in which they show that the differences found between the results of Mr. Davy's experiments and their own, appertain to causes which cannot influence the consequences to which they have led.

To conclude:—On either hypothesis, the discovery of Mr. Davy has produced an extremely active re-agent in chemistry, and which must produce, on other bodies, effects hitherto unknown.

This new discovery gave rise, therefore, to very different experiments, but which led to the same end: some had for their objects to ascertain the action of the pile on the other alkalis, on the earths, and generally on all the simple non-metallic substances which we might suppose to be oxides, like potash and soda. The object of the others was to decompose, by me he new metals, substances oxygenized, or supposed such, and particularly the boracic, fluoric, and muri ls.

We informed the pu year, that Messrs. Gay Lussac and Thenard had led in effecting the decomposition of the first cids, and had ascertained its radical. Since that t ir inquiries have been directed to the fluoric acid.

They began by studying ysical and chemical properties of this acid more y than had been done by any one else. The affin er for this gas is extreme: as soon as it is mixed w gases which contain some portions of this liquid, a e of vapours is formed: nevertheless, this gas ca mmunicate its expansive force to water; it can lissolved nor *gazify* the smallest quantity, and in form state it is absolutely dry: but it is impossible to obtain this acid pure; it always retains some portions of those bodies with which it has been in contact; and in the labours which Messrs. Gay Lussac and Thenard have undertaken on the subject of this acid by means of potassium they made use of siliceous fluoric gas in preference, as not containing any foreign body susceptible of being decomposed and obscuring the results of the experiments. In the reciprocal action of these two substances, there is a great absorption of fluoric acid, very little hydrogen gas extricated, and a transformation of the metal into a solid matter the colour of which is reddish brown.

Messrs. Gay Lussac and Thenard regard this new combination as a compound of potash, of silex, and of the radical of the fluoric acid; but they have not been able to obtain this last substance in a separate state. "It appears," say they, (after several experiments which we cannot give here,) "that when this radical is combined with only, it may decompose water like the phosphures; but when it is combined with potash and silex, it does not decompose it: doubtless, because this triple combination is insoluble."

Mr. Davy has also made attempts to obtain in a free state the fluoric radical, and has obtained results analogous to those above referred to: he ascribes the hydrogen produced in the combination of the potassium with the gas, to the water which he thought was contained in this acid, and which the metal had decomposed.

The muriatic acid has also been the subject of numerous and interesting experiments by Mr. Davy and Messrs. Gay Lussac and Thenard. All three have made some fruitless attempts to decompose this acid, and to insulate the radical which has been considered as forming one of its elements. But Messrs. Gay Lussac and Thenard have ascertained that the muriatic acid could not exist without water in the state of gas; that it then contains one fourth of its weight; and that water alone possessed the property of taking it up from its dry combinations. It must be remarked, that in all the experiments made with the metals, the water, on being decomposed, has always produced a quantity of oxide equal to what was required by the acid in order to neutralize it; so that, for every result, hydrogen and a neutral salt were obtained. The limits of this report do not admit of our detailing all the experiments made by Messrs. Gay Lussac and Thenard; but we ought not to pass over the happy application which these chemists have made, in the decompositions of the muriate of soda, of the affinity which the muriatic acid has for water. We know that soda enters as a primary matter into several branches of manufacture, and it is very important to possess a simple and direct method of extracting this alkali from common salt.

As to the oxygenized muriatic acid, Messrs. Gay Lussac and Thenard subjected it to numerous experiments:—"These," they inform us, "ought to give an idea of the constitution of this acid totally different from that which has been formed. It has been regarded as the most easily decomposed body, and on the contrary it resists the action of the most energetic agents. We cannot extract the muriatic acid from it in the state of gas, except by means of water or hydrogen." This acid weighs 2·47 more than air. It contains the half of its volume of oxygen gas, and all the water which it can form with the hydrogen is retained by the muriatic acid which it contains. This water makes one fourth of the weight of this last acid.

The action of the metal of the potash on the oxides and
e metallic salts, and on the earthy and alkaline salts, has
o been particularly examined by Messrs. Gay Lussac
d Thenard. It results from their inquiries, that all the
dies in which we know the presence of oxygen are de-
mposed by this metal; that this decomposition almost
vays takes place with an extrication of light and heat; that
is extrication is the more considerable in proportion as
e oxygen is less condensed; and that, consequently, this
ght furnish a method of appreciating the degree of con-
nsation of the oxygen in any body.

After having operated on potash and soda, by means of
e Voltaic pile, the charges m ioned in the early part
this memoir, it was natura o endeavour to produce
alogous effects on the other alis, and on the earths.
short, Mr. Davy undertook nerous experiments, in
der to discover, according t s system, the metals of
rytes, strontian, lime, magn silex, alumine, zircon,
d glucine. After several f ss efforts, he succeeded
de-oxygenizing the first four these substances, and in
rming amalgams of the ne als which resulted. He
inks that the other four so metallic oxides; but
s experiments, as he confes do not prove this in a
tisfactory manner.

Another amalgam produc y ammonia was dis-
vered last year, at Jena, r. Seebeck. This after-
irds became the subject certain researches by
essrs. Berzelius and Pontin f Stockholm, and by
r. Davy in England: all three agree in ascertaining am-
onia to be a metal. In the ordinary temperature of the
mosphere this amalgam has the consistence of butter, and
the cold it crystallizes in cubes; but the new metal has
t yet been obtained in a separate state. Messrs. Gay
ussac and Thenard have repeated and proved the correct-
ss of the above experiments. But this amalgam, which
d been formed by the action of the pile only, has been
oduced by the French chemists by the action of the
etal of potash, and they have ascertained that a slight
itation was sufficient to decompose it. By this simple
tion the mercury once more becomes fluid, and ammonia
d hydrogen are liberated in the proportion of 28 to 23.
ne mercury absorbs 141 times its volume of hydrogen
is, and 88 times its volume of ammoniacal gas, in order
pass to the state of amalgam: whence it results, according
our authors, that in this combination the mercury in-
eases about 0·0007 of its weight; whereas, according to

Mr. Davy, it should only increase one 12000th part. Thus the theory by which Messrs. Gay Lussac and Thenard explain the formation of potassium may be applied to the formation of ammonium. This new metal, according to them, is nothing but ammonia and hydrogen.

Finally, Mr. Davy has also directed his attention to sulphur, phosphorus, plumbago, charcoal, and the diamond. The chief experiments relative to these two first substances have been made on hydrogen, sulphuretted and phosphuretted gases, by means of potassium; and he concludes, from the results obtained by him, that these two inflammable bodies are combinations of hydrogen, oxygen, and an unknown base, and which has not yet been obtained in a separate state. As to the other substances, he is inclined to regard plumbago as an alloy of iron with a peculiar metal which is found in charcoal combined with hydrogen, and in the diamond with a small part of oxygen.

These ideas were too strongly contradictory of those which are commonly received, not to excite the inquiries of other chemists. Messrs. Gay Lussac and Thenard therefore made sulphur and phosphorus the subjects of a very extensive series of experiments; and as Mr. Davy had employed the hydrures in his experiments, the French chemists in the first place endeavoured to determine the elements of these substances with precision. They ascertained that sulphuretted hydrogen gas contains a volume of hydrogen equal to his quantity; that the phosphuretted hydrogen gas contains at least one and a half of its volume; that the former of these gases may be absorbed by potassium and sodium; and that in this absorption there is developed precisely the same quantity of hydrogen which the metal alone would give with ammonia and with water: lastly, that phosphuretted hydrogen gas is decomposed by potassium and sodium, so that the phosphorus is combined with this metal, and the hydrogen is set free. But these chemists have not confined their researches to the substances employed by Mr. Davy: they made experiments on arseniated hydrogen gas, and found that this gas acts with the new metals in the same way with phosphuretted hydrogen gas; and that the metallic arsenic may be combined with hydrogen so as to form a solid hydruret, which has the form of light flakes, of a brown colour. They concluded that sulphuretted and phosphuretted hydrogen gas, as well as sulphur and phosphorus, contain no oxygen, or at least that the experiments of Mr. Davy do not demonstrate it. They are of opinion, however, as has been already suggested, that sulphur, and perhaps phosphorus, ontains hydrogen.

We shall not presume to decide between the opinions of Ir. Davy and of Messrs. Gay Lussac and Thenard; but will not fail to be remarked, although this cannot lead any consequence injurious to modern chemistry, that ydrogen, which frequently in the theory of Stahl was othing else than phlogiston, produces combinations which ave all the characters of the metals.

In addition to the labours we have mentioned, we are ndebted to M. Gay Lussac for some observations on the ombination of gaseous substances with each other, which d him to prove that the gases, in such proportions as to ender them fit for combination, always produce com- ounds the elements of which are in very simple ratios with ach other. Thus, 100 parts of oxygen gas saturate ex- ctly 200 parts of hydrogen; the fluoric and muriatic gases, iixed with the ammoniacal gas, saturate a volume of the atter equal to their own, and form neutral salts, &c. But e observes, that, when we consider the proportions in eight, we obtain no simple ratio between the elements of similar combination. Moreover, he shows that the apparent ontractions which the gases undergo on combining, also orm very simple ratios with the primitive volume of the ases, or only with the volume of one of them; and he af- erwards makes the remark, that the apparent contraction oes not indicate the real contraction which the elements ave undergone in combining.

These observations have been followed up by a particular nquiry as to the nitrous vapour and nitrous gas considered s a eudiometrical method. Here we see in a very evident anner the influence of the quantities on the result of the ombinations. If we mix 200 parts of nitrous gas with 00 parts of oxygen gas, nitric acid is produced; and 100 arts of oxygen remain at liberty. If, on the contrary, we nix 100 parts of oxygen and 400 of nitrous gas, an ab- orption of 400 parts takes place, which produces nitrous cid, and 100 parts of nitrous gas remain free. Thus we btain nitric acid, or nitrous acid, according as either of the ases of which these acids are composed is predominant.

But in both cases the absorptions are always constant. Thus, the nitric acid is composed of 100 parts of azotic as and 200 of oxygen gas, or 100 of oxygen gas and 200 itrous gas. The nitrous acid results from the combina- ion of 100 parts of oxygen gas and 300 of nitrous gas. And if we add that the nitrous gas is composed of equal parts of oxygen gas and azotic gas, as M. Gay Lussac had already demonstrated, we shall have a complete history of the combinations of oxygen and azote.

M. Guyton de Morveau, in a course of experiments on the diamond and the substances which contain carbon, has endeavoured to determine the action of the diamond on water at a very high temperature. The water was decomposed, and carbonic acid produced.

M. Sage has communicated to us his researches on the revivification of silver by mercury in the nitrate of silver; on an acetate of ammonia, extracted from wood by distillation; on the analysis of the calcareous stone, called the printing stone; on the magnesia contained in shells, madrepores, limestone and the arragonite; on an ore of *arenaceous* iron; on an unknown petrifaction; and on the analysis of a petrified wood which was both cupreous and ferruginous. We regret that our limits do not permit us to enter more into the details of these numerous inquiries.

When chemistry descends from crude to organized bodies, the phænomena which come under its observation are more complex, and the results obtained more obscure. Thus has this branch of chemistry been neglected until lately; and most of the observations and discoveries with which it is enriched are undoubtedly owing to the labours of M. Fourcroy, (whose loss we have now to deplore,) and to his distinguished friend M. Vauquelin.

This last chemist has lately been occupied with the analysis of tobacco, with a view to ascertain the principles which characterize this plant, and which have recommended it to general use; and, finally, with a view to appreciate the modifications which it undergoes, in order to prepare it for becoming an article of commerce. It results, from M. Vauquelin's inquiries, that the broad-leafed tobacco plant *(Nicotiana latifolia)* contains an animal matter of an albuminous nature, some malate of lime, with an excess of acid, acetic acid, nitrate and muriate of potash, a red matter the nature of which is unknown, muriate of ammoniac, and, lastly, an acrid and volatile principle, which seems to be different from all those known in the vegetable kingdom. It is this principle which gives to tobacco the qualities which we know it possess; we may separate it from the plant by distiltation, and employ it separately. Prepared tobacco presented more sensibly, than when unprepared, carbonate of ammonia and muriate of lime.

M. Vauquelin, thinking that the juice of bella donna, from having effects on the animal œconomy analogous to those

hose of tobacco, contained the same acrid principle, pro-
eeded to analyse it; but he found only an animal sub-
tance, salts with a base of potash, and a bitter substance
rom which the juice of the bella donna receives its narco-
ic properties.

Under the head *Physiology*, we shall allude to the expe-
iments made by M. Vauquelin, on animals, with this
uice.

M. Chevreul has presented to the class some very exten-
ive experiments on vegetable substances. Some have for
heir objects, the bitter principle produced by the action of
he nitric acid on organized bodies which contain azote,
nd which had already occupied the attention of Messrs.
Haussman, Welther, Proust, Fourcroy, and Vauquelin.

M. Chevreul thinks that this bitter principle is composed
f nitric acid and a vegetable oily or resinous substance;
nd he ascribes the property which this substance has of
etonating, to the decomposition of nitric acid, to the
ormation of ammoniacal gas, of prussic acid, and oily hy-
rogen gas, &c. This agrees partly with the observations
f Messrs. Fourcroy and Vauquelin.

But along with this bitter principle, a resinous matter
nd a volatile acid are produced, on which M. Chevreul
as made various experiments, and which he regards as dif-
ering from the bitter principle only in consequence of its
aving a small portion of nitric acid.

A second inquiry of M. Chevreul has for its object the
ubstances formed by the action of the nitric acid on charry
r resinous bodies, and which have the property of preci-
itating gelatine. The first observations on this subject
vere made in England by Mr. Hatchett; and they in-
luced a belief that these substances were analogous to tan-
in. M. Chevreul is of opinion that this is erroneous,
nd that they differ from each other, not only according
o the kind of acid and other substance with which they
ave been prepared, but also according to the quantity of
cid which has entered into their composition.

Lastly, pursuing always the same kind of experiments,
M. Chevreul directed his attention to the different com-
ounds formed by the reaction of the sulphuric acid on
amphor. All the above researches obtained the appro-
ation of the class, and were ordered to be inserted in the
Mémoires des Savans Etrangers.

We ought to speak, perhaps, of the supposed discovery
made by M. Vinterl, of an earth which he calls *andronia*,
nd in which he thinks he has found some extraordinary
properties;

properties; and of the memoir of M. Pitaro, in which he endeavours to demonstrate, that a substance discovered in the Grotto de l'Arc, and analysed by M. Laugier, originates from the decomposition of the insects and reptiles which this grotto contains. But there are errors sometimes so palpable, that it is best perhaps to say nothing of them.

Every year has added to the store of knowledge we possess on the subject of the application of chemistry to the arts; and thus new proofs are afforded of the assistance which our wants, added to our industry, may derive from the sciences.

M. Chaptal, to whom manufacturers are already indebted for so many useful processes, has published some interesting observations on distillation from wines. We find, from the history which he gives of this art, by the description of the apparatus formerly employed, and that of the present day, that the processes for the production of spirits have been ameliorated in proportion to the improvements in chemical apparatus. One of the most important of these improvements, adopted in the South of France, consists of scarcely any thing else than the apparatus of Woulf on a large scale. The laws of evaporation, and the processes by means of which the liquids are heated by steam, have been ingeniously combined, in order to effect the distillation of spirits in an œconomical manner; but the observations of M. Chaptal will undoubtedly lead to new improvements in the preparation of spirits, and will contribute to preserve to this important branch of French trade, the superiority which it has acquired.

The same member has analysed seven specimens of colours found at Pompeia, which had been sent to him by the empress Josephine *.

M. Sage has been occupied in ascertaining the best processes for extracting quicklime, in order to obtain a solid mortar. He has also examined the nature of different pieces of stucco; the best method of giving the polish of marble to artificial stones; and, finally, he has given an account of a process for reducing white wax into soap.

The same author in a memoir, and Messrs. Guyton and Vauquelin in a report, have communicated some observations on the advantages and disadvantages of employing zinc in covering houses †. And on the request of the minister of the interior, the committee for chemistry has should

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* See Phil. Mag. vol. xxxiv. page 411.

† This is not a French discovery; nor is it new. See an ingenious paper by Mr. Randall on this subject in our 28th volume, p. 344.—Edit.

hown what are the descriptions of manufactories which are njurious to the health of the surrounding inhabitants; and neasures have been suggested for removing such as are uisances, without compromising the interests of the prorietors.

A report has been made to the Institute on a memoir by I. Tarry, relative to the composition of writing ink. The uthor has succeeded in making an ink which cannot be estroyed by the acids or alkalies, and which has only the light inconvenience of allowing its colouring matter to be eposited rather too easily. "The discovery of M. Tarry," ays the reporter, "promises a great benefit to society; iz. the introduction of an ch, not being susceptible f being obliterated by t nical agents at present nown, will put an end alsification of writings, hich is but too common."

Another, on the artifici ises of M. de Sauviac, ives reason to hope that shortly rival nature in ese productions, and fur w source of riches.

A committee has been nployed in examining a rocess of the late M. B or the composition of a reservative plaster of P ses built of stone are uickly covered with an ating, of a dirty gray olour; and this first cha cause of the deteriora-on which they soon aft ndergo. A small kind f spider fixes his web in ws on the surface of the one: these webs accum d, with the dust which ey collect, form the e st just mentioned, in hich lichens sometimes t, and which naturally tain a constant humidity at the surface of the stones: e frosts then produce considerable injury, and give occa-on for those raspings, which are in themselves a real de-rioration.

A plaster therefore became a desideratum, which should ll up the inequalities of the stone without making the gles look clumsy, or deadening the carvings, and which ould resist rain and other effects of weather. The late I. Bachelier had made some interesting experiments on is subject; and the above committee, aided by his son, ave succeeded in producing a plaster which has resisted e tests to which they exposed it, and which gives fair ounds to expect that our buildings will in future be procted from the causes of decay above enumerated.

MINERALOGY.

Our labours in mineralogy will appear a

comparison with those of which we have had occasion to give an account in former years.

M. Guyton has made us acquainted with a new crystalline form of the diamond. We know that the forms under which it is most frequently presented, are the regular octahedron and the dodecahedron with rhomboidal faces. The variety discovered by our associate is formed of two demi-spheroids, the returned position of which, imperfectly terminated at one of its extremities, presents at the other very clear re-entering angles, which characterize the form called hemitrope by M. Hauy.

The same member, having directed his inquiries to the tenacity of metals, was led to some new experiments on the diminution of specific gravity in lead, in consequence of being put into a vice, as proved by Muschenbroek; and the cause of which remained unknown. Pieces of this metal were driven into ferules; and when the dies and the latter were adjusted, so that there could be no oozing out of metal, nor was the lead permitted to become soft, the lead in this case was found, like all the other metals, to increase in specific gravity by the operation.

M. Sage has communicated to the class his inquiries respecting emery, and the substances which are calculated to supply its place in polishing. It results from his observations, that the pulverized chrysolite of volcanoes may supply the place of emery. All the artists who employed it have been satisfied with its effects.

[To be continued.]

LII. *Intelligence and Miscellaneous Articles.*

DE LUC'S ELECTRIC COLUMN.

We learn, by a communication dated 23d April, that the small bells which were connected to De Luc's *electric column*, mentioned in our last number, ceased to ring for about a minute on the 24th March; and again on the same day for about three minutes. They were also supposed to have stopped for about half a minute the next day; but this is much doubted. Since that time they have been known once to cease ringing. On the 15th April, the closet where they are placed was opened, when the clapper was observed to vibrate with very great velocity. It is thought that the loudness of the sound is considerably increased of late; also that the vibrations of the clapper are quicker than when the apparatus was put into the closet on the 14th March. [April 28.]

The cranium of a horned animal, the race of which ems to be extinct, has been recently dug up at Oeltre, ar Ninava, in Russia. From the description given of is part of the skeleton, the animal must have been at least or 12 feet long; the horns which are attached to the ad, and which have partly passed into a fossil state, far ceed in size those of the oxen of the present day: when easured at the root, they are a foot and a half in circumference, and two feet and a half long. After finding the anium, several efforts were made to recover the entire eleton, but two teeth only were found. Foreign naturats are of opinion, that the head in question must have longed to the race of *Urus* or *Aurochs*, mentioned by esar in the 6th book of his Commentaries, and which are pposed to exist still in the mountains of Siberia, and even the forests of Poland.

Sir George Mackenzie, accompanied by Mr. Henry olland, and Mr. Richard Bright, of the university of inburgh, has sailed from Leith for Stromness; from ence they are to proceed to Iceland, in a vessel which is pected there from London.

The object of this arduous undertaking is to explore part that inhospitable country, which, without British comseration, would, in consequence of the war, be deprived the absolute necessaries of life.

In the circumscribed state of our commerce, this country very well worth the attention of Great Britain. In ren for our coarse fabrics, we might procure such articles Iceland, under proper management, would afford in eat plenty; such as fish, oil, feathers, and sulphur, the rcity of which last article has been such as to have aldy attracted the attention of parliament.

MEDICAL AND CHEMICAL LECTURES.

Dr. CLUTTERBUCK will begin his Summer Course of ctures on the Theory and Practice of Physic, Materia edica, and Chemistry, on Monday, June the 4th, at a arter before Ten in the morning, at his house, No. 1, escent, New Bridge Street; where further particulars y be had.

Mr. TAUNTON will commence his Summer Course of ctures on Anatomy, Physiology, Pathology, and Sury, on Saturday, May 26th, 1810, at 8 o'clock in the ning precisely. The Lectures will be continued every ceeding Tuesday, Thursday, and Saturday, at the same ur, until the completion of the Course.

In the above Course of Lectures it is proposed to take a comprehensive view of the structure and œconomy of the living body, and to consider the causes, symptoms, nature, and treatment of surgical diseases, with the mode of performing the different surgical operations: forming a complete course of anatomical and physiological instruction, for the medical or surgical student, the artist, the professional or private gentleman.

An ample field for professional edification will be afforded by the opportunity which pupils may have of attending the clinical and other practice of both the City and Finsbury Dispensaries.

Particulars may be had on applying to Mr. Taunton, Greville-Street, Hatton Garden.

LIST OF PATENTS FOR NEW INVENTIONS.

To Frederick Koenig, of Camden Town, in the county of Middlesex, printer, for a method of printing by means of machinery.—March 29, 1810.

To Jonathan Ridgway, of Manchester, plumber and glazier, for an improved method for preparing rollers and blocks used for calico printing.—April 6.

To John Stancliffe, of Caius College, Cambridge, bachelor of physic, for certain improvements in apparatus for combination and condensation of gases and vapours applicable to processes of distillation.—April 6.

To John Woodhouse, of Bromsgrove, in the county of Worcester, for several improvements relative to canals. —April 6.

To William Speer, late of the city of Dublin, but now of the city of Westminster, esq., for a new or improved method or process of increasing the inflammability and combustibility, and of improving the light of oils used for burning, particularly applicable to the oils refined according to the patent process, which will also improve oils refined according to the patent process, and oils when used for burning.—April 6.

To James Fussel, of Mells, near Frome, in the county of Somerset, iron manufacturer, for a method of making and working forge and other bellows.—April 6.

To Charles Frederick Davis, of the parish of Itchcombe, in the county of Gloucester, clothier, for an improvement in the manufacture of woollen cloth.—April 6.

To William Parr, of the Portland Hotel, Great Portland Street, in the county of Middlesex, esq., for his improved gunpowder.—April 11.

METEOROLOGICAL TABLE,

By Mr. Carey, of the Strand,

For April 1810.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Thermometer. Noon.	Thermometer. 11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Iarch 27	44	52°	42°	29·59	0	Rain
28	44	50	40	·78	22	Fair
29	40	51	39	[illegible]	20	Showery
30	40	49	42	[illegible]	25	Cloudy
31	43	51	45	[illegible]	0	Showery
April 1	45	49	42	[illegible]	0	Showery
2	40	45	40	[illegible]	15	Cloudy
3	43	56	46	[illegible]	21	Cloudy
4	45	47	39	[illegible]	0	Rain
5	37	47	45	[illegible]	36	Fair
6	42	45	42	[illegible]	0	Stormy
7	41	53	43	[illegible]	31	Cloudy
8	44	51	43	[illegible]	20	Cloudy
9	45	48	44	[illegible]	0	Rain
10	44	42	39	[illegible]	0	Rain
11	37	42	36	[illegible]	5	Showery
12	33	41	34	[illegible]	25	Cloudy
13	34	42	35	[illegible]	20	Cloudy
14	36	47	39	[illegible]	26	Cloudy
15	44	49	40	·71	36	Fair
16	39	47	40	·48	10	Showery
17	42	52	45	·64	20	Showery
18	48	56	46	·70	35	Cloudy
19	49	57	47	·80	42	Fair
20	48	57	45	30·10	39	Fair
21	49	59	47	·17	46	Fair
22	50	63	54	·16	40	Fair
23	54	66	55	·20	39	Fair
24	54	63	44	·15	40	Fair
25	44	58	43	·14	57	Fair
26	47	59	45	·12	63	Fair

N. B. The Barometer's height is taken at one o'clock.

LIII. *Description of an improved Apparatus for the Decomposition of Potash and Soda.* By WILLIAM JOHNS, *Esq.*

To Mr. Tilloch.

SIR, THE publicity of the Philosophical Magazine induces me to transmit for insertion the following account of some successful attempts to repeat the brilliant experiments of professor Davy, in the decomposition of potash and soda, on a more œconomical plan than has before been suggested [*], and which I hope will enable your readers to obtain potassium, &c. for their use at a very moderate expense.

I am yours, &c.

WILLIAM JOHNS.

April 16, 1810.
No. 3, Orford Row, Kent Road.

EARLY in the present year, wishing to decompose potass with the bent gun-barrel as is done at the Royal Institution, and not enjoying the ample resources of its excellent chemical professor; it became a desideratum to construct an apparatus, which might serve for the repetition of the experiment, instead of cutting the gun-barrel in pieces, as in the method before in use,—a practice attended with considerable expense, and which could not but check the ardour of those who are fond of chemical researches, and prevent the frequent repetition of the experiment: the disappointment resulting from an unsuccessful experiment being always much heightened when accompanied with the loss of an expensive apparatus.

Having succeeded in constructing an apparatus simple and efficient, and which has met the approbation of several eminent chemists, I herewith send you a drawing and description of it.

The apparatus consists of a common gun-barrel with one bend, and one of the ends inclining downwards a little. The inclining straight part is cut off from the bent portion at about three inches distance, is ground into it, being made to fit air-tight. Underneath is a small thin iron tube open at both ends, made a little conical, which when the apparatus is taken to pieces is placed within the interior of the straight piece, one half of it going into this part; the other will be received in the opposite part, when the apparatus is again put together. This small tube is to collect the potasium in, and which purpose it answers exceedingly well, bviating an inconvenience and waste which sometimes appen from its being dispersed through the barrel.

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[*] See Phil. Mag. vol. xxxii. p. 276.

In the first apparatus which I had constructed on this lan, I had the parts made to screw together: this answered xtremely well, but it possesses no advantage over a ground oint, and is more expensive.

The first attempt to use this apparatus, I obtained less han 20 grains of the potassium: this was, however, encouagement, my apparatus being in every respect perfect as vhen I began the process; it served me twelve times very ompletely, and in the last experiment I collected 140 rains of the metalloid from eleven drachms troy of the alali.

Having succeeded in preserving my apparatus (it at ength was melted in a small plac , the lute having fallen off n the process), my object was o substitute the common *austic potass* instead of the pure potass which had been enerally used. I tried it; and o my satisfaction obtained he result just mentioned. this I was much pleased, he kali purum being sold at one eighth of the price of he pure potass. In my subsequent experiments twelve lrachms yielded the extraordi y produce of 170 grains, he apparatus being taken out he fire entirely free from eing acted on.

Having attempted the caustic soda alone, and not sueeding with it, I used the proportions of two drachms of oda to six drachms of potass, a I obtained 60 grains of a eautiful compound nearly fluid, of considerable lustre, nd which lustre was inconsiderably diminished some days fter; it nearly floated in the naphtha, being apparently of he same specific gravity.

From one part of soda to seven of potass, the proportion sed by Mr. Davy in a similar experiment, I obtained from en drachms 150 grains of metalloids, in appearance resembling quicksilver, equally fluid at a low temperature, though vith this striking difference, of less specific gravity than ome very pure naphtha, in which it floated.

The furnace I use is the common black-lead crucible, bout eight inches diameter; I generally leave off the upper ection as well as the flue. The furnace used at the Institution is, if I do not mistake, above 20 inches. The louble bellows are small in proportion, about 36 inches y 14 inches.

In performing the experiment, it may be observed in ddition to what has been said, I leave the stopper out of the potass tube till near the end of the process, bring it to a red heat, before any of the alkali is supplied; then the iron turnings being at a white heat, I introduce pieces of the potass in succession: these are immediately brought into igneous fusion, admitting of part of the water contained in it to pass off, and in this state it drops down on the iron. When the last portion is introduced, and this also is become red hot, the stopper is put into the tube and luted over. This is all done in less than ten minutes, from the first supplying of the potass tube with alkali: at the other end a glass tube passes down into some olive oil: here the hydrogen escapes, often in a state of brilliant combustion, and potassium deposits in the glass tube: this however should be prevented by wet cloths applied to the straight part of the gun-barrel.

Description of the Drawing (Plate IX.)

A. Potass tube.
B. Stopper to be put in at the end of the process.
C. The situation of the iron turnings.
D. The fire-place.
E. The grate.
F. The pipe of the bellows.
G. Straight part ground to the bent part.
H. Thin iron tube to be placed within the joint, to collect the potassium.
I. Glass tube.
K. Olive oil.
L. The part where the gun-barrel begins to decline downward. N. B. This inclination should commence precisely at the point where the tube emerges from the furnace, to prevent the potassium from flowing back on the iron turnings.
M. Stop-cock screwed into a socket fixed in the gun-barrel, which is shut the moment gas ceases to be given off, and which is known by the consequent absorption*.

LIV. *On the Composition and Decomposition of Forces. Translated from* "Traité élémentaire de Statique, par Gaspard Monge;" *by Mr.* W. Marrat, *of Boston, Lincolnshire**.

1. When a force P, (fig. 1 and 2,) applied to a determined point C, of the solid body AB, draws, or pushes this

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* Communicated by Mr. Marrat.

s body in any direction CF; we may be permitted to nsider this force as if it were applied to any other point D, *in the body*, which is in the direction of this force.

For as the several points of the body which are in the ght line CF can neither approach towards nor recede from one another, it is evident that no point whatever in this line can move without moving all the others, in the same manner as if the force were immediately applied to them.

We may also be permitted to consider the force P as if t were applied to any other point G *out of the body*, in the irection of the force, provided that this point be invariably attached to the body.

2. It follows then, that he direction of the force there be found either in ly a fixed point D, or out it an unmoveable obst provided that in this latter case the obstacle be inva ttached to the body, the force will be destroyed, body remain at rest; for we may regard the force diately applied to the fixed point, and its effect w royed by the resistance of that point.

3. Reciprocally, if th P applied to the body AB be destroyed by the resi a single fixed point, this point will be found in ion of the force; for this point could not destroy ct of the force unless it pposed the motion of t t of application C, and it ould not prevent this m nless it were in the right ne in which the force te move the point of application.

AXIOMS.

I.

A point cannot move several ways at once.

Therefore, when several forces differently directed are applied at the same time to the same point, this point either remains at rest, or moves in a single direction, in the same manner as if it were moved by a single force in that direction, and capable of producing the same effect.

Thus, whatever may be the number and direction of forces applied at the same time to the same point, there always exists a single force which can move it, or which tends to move it, in the same manner as all the forces together. This single force is called the **RESULTANT**, and the several forces which compose the system, and act all together, are called **COMPOSANTS**. The operation by which we seek the *resultant* of several given *composant* forces, is called the COMPOSITION OF FORCES, and that by which we find the composants when we know the resultant, is called the DECOMPOSITION OF FORCES*.

II.

7. Two forces equal and directly opposite, applied at the same time to the same point, destroy each other and are in *equilibrio*. Reciprocally, when two forces are in equilibrio, they are equal and direetly opposite.

8. If, therefore, several forces differently directed are applied to the same point, to produce an equilibrium, that is to destroy the effect of their resultant, we must apply to this point a single force equal to their resultant, and directly opposite to it; or else we must apply several forces the resultant of which may be equal and directly opposite to the resultant of the first.

9. Reciprocally, when several forces, differently directed, and applied to the same point, are in equilibrio, their resultant is nothing, or, which comes to the same, any one of these forces is equal and directly opposite to the resultant of all the others, or lastly the resultant of any number of forces is equal and directly opposite to the resultant of all the others.

III.

10. If several forces applied to the same point have all the same direction, and all act the same way, the effect produced upon this point is the same as would be produced by a single force equal to their sum, acting the same way, and in the same direction; consequently this single force is their *resultant*.

11. Hence, to produce an equilibrium with all these forces, we must apply to the same point, and in an opposite direction, a force equal to their sum; for this force will be equal and directly opposite to their resultant.

12. It follows then, FIRST, that when two unequal forces are applied to the same point, but in opposite directions, the resultant will be in the direction of the greater, and equal to their difference. For the greater of these two forces may be considered as composed of *two* forces, ditected the same way, one of which is equal to the less and rhe other equal to their difference; now of these two latter forces, the first is destroyed by the less (7); therefore, there remains to move the point, only the difference, which acts in the same direction as the greater.

13. SECONDLY. That if ever so many forces be applied to the same point, some of which act one way and the

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* English authors call this *resolution* of forces.

hers the contrary; after having found the sum of all those hich act one way, and also the sum of all those which t the contrary way, the resultant of all these forces is ual to the difference of these sums, and is directed the me way as the greater.

14. Therefore, to produce an equilibrium with all these rces, we must apply to the same point, and in the di- ction of the less sum, a force equal to the difference of e sums. For this force will be equal and directly oppo- e to their resultant.

THEOREM.

15. If to the extremities of a inflexible right line AB, g. 3) two equal forces P, Q, b applied, both of which t the same way, and the directi s of which AP, BQ, are rallel to each other:

1. The direction of the res R of these forces is parallel to AP, BQ, and passes th gh the middle of AB.

2. The resultant is equal to sum $P+Q$ of these two rces.

DEMONSTRATION OF THE FIRST PART. Let another flexible right line DE perpend ular to the direction of e forces be invariably attached o the right line AB, and oduce the direction of the forc P, Q, till they meet the ght line DE in D and E; we may consider these forces as plied at D and E.

Divide DE into two equal in C, and on that side to hich the forces tend to mov s right line place an im- oveable obstacle at C, and the right line ED will be at st; for, the parts of the line on each side of the obstacle ing equal, there is no reason why one of these equal rces should overcome the other; therefore the resultant ill be destroyed by the obstacle C, or the resultant will ss through the point C. In the same manner it may be own that the direction of the resultant of the two forces sses through I, the middle of any other right line GH rallel to DE.

Therefore, it passes at the same time through the two ints C, I, which are equally distant from the direction the forces P, Q, of course it is parallel to them, and sses also through the middle of AB.

PART II. The direction of the two forces P, Q, and that their resultant R, being parallel, we may consider them concurring in a point at an infinite distance, and the o forces P, Q, as both applied to this point: now the sultant of two forces applied to the same point is equal their sum (10); therefore the resultant of the two forces Q, is equal to the sum $P+Q$ of those forces.

16. *Cor.* 1. To produce an equilibrium with the forces P, Q, we have only to apply to the point K a third force equal to their sum, acting in a contrary direction, but parallel to AP or BQ: for this third force will be equal and directly opposite to their resultant.

17. *Cor.* 2. If after having divided an inflexible right line into any number of equal parts, we apply to all these points of division equal forces the directions of which are parallel among themselves; the resultant of all these forces passes through the middle of the right line in a direction parallel to that of the forces, and is equal to their sum.

For all the particular resultants of these forces taken two and two at equal distances from the middle of the right line, will pass through the middle, in the same direction, and each resultant will be equal to the two forces which are its composants: therefore, the general resultant will pass through the middle of the line (10), according to the same direction, and will be equal to the sum of all the particular resultants, or equal to the sum of all the composants.

THEOREM.

18. If to the extremities of an inflexible right line AB (fig. 4.) two unequal forces P, Q, be applied, the directions of which AP, BQ, are parallel and act the same way:

1. The resultant R is equal to their sum, and its direction is parallel to that of the forces.

2. The point of application C, of the resultant R, divides the right line AB into two parts which are reciprocally proportional to the forces; or in such a manner that

$$P : Q :: BC : AC.$$

DEMONSTRATION OF FIRST PART. For divide the right line AB into two parts in D, directly proportional to the forces P, Q, and we shall have P : Q : : AD : DB.

Produce the inflexible line AB both ways, and make AE=AD and BF=BD, also conceive that the two forces P and Q are uniformly distributed through every point of the right line EF, and act in directions parallel to the direction of the two forces P, Q; it is evident that the force P will be distributed through the part DE, and that the force Q will be distributed through the part DF.

Moreover, the force P will be the resultant of all the forces distributed through ED, and Q that of all the forces distributed through the line DF (17): hence the general resultant of all the forces uniformly distributed through the line EF, will pass through C, the middle of EF, and according to a direction parallel to the direction of the

composants,

omposants, and is equal to their sum: therefore, the re-
ultant of the forces P, Q, is equal to their sum, and its
irection, which passes through C, is parallel to BQ or AP.

2. The right line AB being the half of EF, we have
B=EC, and by subtracting from each of these equal
uantities the part AC, which is common, we have BC=
A=AD.

Also, because AB=CF, by taking away the common
art CB, we have AC=BF=BD, and because, by suppo-
ition, $P : Q :: AD : DB$,
e shall have ..... $P : Q :: BC : AC$.

But the resultant of the two forces P, Q, has been proved
o pass through C: hence the point of application of this
esultant divides the right line AB into two parts which
re reciprocally proportional to the two forces.

19. *Cor.* 1. Therefore, to produce an equilibrium with
he two forces P, Q, we must divide the right line AB in
, so that the two parts may be reciprocally proportional
o these two forces, and apply to the point C a third force
qual to the sum P+Q, in a contrary direction but parallel
o AP or BQ.

20. *Remark.* If the relation of the forces P, Q, and
he length of the right line AB be given in numbers, and
e want to determine the distance of the point C from
or B, the proportion

$$P : Q :: BC : AC$$

annot be immediately applied, because in this proportion
e only know the two first terms: but since

$$P : Q :: BC : AC,$$

by composition $P+Q : Q :: BC+AC=AB : AC$;
also $P+Q : P :: AB : BC$;

n each of which the three first terms are given.

21. *Cor.* 2. When a single force R is applied to a point
, in the inflexible right line AB, we can always resolve
t into two others P, Q, which being applied to the two
iven points A, B, and directed parallel to RC, will pro-
uce the same effect; for the force R may be divided into
wo parts which are reciprocally proportional to the line
C, BC, by means of the two following proportions

$$AB : BC :: R : P$$
$$AB : AC :: R : Q.$$

n each of which we know the three first terms. And
he resultant of the two forces P, Q, has the same quantity
nd direction, and acts the same way as the force R.

22. *Cor.* 3. Every thing being (in fig. 5) as in the pre-
eding corollary, if we apply to the point C, of the right
line

line AB, a force S equal and directly opposite to the resultant of the two forces P, Q, in such a manner that S=R =P+Q; the three forces P, Q, S, will be in equilibrio (19), and either of the two forces P, Q, may be regarded as equal and directly opposite to the resultant of the other two. Hence the resultant of two forces S, Q, of which the directions are parallel, and which act contrary ways, is a force *p*, equal and directly opposite to the force P. Now the force P is equal to the difference of the forces S, Q, and acts in a direction contrary to that of the greater S of these two forces; therefore, 1. The resultant *p* of the two forces S, Q, is equal to their difference S—Q, and it acts the same way as the greater, in a direction parallel to that of these two forces.

Moreover we have P+Q or S : Q : : AB : AC (20).

2. The distances of the point A of application of this resultant from the two points C, B, are reciprocally proportional to the forces S, Q.

23. *Remark.* If the ratio of the two forces S, Q, and the length of the right line BC are given in numbers, and we would find the distances of the point A from the points B, C, the preceding proportion cannot be directly applied, because in this proportion we know only the two first terms; but from it we easily deduce this by division of ratios; viz. S—Q : Q : : AB—AC or BC : AC in which we know the three first terms.

We find the distance AB by this other proportion,

S—Q : S : : AB—AC or BC : AB.

24. *Cor.* 4. If the two forces S, Q, the directions of which are parallel and which act contrary ways, are equal between themselves, 1. their resultant P, which is equal to S—Q, (22) becomes nothing; 2. in the proportion S—Q : Q : : BC : AC, the second term being indefinitely great with respect to the first, which is nothing, the fourth term AC is also indefinitely great with respect to the third. Therefore, the point A of application of the resultant P is at an indefinite distance from the point C. Hence, to produce an equilibrium with the forces S, Q, we must apply to the inflexible right line a force equal nothing, the direction of which passes at an indefinite distance: this is not absurd, but it cannot be executed.

We see then that it is impossible, by means of a single force, to produce an equilibrium with two equal forces the directions of which are parallel and which act contrary ways; but by means of two forces we can produce with them an equilibrium in an indefinite number of ways.

PROBLEM.

25. Any number of forces P, Q, R, S, &c. the directions
which are parallel, and which act the same way, being
lied to the points A, B, C, D, &c. given in position,
. 6) and connected in an invariable manner; to deter-
ne the resultant of all these forces.

OLUTION. Take any two of the forces, as P, Q, and
ermine (20) their resultant T; this resultant will be
ial to $P+Q$, and its direction will be parallel to that of
two forces P, Q, and we find its point of application
y the following proportion, $P+Q : Q :: AB : AE$.
nstead now of the two forces ınd Q we may substitute
ir resultant T, and having dra n the right line EC, we
st determine the resultant V f the two forces T, R;
resultant V will also be that f the three forces P, Q,
its quantity equal to $T+R$ r $=P+Q+R$, and its
nt of application F will be und by this proportion,

$$T+R \text{ or } P+Q+R : R . \quad EC : EF.$$

nstead of the three forces P, Q R, we may now substi-
their resultant V, and after aving drawn the right
FD, we must find the result t X of the two forces
S; this resultant X will be tha of the four forces P, Q,
S, and its quantity equal $P+Q$ $-R+S$; and we deter-
ne upon FD its point of appl tion G, by the propor-
,

$$V+S \text{ or } P+Q+R \quad : S :: FD : FG.$$

n the same manner we ma ceed for any number of
es whatever, and the quant of the last resultant will
equal to the sum of all the forces in the system.

6. *Cor.* 1. Hence, by supposing that the point G is
ariably connected to the points A, B, C, D . . . we shall
e an equilibrium with all the forces P, Q, R, S . . . by
lying to the point G a force, the direction of which is
allel to that of the original forces, which acts the contra-
vay, and which is equal to their sum $P+Q+R+S \ldots$,

7. *Cor.* 2. If among the forces P, Q, R, S, . . . the di-
ions of which are parallel, some of them act one way
the remainder the contrary way; we first determine the
ticular resultant of those which act one way, and then
particular resultant of all those which act the contrary
. For all the forces being reduced to two acting in
osite directions, in determining by art. 23 the result-
of these two forces we have the general resultant,
, consequently, the force which, if applied in a contrary
ction, would keep the whole in equilibrio. The ge-
al resultant being equal to the difference between the

two particular resultants (22), and each of these being equal to the sum of those which compose it (25), it follows that the general resultant is equal to the excess of the sum of the forces which act one way, above the sum of the forces which act the contrary way.

28. *Cor.* 3. If the forces P, Q, R, S, ...remain parallel among themselves, and without changing in quantity, take another direction, and become p, q, r, s, ..., the resultant t of the two first will still pass through E and be equal to their sum $p+q$. Likewise the resultant v of the three forces p, q, r passes through the point F, and is equal to $p+q+r$. In the same manner the resultant x of the four forces p, q, r, s will pass through the point G and will be equal to the sum $p+q+r+s$. Hence the general resultant of all the forces p, q, r, s . . will always pass through the same point as the general resultant of the first forces P, Q, R, S, &c . . . .

We see then that when the quantities and the points of application of parallel forces remain the same, the resultant of these forces always passes through the same point whatever may be their direction, and the quantity of this resultant is always equal to their sum.

The point through which the resultant of parallel forces always passes, whatever may be their direction, is called *the centre of parallel forces.*

It is easy to perceive that if the points of application A, B, C, D, . . . of the parallel forces P, Q, R, S . . . .are in the same plane, the centre of these forces is in the same plane; for this plane contains the right line AB, and consequently the point E in this right line, which is the centre of the forces P, Q: it contains also the right line EC, and of course it contains the centre F of the forces P, Q, R: it also contains the right line FD, and consequently it contains the centre G of the forces P, Q, R, S; and so on.

If the points of application are in the same right line, we can demonstrate in the same manner that the centre of parallel forces is in the same right line.

THEOREM.

29. Two forces applied to the same body cannot have a resultant, unless their directions concur in the same point, and are contained in the same plane.

Demonstration. When the directions of two forces do not concur in the same point, they cannot be considered as destined to move a single point; therefore a single force cannot produce the same effect, and consequently they have no resultant.

THEOREM.

THEOREM.

30. If the directions of two forces P, Q, applied to two
oints A, B, (fig. 7 and 8) of the same body, are contained
ı the same plane and concur in a certain point D :

1stly. The direction of the resultant of these forces will
ass through the point of concourse D ;

2dly. The direction of the resultant is in the same plane
; the two forces P, Q, which are its composants.

DEMONSTRATION OF 1. The point D being found in
ıe direction of both the forces, if we suppose that this
oint is connected to the b
an invariable manner,
e may consider the two forc
, Q, instead of being ap-
ied to the points A, B, as
lied to the point D, and
at they have no other eff
ıan a tendency to move
is point; therefore, their
ant may also be consi-
red as having no other effe
Now a single force can-
t act upon a single point u
it be immediately applied
this point. Therefore th
ultant of the two forces
Q, may be regarded as ap
to this point. Hence,
e direction of this force pas
rough the point of con-
urse of its two composants.

DEMONSTRATION OF THE
If at the two points of
plication A, B, we conceive
nflexible right line to be
ached, in an invariable ma
, the effect of the two
ces P, Q, and consequentl
of their resultant, is evi-
ıtly a tendency to move the
line AB. Now a single
ce cannot move a right
unless it be immediately
lied to some point in this line. Therefore, the resultant
the two forces P, Q, may be considered as applied to
ne point in the right line AB. Hence the direction of
s force passes at the same time through the point D, and
o through a point in the right line AB; it is therefore
nprised in the plane of the triangle ABD, determined by
directions of the two composants P, Q.

1. *Cor.* It follows then that if three forces P, Q, R,
lied to the same body, are in equilibrio among them-
es, the directions of these three forces concur in the
e point D, and are comprised in the same plane.

or these three forces being in equilibrio, any one of
m is equal and directly opposite to the resultant of the
er two; consequently any two of these forces have a
ltant; therefore, (29) the directions of these two forces
comprised in the same plane, and concur in the same
ıt; and the direction (30) of the resultant of these two
es, and consequently that of the third force which

keeps them in equilibrio, passes through the point of concourse, and is comprised in the plane determined by their directions.

LEMMA.

32. If a power P be applied to the circumference of a circle moveable about its centre A (fig. 9), according to the direction BP, which is a tangent to the circle at the point B; this power has the same tendency to turn the circle about its centre, as if it were applied to any other point C, and in the direction CQ, which is also a tangent at the point C.

THEOREM.

33. When the directions of two forces P, Q, are in the same plane, and concur in the same point A (fig. 10), if we take upon these directions the right lines AB, AC, proportional to these forces, in such a manner that

$$P : Q :: AB : AC;$$

and having completed the parallelogram AB DC; the direction of the resultant of these two forces will be according to the diagonal AD of the parallelogram.

Demonstration. Conceive for a moment that the point D is an immoveable obstacle, and from this point upon the directions of the two forces let fall the perpendiculars DE, DF; the triangles BED, CFD are similar, because the angles at B and C being each of them equal to the angle A, are equal to each other; therefore we have

$$DC : DB :: DF : DE;$$

& by the supposition $P : Q :: AB : AC$ or $:: DC : DB$;

therefore, $P : Q :: DF : DE$.

From the point D as a centre, with the radius DF, describe the circular arc FG, meeting ED produced in G; then, regarding this arc and the right line EG as inflexible lines invariably connected to the point A; conceive that the force P is applied at E, in the direction EP, and that a force M, equal to the force Q, is applied to the point G, in a direction parallel to AP, and consequently in the direction of a tangent to the arc FG; and because the force $M = Q$, and $DF = DG$, we have

$$P : M :: DG : DE.$$

Therefore (18) the resultant of the two parallel forces P, M, passes through the fixed point D, and is destroyed by the resistance of this point; also these two forces are in equilibrio about this point.

Now the force Q, the direction of which is a tangent to the arc FG, and which we may regard as being applied to the

: point F of its direction, tends to turn this arc in the
ne manner as the force M (32), and may be substituted
it in order to counterbalance the force P: therefore the
o forces P, Q, are also in equilibrio about the fixed point
and their resultant will be destroyed by the resistance of
s point, and consequently the direction of this resultant
l pass through the point D.

But we have seen that the resultant of the two forces
Q, passes through the point of concourse A of their
ections (30); therefore this resultant will be directed
ording to the diagonal AD.

34. *Cor.* 1. If from any point D taken in the direction
) of the resultant of two forces P, Q, we draw the right
s DB, DC, parallel to the ections of these forces,
shall have a parallelogram A D, the sides of which
, AC, are proportional to the rces P, Q; that is, we
ll have P : Q : : AB : AC : DC : DB.

For if these sides are not pro onal to the forces, their
ltant will be directed accor to the diagonal of the
allelogram, the sides of whic re proportional to these
es (33), and not according AD; which is contrary
he supposition.

5. *Cor.* 2. If from the poi D, taken upon the di-
ion AD of the resultant of tw forces P, Q, we let fall
perpendiculars DE, DF, the directions of these
es; these perpendiculars e reciprocally propor-
al to the forces P, Q.

For it has been shown above 4) that P : Q : : DC :
; and the triangles DBE, DC , being similar, give

DC : DB : : DF : DE;

therefore P : Q : : DF : DE.

THEOREM.

6. When the directions of two forces P, Q, are com-
ed in the same plane, and concur in a point A (fig. 11);
ve take upon these directions the right lines AB, AC,
ortional to these forces, in such a manner that we have

P : Q : : AB : AC;

having finished the parallelogram AB DC; the re-
ant R of these two forces will be represented in quantity
direction by the diagonal AD of the parallelogram;
is, we shall have

P : Q : R : : AB : AC : AD.

EMONSTRATION. We have already seen (33) that the
ltant of the two forces P, Q, will be directed according
e diagonal AD of the parallelogram; it therefore re-
mains

mains only to show that its quantity will be represented by this diagonal. To the point A apply a force S equal and directly opposite to the resultant R, this force will be directed according to the prolongation of the diagonal DA, and the three forces P, Q, S, will be in equilibrio. Therefore the force Q, will also be equal and directly opposite to the resultant of the two other forces P, S; and consequently this last resultant will be directed according to the prolongation of the right line CA. Produce CA till AH =AC, and draw the right line HB, which will be parallel to AD, and consequently to the direction of the force S, also from H draw HK parallel to the direction of the force P; the two forces P, S, are proportional to the sides AK, AB, of the parallelogram ABHK, (34); that is, we shall have P : S : : AB : AK or HB.

Now because ADBH is a parallelogram, we have HB= AD; moreover the forces S and R are equal, therefore

P : R : : AB : AD.

But by supposition P : Q : : AB : AC;
therefore, by uniting these two proportions, we get

P : Q : R : : AB : AC : AD.

37. *Cor.* 1. If the two forces P, Q, are applied to the point A, they will be in equilibrio with a third force applied to the same point, the direction of which is DA, and which is proportional to the diagonal AD; for this force will be equal and directly opposite to the resultant of the forces P, Q.

If the forces P, Q, are applied to any other points of their directions, there will be an equilibrium in applying to any point whatever of the right line AD, and in the direction DA, a force proportional to AD; provided that the point of application of this last force be invariably connected to the points of application of the forces P, Q.

38. *Cor.* 2. We can always decompose a force R, given in quantity and direction, into two other forces P, Q, directed according to the given right lines AP, AQ; provided that these directions and that of the force R, are comprised in the same plane, and concur in the same point A.

For let the force R be represented by a part AD of its direction, and drawing, through the point D, the right lines DC, DB, parallel to the given directions AP, AQ, they form the parallelogram ABDC, the sides of which AB, AC, represent the forces P, Q, required; for (36) the resultant of these forces will have the same quantity and di-

rection

tion as the force R. We find the quantities of the
ces P, Q, by means of these proportions,

$$AD : AB :: R : P$$
$$AD : AC :: R : Q.$$

PROBLEM.

39. To determine the *resultant* of any number of forces
Q, R, S, &c. whose directions, comprised or not com-
sed in the same plane, concur in the same point A.
DEMONSTRATION. Upon the given directions lay off,
m the point A, the lines AB, AC, AD, AE . . . . pro-
rtional to the forces P, Q, R, S . . . . respectively ; and
isidering, at first, any two se forces as P, Q, and
npleting the parallelogram C, the diagonal AF will
present the quantity and d n of the particular re-
tant T of these forces (3
Instead of the forces P we may now take their
ultant T, and considerin two forces T, R, repre-
ited by AF and AD, com e parallelogram AFGD,
l the diagonal AG will nt the quantity and di-
tion of the resultant V forces T, R, or of the
ee forces P, Q, R. Ag tead of the three forces
Q, R, we may take their nt V, and considering
two forces V, S, comple parallelogram AGHE,
diagonal of which AH resent the quantity and
ection of the resultant X two forces V, S, or of
four P, Q, R, S.
In the same manner we oceed to determine the
ultant of any number of forces whatever, and the last
ultant will be the general resultant of the system.

40. *Cor.* If all the forces P, Q, R, S . . . . are applied to
point of concourse A of their directions, to produce an
uilibrium, we must first find the quantity and direction
their resultant, and then apply to the point A a force
ual and directly opposite to it. But if the forces are
plied to other points of their directions, invariably con-
cted among themselves; to produce an equilibrium we
st apply to any point of the direction of their resultant
orce which shall be equal and directly opposite to that
their resultant, provided that the point of application of
s force be also connected in an invariable manner to
ose of the forces P, Q, R, S . . . .

PROBLEM.

41. To determine the resultant of any number of forces

P, Q, R, S

P, Q, R, S, &c. whose directions comprised in the same plane do not concur in the same point; whose points of application A, B, C, D, &c. are connected together in an invariable manner; and whose magnitudes are represented by the parts A*a*, B*b*, C*c*, D*d*, of their directions.

SOLUTION. After having prolonged the directions of any two of these forces, such as P, Q, until they have met somewhere in a point E; we lay off from E to F and from E to G the lines A*a*, B*b*, representing these forces, which will complete the parallelogram EF*e*G, whose diagonal E*e* will represent in magnitude and direction the resultant T of the two forces P, Q, (36).

Instead of the forces P, Q, we take the resultant T, and prolong its direction as well as that of the force R until they meet somewhere in a point H; we lay off the line E*e* from H to I, and the line C*c* from H to K; which will complete the parallelogram HI*h*K, whose diagonal H*h* will represent in magnitude and direction the resultant V of the two forces T, R, which will also be that of the three forces P, Q, R.

In the same manner, instead of the three forces P, Q, R, we take their resultant V, and prolong its direction as well as that of the force S until they meet in a point L; then laying off from L to M and from L to N the lines H*h*, D*d*, which represent the forces V and S, they complete the parallelogram LM*l*N, whose diagonal L*l* will represent the resultant X of these two forces, which is also that of the four forces P, Q, R, S.

By proceeding thus we may find the magnitude and direction of the general resultant of all the proposed forces, whatever may be their number.

42. *Cor.* Therefore when several forces, directed in the same plane, are applied to points connected together in an invariable manner, these forces have always a resultant; so that it is possible to make them in equilibrium by means of one force only; except in the case where the direction of one of these forces being parallel to that of the resultant of all the others, this force and this resultant are equal to each other, and act in contrary directions: for we have seen (24) that then to make them in equilibrium it is necessary to apply a force of nothing, whose direction should pass to an infinite distance; which is impracticable.

THEOREM.

43. If three forces P, Q, R, have their magnitudes and directions represented by the three edges AB, AC, AD, con-

iguous to the same angle of a parallelopipedon ABFEGD, n such a way that P : Q : R : : AB : AC : AD, their re- ultant S will be represented in magnitude and direction by the diagonal AE of the parallelopipedon contiguous to that ngle, and we shall have P : Q : R : S : : AB : AC : AD : AE.

DEMONSTRATION. On the face ABFC, which contains the directions of the two forces P, Q, let the diagonal AF be drawn; let the diagonal DE also be drawn on the opposite face DHEG; these two diagonals are parallel and equal, for the two edges AD, EF, of the parallelopipedon, at the extremities of which they terminate, are parallel and equal; therefore AFED v a parallelogram. Hence the two forces P, Q, bein sented in magnitude and direction by the sides AB, f the face ABFG, which s a parallelogram, their re T will be represented in magnitude and direction b iagonal AF, and we shall have P : Q : T : : AB : A.

In like manner the tw T, R, being represented by the sides AF, AD, of allelogram AFED, their resultant S, which is also the three forces P, Q, R, will be represented by th al AE of the same parallelogram, and we have T : AF : AD : AE; therefore, by uniting the abov series of proportionals we have P : Q : R : S : : AB : AD : AE.

Now the diagonal AF wise that of the parallelopipedon; therefore th nt of three forces will be represented in magnitude ection by the diagonal of the parallelopipedon.

44. *Cor.* We may always decompose a force S given in magnitude and direction into three other forces P, Q, R, directed according to the three given lines AP, AQ, AR, not comprised in the same plane, provided these three directions and that of the force S concur in the same point A.

To effect this, by the three directions considered two and two we draw the three planes BAC, CAD, DAB; the force S will be represented by a part AE of its direction; and by the point E we draw three other planes, EGDH, EHBF, EFCG, respectively parallel to the three first; these six planes are the faces of a parallelopipedon, whose diagonal is AE, and whose edges AB, AC, AD, which are taken on the three given directions, represent the magnitude of the forces required, P, Q, R; for (43) the resultant of these three forces will have the same magnitude and direction as the force S.

Otherwise, we draw through the point E three right lines parallel to the directions AP, AQ, AR; and the parts EF, EH, EG, of these right lines, comprised between the point E and the planes BAC, CAD, DAB, represent the magnitudes of the required forces P, Q, R; for these lines being three edges of a parallelopipedon, they are respectively equal to the other edges AB, AC, AD, which are parallel to them.

LV. *Description of the French Telegraphs used on the Coasts of Flanders, &c. with Observations on the same, and a Plan of a Polygrammatic Telegraph on a new Construction. By Capt.* C. W. PASLEY, *of the Royal Engineers*[*].

HAVING last year had an opportunity of observing the nature of the French telegraphs used on the coast of Flanders, of which no account has, to my knowledge, been published in this country, a description of them may perhaps be acceptable to the public; particularly as I am led to believe that they have been very recently invented, or at least established, and that they are now in general use throughout the whole extent of coast of the French empire. Various naval officers, whom I have consulted with a view of obtaining information on the subject, agree, that it is only a few years since these telegraphs first made their appearance; but I have not been able to ascertain the precise period. It does not seem, however, from what I have learned, to be more than three years ago, since the system of signals by masts, yards and balls, which I know was formerly used on many parts of the enemy's coast, was abolished, in order to make room for this new establishment.

Every telegraph consists of one upright post, to which are attached three arms exactly similar to each other, moving each upon its own distinct spindle or axis. The axis of one of these arms is near the head of the post. The distance from the centre of motion of each of the two uppermost arms to that of the one immediately below it, is rather less than double the length of one arm. The highest of the three arms (A) can exhibit seven distinct positions, but the other two arms (B and C) can only exhibit six positions each. The total number of combinations, or of distinct signals, which can be made by this telegraph,

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[*] Communicated by Capt. Pasley.

legraph, will consequently be three hundred and ninety-ne*. As only three bodies are employed in the French legraph, it may therefore appear superior to the Admiralty legraphs used in England, which by the combination of ouble that number of bodies can only make sixty-three istinct signals.

The mechanism of the French telegraphs just described, ust either be imperfect, or the men employed in working em must have been very unskilful, for the signals were ade and repeated in an awkward manner, with, what emed to me, much unnecessary loss of time; but these efects, it will be evident, detract nothing from its merit an invention. In regard mechanical construction, could only observe that th ms, which are painted lack, and appear solid at a dist , are made in the fashion f a Venetian blind, in orde ay be presumed, to diinish the action of the wi ad weather. Each arm as a counterpoise of thin ls painted white, which, nless the observer is very telegraph, becomes insible.

In the annexed Plate, fig ows the telegraph in a ate of rest, the dotted line ing the several positions which the arms can be e d.

Figs. 2 and 3 are a specim the telegraph at work.

Fig. 4 shows the construc f one of the arms on a rger scale, DE being the hich is fashioned like a enetian blind, and EF the rpoise.

From the above descriptio l appear, that the French ave adopted, in the arms of their new telegraph, the same inciple of motion used in the polygrammatic telegraph vented by me, of which an account is contained in the xth volume of this work; the only difference being, at in my telegraph two arms are placed on the same axis, stead of one, to which they have confined themselves.

Instead of using several posts, which appeared to me e most eligible mode, a polygrammatic telegraph may so be constructed upon one post. Fig. 5 shows how is may be done, on comparison of which with my former vention, (fig. 6,) it will be seen that the same signals can some cases be made by both.

The disadvantage attending a polygrammatic telegraph constructed upon one post instead of several, is, that in the former the combinations are more limited in number. The signal made at A in fig. 5, for instance, cannot be made either at B, C, or D in the same figure, although it may be made on all the posts A, B, C and D in fig. 6. Hence, whilst each post in fig. 6 can make twenty-eight distinct signals, that number in fig. 5 is limited to the part A alone, the signals that may be made at B, C or D, in fig. 5, being only twenty-one.

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* A naval officer who has been lately employed in the Mediterranean orms me, that he does not believe that the arm A is ever shown in its rth position. I think I have seen it myself in that position, but am not tain. If the French confine themselves to only six positions upon that n, the total number of signals will be reduced from 391 to 342.

A polygrammatic telegraph upon one post, on the principle shown in fig. 5, may be thought, however, sufficiently powerful and copious.

If four pair of arms are used, the total number of distinct signals that may be made by the parts A, B, C and D combined, will be no less than 308,791.

If only three pair of arms are used, in which case the upper part of the telegraph may be supposed cut off at the letter B in fig. 5, the number of distinct signals that may be made by this kind of telegraph (by the various combinations upon the parts B, C and D) will be 13,935.

If we suppose all the upper part of the telegraph to be cut off a little above the letter C in fig. 5, so that only two pair of arms are used, the number of distinct signals that may be made by this telegraph (by the combinations upon the parts C and D alone) will be 637.

Hence, even in this reduced state, my polygrammatic telegraph, whether constructed upon one or upon two posts, will preserve a considerable superiority over the French as well as over the British Admiralty telegraphs; and it may perhaps be allowed, that, in all cases, it will be perfectly clear, and as little liable to mistake, as any other telegraph that has been invented.

A disadvantage attends the polygrammatic telegraph upon four posts, in the form in which it was originally published (fig. 6), arising from the great space upon which the posts must stand; so that, if the arms are supposed to be six feet long, (measuring from their centre of motion,) the distance AD in that figure can hardly be less than forty feet. This disadvantage will be done away by making the posts of unequal heights, in the manner shown in fig. 7. By this method, the arms being still supposed six feet long, the distance AD may be reduced to twenty-two feet; so that the telegraph may be conveniently fitted to the roof of the smallest building.

C. W. Pasley,
Captain Royal Engineers.

VI. *On Crystallography. By M.* Hauy. *Translated* 'rom *the last Paris Edition of his* Traité de Minéralogie.

[Continued from p. 277.]

OF THE CHARACTERS OF MINERALS.

Ve understand bv the term *characters* of a mineral, ery thing that can be the subject of an observation proper r making it known. We could not refrain, in treating 'the mineralogical methods in the preceding article, from ving an idea of the characters which are the soul of the stem. But it is necess [illegible] ıter into more extensive :tails on this important s[illegible]

If we consider the ch[illegible] relative to the various anches of science which [illegible] them, we must distin-ıish them as physical, [illegible] al, and chemical cha-cters.

The physical characters [illegible] ose the observation of hich produces no remar[illegible] nge in the state of the bstance which presents [illegible] r with respect to which is change is only a con[illegible] essary for observing an fect which in other res[illegible] ongs to physics. Thus e phosphorescence prod[illegible] rowing the dust of a ineral upon burning coa[illegible] gh it occasions an alter-ion in the state of the mi[illegible] be a physical character, ce that which arises fr[illegible] mutual friction of two eces of quartz. In cas[illegible] s kind, where physics ıd chemistry are so clo[illegible] d that it would be dif-ult to discern their respective limits, we have had it ırticularly in view to preserve the analogy of the cha-cters, by bringing together those which give rise to ob-rvations of a similar nature.

Properly speaking, we ought to denominate as geome-ical characters those only which are drawn from the de-rmination of the primitive forms, and from the measure-ent of the angles which form by their meeting the faces 'the crystals and the sides of these same faces. But we ıve thought it right to give to this character a greater tent than that which seems to agree with it when we ke it in a rigorous sense, and to include within it every ing which has a reference to the configuration; such as e aspect of the fracture, which sometimes forms con-xities and concavities, and sometimes presents points or perities, &c. Besides, we consider, independently of this pect, the direction in which the fracture takes p[illegible] nich is sometimes longitudinal, or parallel to the crystals, sometimes transversal, or perpendicular to the same axis, which also tends to refer it to the geometrical characters. Perhaps it may be said that it would have been more agreeable to change the word *geometrical* into another, which would have indicated in a looser manner the modifications depending on the configuration. But this word is so well adapted to those of *physical character* and *chemical character*, that we have preferred preserving it, by explaining the signification which it ought to have in the language of mineralogy.

The chemical characters are those which are proved by the decomposition of a mineral, or a sensible alteration in its nature, or a rupture of aggregation between its molecules. Such are the characters which are drawn from the action of the acids, from fusion with or without addition, by the intermedium of the blowpipe, &c.

It is from the assemblage of these three orders of characters that the character will be formed which we call *specific*, or that which will serve to distinguish all the bodies comprehended within one and the same species. We ought not to be afraid, from the reasons which we have already given, of multiplying the particular indications of which it is the assemblage, in order to procure the facility of making them serve for mutually verifying each other, or even of substituting the one for the other.

But is it not also an inconvenience, that the table of the characters of a mineral is so overloaded, that we are obliged to run over the whole of it without fixing on any thing which can give a precise knowledge of this mineral, and assist the mind in representing it as it were in miniature? It is with a view to obviate this inconvenience that I have adopted a character which I call *essential*, and which is composed of the smallest possible number of particular characters taken from among those of the species which are proper for distinguishing the latter from all others. Thus the essential of the telesia consists in its having a specific gravity of about 4, and presenting joints only in a direction perpendicular to the axis of the crystals;—that of the chabasy consists in its dividing into a rhomboid a little obtuse, and it melts easily in the blowpipe:—that of borated magnesia consists in the crystals of this mineral being electrical by heat in eight points opposed to each other in pairs;—that of sulphurated molybdenum is to leave metallic traces on paper, and to communicate electricity to resin by friction. The characters which compose what I call *essential*, will not be observable in all cases: but it will be always orrect to say that they belong exclusively to a certain pecies of mineral, in such a manner that the idea which hey originate will be the faithful representation.

The character in question will be placed at the head of hose which compose the specific character. It would, perhaps, be pushing the matter too far, to require that it hould always distinguish precisely the substance to which t is applied, not only from all those of the same class, but n general from all the minerals. It would seem to be llowable to understand the classical name from its repreentative (*enoncé*), in such a way as to form with this name he entire definition of the substance for which it has been hosen. In this manner we define telesia, an earthy subtance with a specific gravity of about 4, and which only presents well defined joints perpendicularly to the axis of ts crystals. We shall see, however, that in a great number of cases the essential character taken by itself gives an exclusion to all the minerals different from that which it deignates.

To conclude:—I do not flatter myself with having always succeeded in making the best possible choice of the haracters which ought to form that which I call *essential*; nd they will be found sometimes a little vague, when they efer to substances of which we have as yet but a slight otion. Time will add to our stock of knowledge, and his will serve to give more edge to those parts of the picure which are too feebly marked in the present state of cience.

Still, however, this was not enough; and one of two hings may have happened. Either the observer, who wished to determine a mineral, would proceed straight forward to the species of which this mineral formed a part, nd then he would have nothing else to do but to consult he essential and specific characters, to ascertain that he was right; or, deceived by a false resemblance, he would be ed to a foreign species. To bring him back to the right path in this last case, we have added, at the end of the pecific character, another which we call *distinctive*, composed of the principal differences which may enable us to pick out a mineral from among those with which we should be tempted to confound it.

We have also placed at the head of each class a general view of the substances which it contains, with the enumeration of the characters, the assemblage of which may serve o distinguish this class from the others; and we have endeavoured to restrict these characters, so that there may not result a picture too much overcharged. Our method being founded on analysis, it is only as it were by accident that certain divisions suit an assortment of characters which most frequently vary in a respect quite different from that to which the combination of the component principles is subjected. After all, if we examine the systems in which arbitrary terms most prevail, those in which the characters themselves have led the way for the distribution of bodies instead of following it, we shall perceive that it will often happen that general divisions are therein clearly circumscribed. Almost continually we find substances emerging from the limits within which they were supposed to have been confined. The important point is, that the species are well determined; because, as we have remarked, the number not being considerable, it is much easier to study the system, and to render it always sufficiently present to our minds, to apply it easily as occasion may require, particularly when on the one hand the progress is traced according to fixed principles, which second the efforts of the memory by connecting it with the understanding, and when on the other hand the methods which it employs for characterizing the bodies belong to interesting observations or experiments, which leave upon the mind durable traces of what has once spoken as it were to the eyes.

We have collected under one and the same point of view the principal characters which may serve for the description of a mineral, and we have formed a table of them, which will be found prefixed to the plates. We have arranged this table according to the methodical order of the different branches of knowledge to which it refers; this order having appeared more favourable for assisting us in seizing the whole at a glance, and to render it present to the memory.

We shall here subjoin a series of annotations, intended to give a more developed idea of certain characters, or details relative to the method of verifying those which, in order to become evident, require experiments.

These annotations will be followed by several distinct tables, which will present successively the indication of the specific gravities of minerals reduced to their limits, that of their hardness, the enumeration of the substances which possess double refraction, of those which are electrical by heat, of those which have for their primitive form a rhomboid, an octahedron, or a solid of another kind, &c. These various tables will serve as a kind of supplement to the system, particularly with respect to the second class, which

has remained without subdivisions; or rather they wil.
by themselves a kind of system, which will have the
tage of multiplying the points of view under which
als may be regarded.

OTATIONS RELATIVE TO THE GENERAL TABLE OF MINERALOGICAL CHARACTERS.

Physical characters.

Specific gravity. Let us suppose a series of bodies
fferent natures, which have equal volumes. If we
all these bodies successively by means of common
, it will be necessary, in order to establish the equili-
, to employ weights more or less considerable, ac-
ng as these bodies are more or less dense. Let us
se, moreover, that, having taken as a term of com-
n one of these bodies, for instance the lightest, we
ent its weight by unity, and we express the weight of
other bodies by numbers proportional to that unity.
hall have the relations between the weights of the
nt bodies of equal volume, or the specific gravities
se bodies.

the hypothesis, that all the bodies of which we
propose to determine the specific gravities have equal
es, not being capable of being realized, it will be-
necessary to seek for another method in order to at-
ne same object. We might succeed equally well, were
estimate exactly the volume of each body; after
it would be easy to bring the results of the different
ings to what they would have been on the hypothesis
re having been an unity in the volume. But as this
d presents obstacles which are insurmountable in
ce, we supply its place by an ingenious process, which
ts in seeking for the relation between the weight of
ody when weighed in the air, and the loss of weight
appears when the same body is weighed in water,
we here suppose to be respectively lighter than it. This
roceeds from the effort made by the water to sustain
dy in part; and this effort being equal to that which
cised in order to keep in equilibrium the volume of
me liquid displaced by the body, it results that the
question represents the weight of a volume of wa-
ual to that of the body. We have therefore the re-
between the weight of the body and that of the
in equal volumes; and this liquid thus serves as a
on measurement, in order to compare with each
the specific gravities of the different bodies.

It is in this way that the tables of specific gravities published by several authors have been made up. Of these the table of M. Brisson is the amplest and the most correct.

We employ in these experiments the *hydrostatic balance.* The body to be examined is suspended by means of a horsehair to a small hook under one of the scales, which procures the facility of plunging this body into water in order to weigh it.

Nicholson has suggested in these experiments the use of an areometer of tinned iron, represented in fig. 75, and the stalk of which, B, is a brass wire, which has at its extremity a small cistern A. This stalk is marked in the middle by a line *b* made with a file. The lower part keeps suspended an inverted cone EG, concave at its base, and balanced within by a piece of lead*. The weight of the instrument ought to be such that, when we plunge it in water and leave it to itself, a part of the tube floats above. The cistern at the top of the stalk, and which has the form of a spherical shell, is fixed to it by means of a small tube of tinned iron into which this stalk enters. Generally there is a second cistern somewhat larger, which is placed above the first, into the concave part of which it is fitted, in consequence of its convexity. We may thus move this second cistern, either to remove more easily the weights with which it is charged, or to make some change in their arrangement. This instrument, which is very cheap, and easily carried from place to place, is extremely useful to mineralogists. An example will best explain the method of using it.

If any doubts are entertained whether a transparent stone of a blue colour belongs to the stony substance commonly called *oriental sapphire*, or to the variety of quartz which is called *water sapphire* :—Take distilled water at a given temperature; Brisson has adopted that of 14° of Reaumur, which answers to 17·5° of the centigrade thermometer, as the medium in our climate. Having plunged the areometer in this water, charge the upper cistern A, until the scratch *b* marked on the stalk descends to the level of the water. This is called levelling the areometer.

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* In several areometers this cone has a fixed position, by means of brass wires which keep it attached to the instrument. But M. Gillet with good reason prefers giving it its liberty, by suspending it with a hook, as is represented in the figure. In this way the axis of the instrument always takes a vertical direction, otherwise it would lean more to one side than to the other, when the instrument would be in equilibrium round its centre of oscillation.

eometer*. Let us suppose that the weights employed rm a sum of 20 grammes. This is the charge of the eometer, which can only serve for bodies the weight of hich does not exceed 20 grammes.

Having taken out the charge, put the stone into the same stern, and place beside it the weights necessary for level- ıg the areometer. Supposing these last weights to be uivalent to 13·6 grs. subtracting this number from 20, e shall have 6·4 grs. for the weight of the stone in the air.

Withdraw the areometer, in order to place the stone in e lower bason E; then having redipped the instrument, ld into the cistern A the weights necessary for producing e level once more. Let uppose these additional eights to be equivalent to grs. This is what the one has lost of its weight i water, and at the same me is the weight of an equ ıme of water.

Make this proportion: grs. or the weight of e volume of water equal t of the stone is to 6·4 s. the absolute weight of ne, as the unity which presents in general the spe gravity of the water is to fourth term, which will l specific gravity of the one†. This fourth ter n with four decimals is 5806. Now on running he table of specific gra- ties, we find that that of w pphire answers to nearly e same number, while t the oriental sapphire is out 4. The stone subject the experiment is there- re a quartz only.

If we wished to weigh a ance respectively lighter an water, it would be necessary, on placing it in the wer basin, to fix it steadily to it. In this case the body hich serves to fix it is considered as forming part of the eometer. In other respects the operation is the same as the preceding case; only the second term of the pro- ortion is smaller than the first, which is necessary; since e fourth term, which gives the specific gravity of the ody, ought to be also smaller than the third, which repre- nts the specific gravity of the water.

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* We may fairly dispense with this operation, because we are under- ood to know before-hand, from a prior experiment, the weight neces- ry for levelling the areometer: it is best however to repeat this ope- tion every time, on account of the small differences which may happen in e temperature, or in the quality of the liquid.

† It is more natural to employ unity in order to designate the specific avity of the water, which is the term of comparison to which we refer all e specific gravities of other bodies, than to represent it by 1000 or by),000, as is generally done. To conclude, the calculation is the same, ccept that we have generally a decimal fraction in the result.

Let us suppose, for example, that the absolute charge of the areometer, including the body which ought to serve as the means of fixing the substance, being also 20 grammes, we must have been under the necessity of placing 16 grammes beside the body which we wish to weigh, in order to produce the levelling once more. We shall have four grammes for the weight of this body. Let us afterwards suppose that, the same body being dipped in water, we had still added six grammes to the 16 which were already in the upper cistern, which makes in all 22 grammes. These six grammes will represent the weight of the volume of water displaced, and the proportion will be $6 : 4 :: 1 : x$, which will give 0·6666 for the specific gravity of the body subjected to the experiment.

In effect, if the weight of the body of an equal volume was exactly the same with the weight of the water, it would be necessary to charge the upper cistern with 20 grammes only, as at first, when the body would be plunged in the water, because it would perform the office of the volume of water displaced. But we have seen that in this case the upper cistern was charged with 22 grammes, from which it follows that there remains to the water an effort of two grammes, besides that of four grammes, which it employs in sustaining entirely the body. The total force therefore of the water is equivalent to six grammes; or, what comes to the same thing, the weight of a volume of water equal to that of the body is six grammes. Thus the specific gravity of the water is to that of the body in the ratio of six to four, as we have seen above.

Water always has a trifling adherence to the areometer, which is such that this instrument loaded with the same weight may remain a little more or a little less deeply plunged in the water. In order to get rid of the trifling uncertainty which arises from this variation in the level, having allowed the areometer to attain the state of stability, raise it a little above its position, and afterwards sink it a little below it by abandoning it every time to itself; and if the scratched line is between the two points which are levelled, you may conclude that the upper basin has its true charge.

We may instead of distilled water employ rain-water, which at the same temperature has visibly the same density. In the event of our only wishing to dispel a doubt whether a mineral might be referred to one species more than to any other, we should have a sufficient approximation on operating with river- or well-water, the temperature of which should differ in a few degrees only from that which had been chosen for arranging the table of specific gravities. If we should desire, however, a greater precision, we might attain it by the method which we shall indicate, in order to assimilate the weight made with any liquid at any given temperature, to the result which would have been given by distilled water at 14° of Reaumur.

Having taken preciſely the absolute weight of the areometer, which will be, for example, 152 grammes, and knowing the additional weight supposed to be 20 grammes necessary for levelling it in distilled water at 14°, we shall have for the sum of these two weights 172 grammes.

Let us now suppose that the additional weight which produces the levelling with another liquid is 20·5 grs. the sum will become 172·5 grs.

Now we know that when a body floats in part, the weight of the volume of the liquid which answers to the part immersed is equal to the total weight of the body. Thus, since the part immersed is the same in both cases, it results that the weights of the two liquids when of equal volume, or what comes to the same thing their specific gravities, are in the ratio of 1720 to 1725.

This being done it is evident at first sight that the liquid substituted for distilled water immediately gives the absolute weight of the body under examination, without any correction being required. Let this weight be eleven grammes. After having found by a second operation the quantity which the body, weighed in the liquid which you employ, loses of its weight, and which we shall suppose to be 4·7 grs., make this proportion, 1725 : 1720 : : 4·7 : a fourth term which will indicate the loss corrected, or that which the body would have undergone of its weight in distilled water at 14° of Reaumur. This loss, which will be found to be 4·69, will give at the same time the weight of the volume of distilled water at 14°, equal to that of the body; after which you will make this other proportion which returns to that above indicated 4·69 : 11 : : the unity is a fourth term, which will be 2·3454, and which will indicate the true specific gravity of the body. By employing no correction, we should have found 2·3404.

There are substances which being immersed in water drink up this liquid. Of this number is the mesotype (zeolite of Cronstedt). We perceive this property when, having placed the substance in the lower basin E, we see the areometer descend, after having mounted up, although the cistern A remains loaded with the same weight. In this case we shall allow the body to imbibe the whole quantity of water which it can admit into its pores, and we shall know that it has reached this kind of point of saturation when the areometer remains in a fixed position. Then we shall level it; and we shall in general seek for the loss which the body has undergone of its weight in the water. We shall afterwards seek for the weight of the quantity of water which it has imbibed, on weighing it again in the air, and in subtracting the first weight from the second; then we shall add the difference to the loss formerly found: and the result will give the true loss, or that which would take place if the body was not susceptible of imbibition; after which we shall form the proportion indicated above.

Let us suppose, for example, a mesotype, the weight of which in the air is nine grammes. Let us suppose that the loss of weight which it experiences in the water, after imbibition, is 4·3 grs. Let us suppose in fine, that being again weighed in this state, it gives 9·13 grs. Subtracting the first weight from the latter, we shall have 0·13 grs. for the quantity of water which the mesotype has imbibed. The real loss, or that which the substance would have experienced of its weight in water, if it was not penetrable by this liquid, will therefore be 4·3 grs. plus 0·13 grs. or 4·43 grs.; which gives the following proportion, 4·43 grs. : 9 : : 1 : x. From which we shall conclude that the specific gravity is 2·0316.

In fact, since bodies lose less of their weight in water in proportion as their absolute weight is more considerable, it results that the mesotype ought to have lost in the water 0·13 grs. less than if the imbibition had not taken place. We must therefore add 0·13 grs. to the loss found by experiment, in order to have the loss corrected.

The character which is drawn from the specific gravity joins to the advantage of a great generality that of being susceptible of a precise estimation, provided we do not employ too small pieces of any substance. Its limit relatively to each mineral is the result of the operation made on a piece chosen in the greatest possible state of purity. It may vary beyond this limit on account of some colouring principle of a metallic nature; or within this limit, by the effect of the mixture of a substance less dense, or whose presence relaxes the aggregation of the molecules. Thus the specific gravity of the limpid hyalin quartz, called *Madagascar crystal*, is 2·653; that of the red hyalin-quartz is 2·6701: and that of the dull quartz is only 2·6459. The more do the limits relative to the species between which it is requisite to determine escape being confounded, the more decisive is the proof of the character.

2. *Hard and soft bodies.* The character suggested by hardness

ardness does not admit of quite so much precision as the regoing. It is besides more variable. Some particles of uartz, disseminated in a body of a soft nature, may, by ltering a little its specific gravity, make it capable of riking fire with steel. But this character has the adantage of being easy and expeditious in its application, by e help of the different methods pointed out in the table. t is proper to distinguish accurately the species whose arieties are but little subject to the alterations produced y accidental mixtures. Thus the cymophane, or the early chrysolite of the lapidaries, scratches glass, whereas e chrysolite of Romé de l'Isle, since ascertained to be a lcareous phosphate, when rubbed upon glass, frequently aves a white mark of its own substance. The ferrugiated scheelin or wolfram yields very easily to the file, hich has much less action on the blackish oxidized tin ometimes found in the same matrix, &c.

3. *Brittle bodies.* These must not be confounded with hat are called *soft* or *tender.* Talc is softer than caronated lime, denominated *calcareous spar,* since this last cratches it. But it is less brittle, as it resists percussion r better.

4. *Elastic bodies* are those which, after having been ompressed, return of their own accord to their original gure. The character suggested by this property is not nployed in mineralogy, except when speaking of bodies hich appear to us in the form, or may be easily reduced to e form, of thin laminæ or filaments, and it suits but a nall number of species.

5. *Ductile bodies* are those which are lengthened or attened by percussion or by pressure, so as to preserve e figure which they have taken in virtue of the one or e other of these two forces. Of this number are several etallic substances, gold, silver, copper, &c. Whereas hers, such as antimony, bismuth, &c. will break, rather an submit to be flattened or elongated.

6. *Bodies endowed with tenacity.* This is the property ssessed by certain substances, and particularly by the etals, of resisting without being broken the action of a rce which draws them by one extremity while they are xed at the opposite extremity. Of this kind is the restance presented to breaking by a harpsichord string which e are tuning. This property is but little susceptible of ing employed as a character, but it is right to mention, the description of a mineral, to what degree it possesses in comparison with others.

7. *Adherence to the tongue.* Certain bodies adhere to

the tongue when brought in contact with it, and a slight resistance is experienced when we separate them. This effect is produced by the faculty which the body has of absorbing the saliva which moistens the tongue, and thus bringing the substance into more immediate contact with this organ. If we put a drop of water on one of these bodies, we shall observe that it is imbibed in an instant, and this proof may suffice instead of applying it to the tongue.

8. *Colours.* In order to fix the degree of confidence which these characters deserve when borrowed from this modification, too much neglected by some, and overvalued by others, we ought to consider it in two very different points of view, according to the various natures of the bodies which are furnished with it. In a certain number of these bodies, and in particular in the earthy and acidiferous substances, the colours are owing to the molecules of a foreign principle, which is frequently iron, and sometimes chrome or manganese, disseminated among the molecules peculiar to the coloured body. Hence it happens that one and the same substance, for example fluated lime, is colourless in certain pieces, and in others presents alternately the red, yellow, green, violet, &c. In this case the colour is only a transient accident, which may merely serve to distinguish certain varieties.

But in other minerals, such as metallic substances, sulphur, amber, some saline substances, the reflection of the rays which produce the colour is made upon the proper parts of the coloured body; it depends on its texture and on the degree of the tenuity of its molecules. It may then be ranked among the specific characters.

We sometimes find muriated soda coloured red. If you dissolve it in this state, it will be stripped of its colouring principle, and the new crystals which it will form will no longer reflect any thing but white light. The operation only frees it from a superfluity, without which it does not cease to be of the same nature. On the contrary, sulphated copper subjected to the same experiment, as often as we please, will always reappear blue, because this colour is inherent in its nature.

Thus those who have said that the true colour of the spinel ruby, for instance, was red mixed with orange, have only designated the stone which pleases amateurs most. But to say that the true colour of gold is pure yellow, is to speak the language of the naturalist. If this colour does not uniformly exist in gold, this must arise from the presence of a foreign substance which has altered the metal itself.

I have met with naturalists, however, who, admitting the variations to which the colour is subject, with respect to one part of the minerals, do not warrant us in attaching much importance to the character which it furnishes; as much because it is that which first speaks to the eyes, as because there is always, according to them, relatively to each substance, a colour which is predominant, and which agrees with the greatest number of varieties. But the more the observations shall be multiplied, the more frequently will it happen that this character will not speak to the eye, except to deceive it and make it take the change. No further proof of this is requisite than what happens with respect to the emerald. Grass-green seems long to have been ranged among the general characters of this substance, and, in short, it is not astonishing that every thing which was emerald was of the same colour. We were not acquainted with any thing else, properly speaking, under this name, than the crystals from Peru, which being formed in the same circumstances had received the impression of the same colouring principle. A discovery in which mineralogy and chemistry have concurred, unites the beryl with the emerald; and from this moment we have emeralds, some greenish-yellow, others blueish, and others of a decided yellow; and the number of the crystals of these different tints, particularly of the first, which exist in our collections, far exceeds the ancient emeralds. The jargon of Ceylon identified with the hyacinth, according to the analysis of Klaproth, has also disturbed this last substance in the place it occupied, namely, that of being exhibited of an orange brown colour only, excepting in one variety which was whitish: and it would be as easy to adduce other examples of this kind [*], as it is to foresee from the moment that these examples will still continue to be multiplied. The indication of the colour, in the case of the latter being owing to an accessary principle, ought therefore to be dismissed from the specific character; and we shall have a new reason to exclude it, if we consider that every thing which enters into this character ought to be so much the more taken into the account, the purer the substance is, or the more it approaches the limit which really constitutes its species: and that in the case of this limit the colour would disappear*.

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[*] Launoy brought from Spain several small orange crystals which belong to phosphated lime, known in Germany by the name of *spargel-stein*, because the colour of the variety of this substance found at first in the same country inclines to that of asparagus. Messrs. Abildgaard and Manthey have since given me some crystals of this substance which are frequently met with in certain granitic rocks of Norway, and the colour of which is sometimes of a greenish blue, sometimes brown, &c.

As to the diversities of which the character is susceptible that is derived from colours, it would be superfluous to enumerate them, because we find on this head in daily observation, and in the commonly received language, every thing that can be required by science and its nomenclature. Thus, in order to designate any given shade of colour, sometimes we add a simple adjective; as when we say *pure green, bright red, dark blue*: sometimes we refer the colour to a term of comparison taken from among familiar objects; as when we say *sky blue, saffron yellow, leek green,* &c.: on other occasions we give the two colours of which the object in question seems to partake, and say, for example, *greenish yellow*, or *yellowish green*, by calling in the predominant colour first.

9. *Cat's eye colours.* This expression alludes to the eyes of a cat which shine in the dark. We say of a substance that it is cat's eyed, when, in proportion as we vary the position of its surface, the reflections of light which it gives off are in some measure moveable, or appear and disappear alternately.

10. *Metallic lustre.* We may distinguish the true from that which is apparent only, in so far as the mark of a file or any sharp instrument with which we have scratched a metal does not cease to shine, whereas it is dirty, and as it were powdery, when the body is not of a metallic nature.

11. *Limpid bodies.* We have given the name of *white* to diaphanous and colourless minerals properly so called, such as the quartz called *Madagascar crystal.* We shall call these *limpid* minerals, and reserve the deno-

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* We must confess that there would have been some advantage in quoting at the head of the description of a species, the character derived from the modification in question, if the greater number of the varieties presented one and the same colour, or nearly so, in such a way that the differences which would have taken place in other varieties might have been regarded as exceptions; because, this character being that which first strikes the eye, its indication would thereby be very proper for becoming as if the first stroke of the pencil in giving the portrait of a mineral. But if we are obliged to quote at once eight or ten different colours, which are shared by various individuals of the species, will it not appear that the description commences by falling into absurdity, and by failing in its principal object, which is to admit of a facility of ascertaining at one glance the substance indicated?

mination of *white* for those which reflect without order the assemblage of all the colours, like statuary marble.

12. *Double refraction.* When a ray of light passes obliquely from one medium into another of a different density, it is diverted from its route, forming a kind of fold. This deviation, which we call *refraction,* is subjected to a constant law which is known to all naturalists.

Certain substances have the singular property to solicit the ray which penetrates them to divide itself into two parts which follow two different routes. This is called *double refraction.*

When the refraction is simple, we only perceive a single image of an object seen through two faces of a transparent piece of the mineral employed on this occasion, whereas, if it were double, we might in the same case see two images of the object. But in order to obtain this effect with most of the substances endowed with the property in question, we must choose two faces inclined towards each other, whether we employ a crystal given by nature or a piece cut by the lapidary.

The quantity of double refraction, or, what comes to the same thing, the opening of the angle formed between each other by the rays, by means of which the eye sees the two images, varies from one substance to the other, every thing else being considered according to the nature of the substances themselves. In zircon, for instance, the double refraction is very strong, whereas it is much less perceptible in the emerald. Besides, this quantity varies in every substance, from various causes. In general it increases or diminishes, according as the refrangent angle, or that which is formed between each other by the two faces, through which we view objects, is more or less open. But there is another cause of variation, which is combined with the foregoing, and which depends on the position of the refrangent surfaces relatively to the faces of the primitive form; and such is the influence of this cause, that under two equal refrangent angles differently situated, we may have distances evidently unequal between the images of the same object, and there is even a limit at which the effect of the double refraction becomes null, *i. e.* the two images are then confounded into one.

This limit takes place, for instance, in the quartz and in the emerald, when one of the faces which belong to the refrangent angle is perpendicular to the axis. It takes place in sulphated barytes, when one of the same faces being parallel to the axis, is at the same time parallel to a plane which should pass by the great diagonals of the bases of the primitive form. I have but a very few observations on this subject as yet; but it is probable that all the substances which have the double refraction fall within one or other of the foregoing cases, which give of themselves the limits of all the positions which refrangent surfaces may have relatively to the primitive form. But as the position parallel to the axis is variable in its turn among several limits, which correspond with the diagonals and the sides of the bases of the primitive form, it will be requisite to know which of these last limits is that which agrees with every substance.

I shall explain, when I come to the article emerald, how a mistake led me to these results; and I even confess that I am still in uncertainty as to the refraction of some substances, not having had time sufficiently to multiply my inquiries, in order to ascertain if a crystal of this description which presented only a single image of the objects, would not exhibit two after having been cut in a certain manner.

I shall detail, in speaking of every substance, what I have observed with respect to its refraction, and I propose to make some fresh experiments on this delicate point connected with minerals, which I have been only able to glance at as yet. We shall find, under the article carbonated lime, the detail of the particular results which I attained relative to the double refraction of this acidiferous substance, which suits more easily than the others this kind of inquiries.

Another observation, which will not be altogether useless when we are occupied with generalizing the theory of the phænomenon in question, consists in this—namely, that all the substances in which the integrant molecule is remarkable for its symmetry have the simple refraction. Of this kind are those which have for their primitive form the cube, the regular octahedron and the rhomboidal dodecahedron.

As yet we have only subjected to the experiments which concern this object, bodies taken from among those which we commonly designate by the name of *stones*. I have extended these experiments to several of those which are called *salts*, as well as to inflammable substances, and to metallic substances oxidized and united to other principles, and I have found that there is no class of minerals which does not present bodies endowed with double refraction.

There are various ways of observing the double refracion. One of the most simple consists in taking a pin by he point, and presenting it against the window at a cerain distance from the eye, against which we shall keep at the same time the mineral applied by one of its faces. By making the pin assume various positions, we shall find hat there is one in which we see two distinct images of this pin parallel to each other, and generally prismatic *irisées**). Then, if we gently turn the pin until it is perpendicular to its first position, we shall see the two images pproach by degrees, until they fall upon one and the same ine, in such a manner, however. that one of the two heads vill frequently exceed the We may also make use of a card on which we hav a line with ink of a good tint.

The separation between s is more sensible, the listance between the obj e eye and all other cirumstances being alike, w aphanous body used in he experiment is of a gre ness. And if we suppose this thickness, in its constant, and the obect removed from the ey o images will be more nd more removed from , at the same time that hey will be diminished in ss.

The following is a third eous process for shortighted people. Place a l lle at a certain distance n a dark room. Having s made a hole in a card vith the point of a pin, o one of the faces of he stone, so as to make rrespond to a point of his face; then having approached with the eye the opposite ace, seek the position proper for enabling you to perceive he flame of the candle. You will then have the two mages distinct and well defined, because the effect of the tole made with the pin is to dismiss the kind of irradiation vhich dazzles them, when we employ the stone by itelf.

It would be difficult to find a character more prominent han that which is drawn from the double refraction, since t belongs to the very essence of the minerals in which it exists. But we cannot always observe it on taking these bodies in the natural state. Several require to be prepared for inspection by cutting. Those which are called gems, and which have come through the hands of the artist, thereby become susceptible of presenting the effect of simple or double refraction, when we know how to guard against the illusion produced by the multiplicity of the facets; and it is even an advantage to be able by the help of this multiplicity to vary the refrangent angle, because, if any one of the facets was situated in the direction of the limits where the two refactions are reduced to one only, other facets would present themselves in order to dispel the doubt. We shall thus avoid confounding a piece of crystal of Madagascar with the gem called by lapidaries *white sapphire*, their Brazilian ruby with the *balass ruby*, the topaz of Saxony with what is called *oriental topaz*; the first stone of each of the above pairs having the double refraction, while the second has the simple: it is fortunate to be able in these cases to make up for the disappearance of the crystalline forms by a physical observation, and to read in a manner into the interior of the stone, when its exterior speaks no longer to the eye.

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* When the double refraction is not considerable, it may happen that he two images touch each other. But, upon attentively examining the head of the pin, we can distinguish at this place as it were two small circles vhich intersect each other; and besides, we shall observe that the same coour which edges on one side the prismatic band reappears on the line of he middle part, where the same series recommences.

13. *Phosphorescence by the action of fire.* In order to observe this character, we must throw on red-hot charcoal a small quantity of the dust of the mineral we wish to examine. The phosphorescence in question is not simply a scintillation, like what is produced by the sawings of wood thrown upon the flame, but a mild and agreeable light similar to that of the glow-worm, the tone of colour excepted, which varies in different substances. This experiment scarcely ever succeeds except in the dark. We must also take great care to pound the mineral well, lest any decrepitation might throw the fragments into the eyes of the by-standers.

14. *Electricity.* There are three ways of exciting the electrical virtue in bodies; namely, by friction, by communication with a body already in a state of electricity, or by heat. This last method takes place only with respect to certain mineral substances.

We distinguish two kinds of electricity: the one which we call *vitreous*, and which Franklin called *positive*, is that which friction produces in glass and other vitreous substances. The second, which we call *resinous*, and which Franklin described by the name of *negative*, is that which is acquired in the same case by resin, sulphur, silk, &c.

These two electricities exercise contrary actions; so that two bodies, both of which are solicited by vitreous electricity or by resinous electricity, are repelled, whilst two bodies

odies one of which possesses the vitreous and the other he resinous electricity are mutually attracted.

Among the number of bodies capable of receiving elecricity by friction, we find some, which, after having been imply presented to the fire for a moment, or dipped into iot water, have acquired the electrical virtue. These bodies iave in this case one side solicited by vitreous electricity, vhile the side diametrically opposite gives signs of resinous lectricity.

One general observation made on such of the same bodies s are crystallized, consists in this; namely, that their forms ake from the symmetry of the ordinary crystals, in the ame manner as the parts in whi the two species of elecricity reside, although similarly situated on both sides, iffer in their configuration. The one undergoes decrements which are null on the posite part, or to which ome decrements answer whic follow another law. It esults that on a simple inspectio of one of these crystals, e may indicate beforehand the de which will give signs f vitreous electricity, and that w ch will manifest resinous lectricity.

Electricity separates the wh mineral kingdom into aree great divisions, which v nearly the methodical rder generally adopted for the lassification of bodies of his kingdom. Almost all the ubstances known by the ames of *stones* and *salts* a by friction the vitreous lectricity, provided they en certain degree of purity. he inflammable substances properly so called, with the xception of the diamond, being rubbed in the same maner, receive on the contrary the resinous electricity. The ietallic substances possess in general in an eminent deree the conducting property of electricity. A few of iem, which being mineralized approach the saline state, ich as carbonated lead, also enter into analogy with the ilts, by the faculty of acquiring the vitreous electricity by ieans of friction.

I ought to premise that we here allude to the ordinary iethods of exciting electricity; as when we employ the iction of the hand, or that of a piece of cloth. I supose also that the bodies rubbed are polished; for there e some kinds of quartz, gems, and other analogous subances, such as glass, which acquire the resinous electricity means of friction when its surface is dull.

It results from all that has been said, that the electrical operty furnishes characters useful in several respects for e distinction of minerals.

Electricity by communication, employed alone, may serve to discover the presence of a metal mixed in a considerable quantity with a stone, as takes place with the iron which enters into the composition of jaspers. In order to ascertain this character, we insulate the stone on a small stalk, so as to bring it in contact with an electrical conductor, and we judge whether the stone is electrical or not by communication, according as the touch of the finger or the ball of the discharger draws sparks from it.

The electricity by friction observed comparatively in two different stones may assist in distinguishing them from one another. The cymophane when polished but not cut, which presents nearly the same appearance with mother-of-pearl feldspar, called *moon-stone*, differs from it by the great facility which it has of being electrified by friction, whereas the same method succeeds but ineffectually and feebly on feldspar.

The simplest apparatus for experiments of this kind consists of a small copper needle *a b* (fig. 76, A) terminated by two bowls and moveable on a pivot. After having rubbed the mineral several times on a piece of cloth, we must present it to one of the bowls, and we may judge pretty nearly of the strength of the electricity, by the distance at which this bowl begins to be attracted.

With respect to substances electrical by heat, such as the tourmaline, we make use of the same apparatus when we merely wish to ascertain what they are. But it is interesting to be afterwards able to determine the parts in which the two electricities reside. To effect this take a stick of sealing-wax, to the extremity of which a silk thread is attached little better than one eighth of one inch in length: after having rubbed this stick, present by turns the two opposite sides of the substance, for example, the two summits of a tourmaline, at a small distance from the silk thread. If the summit which faces the thread be the seat of resinous electricity, there will be repulsion. In the contrary case the thread will be attracted.

We may vary this experiment, by placing the stick of wax, after having rubbed it, below one of the two bowls which terminate the needle, at the distance of about a quarter of an inch. For the greater simplicity, we may give such a height to the stand of the needle, that the stick of wax when resting by the rubbed extremity on another stick or on a glass tube placed transversely, and by the other extremity on the table which holds the apparatus, is at the distance

istance required for the success of the experiment. In
iis case, the wax acting on the bowl communicates to it
ı electricity contrary to its own; whence it follows that
e have inverse effects to the foregoing, *i.e.* the side of
ie stone solicited by the vitreous electricity repels the
eedle to which we present it, and that which possesses the
sinous electricity attracts this needle to it. This method
preferable to the first, when the electrical body is very
nall, or has but a feeble virtue.

Fig. 76 B represents the experiment described. We
iere see the tourmaline *tt'* caught by pincers supposed to
held in the hands of the observer, in such a way that
ie pole *t* is at a small from the bowl *a* of the
eedle. Cc is the stick of iich rests by one of its
xtremities on a tube of gl , and which acts by its
rt C on the bowl *a*, in to produce the vitreous
ectricity.

In what has gone before considered the effects
the action exercised on ral by another body, so
at the former may be r as passive with respect
the latter. What we *active electricity* is that
hich the mineral excites in the sealing-wax, by
eans of friction. In at the experiment may
icceed better, we must, ing heated the stick of
ax, flatten it at one end b ng it on a smooth body.
Ve must afterwards rub th end with a part of the
ineral, which is itself sm at least free from aspe-
ties; then we shall prese vax to the copper needle
ider which we have placed before-hand another electrified
ick of wax, as has been already mentioned.

Every body, the friction of which thus communicates to
ax a certain species of electricity, acquires at the same
me the contrary electricity; so that we might consider
is last electricity in preference, or, what comes to the
me thing, consider the mineral as being passive with re-
ect to the wax. But the minerals in which this experi-
ent becomes interesting being conductors of electricity,
is simpler to examine their action upon wax, either be-
use without this expedient we should be obliged to in-
late them, or because when their volume is somewhat
nsiderable their electricity, by being diffused over a large
ırface, would not be sufficiently palpable.

We have as yet but a small number of substances which
cite the vitreous electricity in wax, whereas other sub-
ances of an analogous nature produce in it the contrary
electricity.

electricity. These are exceptions as it were to the ordinary results, susceptible even on that very account of accurately designating the minerals which present them.

15. *Magnetism.* We know that two magnetic needles, when they turn towards each other their north or their south poles, are repelled; whereas there is an attraction, if the poles facing each other are one of them south and the other north. In consequence we recognize a needle, *i.e.* a piece of iron in the state of permanent magnetism, from the same side of this piece of iron presented successively to the two poles of a magnetic bar suspended freely, attracting the one and repelling the other, or *vice versâ.*

But if we employ a piece of common iron, there will be attraction in both cases; the pole nearest the iron will communicate to the part turned towards it a magnetism contrary to its own, so that there will then be two magnetic needles which will face each other by opposite poles. The magnetism thus acquired is merely instantaneous: it gives place to the contrary magnetism the moment the iron passes from the neighbourhood of one pole to that of the other, and is dissipated the instant the iron is no longer in the sphere of activity of the bar.

In experiments of this kind it is preferable to use a needle in the form of a lozenge, three or four inches long, instead of a bar, the former being more sensible. But the bar would be preferable, if it were requisite for instance to pick out some specimens of iron scattered in a pulverulent mass.

I shall show under the head of oxidulated iron, that most of the crystals, or even the rude masses of this metal, locked up in the bowels of the earth, provided they are not too much oxidized, are two magnets, but of which we cannot observe the polarity except by using a needle slightly magnetic, and this for a reason which I shall give at the same part of my work.

[To be continued.]

III. *Account of the Whynn Dykes in the Neighbourhood of the Giant's Causeway, Ballycastle, and Belfast: in a Letter to the Lord Bishop of Dromore, from* WILLIAM RICHARDSON, *D.D. late Fellow of Trinity College, Dublin*.

MY LORD, WHEN I last had the honour of conversing with you on [illegible] subjects, you were surprised when I told you that the whynn dykes, which of late have so much engaged the attention of naturalists in the western isles of Scotland, abounded on our Irish coast, and especially near the Giant's Causeway.

As your Lordship expressed a wish for further information on the subject, I promised to communicate to you such observations as I should make when I had examined the coast a second time, in order to ascertain the *facts* with the utmost precision.

Previous to my coming into a particular account of our dykes, I will take the liberty of making a few general observations on those in both countries.

The whynn dykes in the Hebrides are seen under very different circumstances from those on the northern coast of Antrim. There they are found on, and above, the surface, rising a few feet; and often serve as fences, whence they obtain their name. In this form they run northwards to the extremity of these islands, ascending and descending mountains, crossing seas; and where these are [illegible] that run into the water at one side of a [illegible] out of it at the other side, steadily [illegible] rectilineal course.

[illegible] sometimes exhibited in a very different [illegible] appearance is in the faces of our vast [illegible], where they are seen cutting vertically [illegible] of which these are composed, and [illegible] in the northern ocean.

[illegible] on these whynn dykes in the two [illegible], make our information on the [illegible] the Hebrides we are surprised at [illegible] which these mighty walls proceed,

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[illegible] the Basaltic Country in the Counties of [illegible] in our 30d vol. has excited so much attention, [illegible] an acceptable service to geologists by [illegible] previous papers by the same gentleman, published [illegible] the Royal Irish Academy. The present paper [illegible] and we see them penetrating indifferently all substances they encounter:—with us we can measure a part, and a part only, of their stupendous height, as at the Milestone 100 feet, at Port Spagna 330 feet, at Fairhead probably more; and we can observe the effect, or rather the non-effect, produced at their contacts with the different materials they meet, as they are seen in the faces of our precipices.

By Mr. Mills's account, (Phil. Trans. 1790,) the island of Lismore, entirely limestone, is crossed by whynn dykes, as is the limestone at Gartness; at Iona granite is the contiguous matter, at Juva chert, at Persabus a whynn dyke is crossed by a lead vein, and another at Glascow Beg; at the isle of Arran Mr. Jameson finds them cutting through porphyry and micaceous schistus.

With us the whynn dykes at the westward of the Giant's Causeway cut through strata of table basalt, and red ochreous matter, placed alternately; at the Giant's Causeway, and Port Spagna, they cut through strata of finer basalt, disposed in prismatic pillars; while at Fairhead they encounter new materials, to wit, alternate strata of freestone and coal.

In both countries these mighty walls are always of basalt; their general thickness is from twelve to fifteen feet, though in one or two instances they do not exceed two or three feet, and at Gartness the whynn dyke is 23 yards across; but it has not been ascertained in any instance to what depth they reach beneath the surface, even in the deepest mines.

Though the material of which these walls are composed seems to be in general the same, yet from Mr. Mills's account there are important differences between the Scotch whynn dykes; and with us scarce any two of our dykes, that are accessible, exactly (as will appear) resemble each other.

As the whynn dykes Mr. Mills observed are unquestionably basalt, he calls them all lava, and attempts to prove it by a sort of vague induction: page 75 he says, Islay whynn dykes resemble those at Ballycastle, *which take their rise in a country confessedly abounding with volcanic matter.*

Now the specimens from the Islay dykes strongly resemble (as he says) the Derbyshire toadstone, formed, as he asserts (page 98), by subterraneous fire.

Of Derbyshire I will not presume to say any thing, having never visited it; but the proof of its strata being lava rests upon the admission of Mr. Whitehurst's position, that these toadstone strata were formed by successive eruptions of a volcano at the centre of the earth, which pouring up repeated torrents of liquid lava, these spread when they approached the surface of the earth at different distances, and formed the toadstone strata.

When Mr. Mills endeavours to establish his opinions by assertions relative to my country, I will venture to reply to him.

The precipice from which the whynn dykes issue at Ballycastle, by his own account, consists of alternate strata of freestone and coal, not very like volcanic matters: and as to his positive and general assertion, that our basaltic country *confessedly* abo h volcanic matters, I must reply in his own style, posi and generally, that it does not afford a single particle lcanic matter; that I have examined this tract for r time, and probably with more attention, than an person ever did, or I would not presume to hazard t ion so confidently.

When your lordship is od as to perform the promise you made me, of s some time with me at the Giant's Causeway, you able to judge for yourself as to the truth of these ctory assertions.

The advocates for ign erations over the surface of our globe are so prejudic t it is sometimes sufficient to refute them merely te their own words. As Mr. Mills's paper is nov me, I will give your lordship an instance: He say e 98,) "In short, from the very rude and irregular a nce of the summit of the hill (Loffit hill), from its rising so suddenly from the limestone strata, and from the whynn dyke that runs through it, I am strongly inclined to believe it of volcanic origin." Now, as limestone and volcanic matters are not very congenial, and as we do not find that a whynn dyke has been met with in the neighbourhood of any volcano, I conceive, with great deference to Mr. Mills, that if he was determined to draw a conclusion from these data, it should have been a contrary one.—But it is time to proceed to facts.

The westernmost whynn dyke I have met with on our coast, is near what is called the Black Rock, at the end of the Bush Strand. The perpendicular precipice is there not very high (probably 60 feet); it is composed of horizontal strata of table basalts, separated from each other by red ochreous layers.

The dyke (which is inaccessible) is seen from the water to cut all these strata vertically, each of them being interrupted in its course by this wall, and resumed on the other side of it, precisely at the same level.

The second dyke is three or four hundred yards further on, towards the north-east; it is a much finer one, and so happily marked that it cannot be mistaken.

A solitary rock, about 200 yards distant from the main, and visible from a great part of the coast on each side, is called the *Milestone*, from its supposed distance from the Giant's Causeway, but in reality it is much nearer to it. The precipice here has considerably increased in height, being near to 100 feet, accurately perpendicular, and stratified as at the other dyke.

This second dyke reaches from the summit to the water, beneath which we can see it continued northwards, until it reaches the Milestone, which is a part of it.

Though this dyke be also inaccessible, it is plainly formed of prisms laid horizontally, and extending quite across; its thickness seems to be about twelve feet.

The strata are interrupted here, and resumed again, without disturbance, at the other side, as before; nor in either case does the slightest separation appear where these dykes meet the contiguous strata, all forming one solid mass.

The third dyke is situated near the western point of the bay, by which we begin to descend to the Giant's Causeway; of this an isolated fragment alone remains, about 100 feet long by 50 feet high; like the rest it is composed of rude prisms laid horizontally.

Our fourth dyke is at the Giant's Causeway itself; it divides vertically part of the cliff, at the foot of which the causeway is situated: and descends quite down to it.

The precipice is not perpendicular here, as at the other dykes, by which means our view of this one is partially interrupted; there is, however, enough of it laid bare to ascertain its nature beyond a doubt, and especially as it is composed of horizontal prisms, a property that seems essential to all whynn dykes.

Where this dyke divides the upper part of the columnar stratum which forms the Giant's Causeway, the basalt pillars on the west side of it have fallen from their original vertical position, until they lean forward almost horizontally; while on the east side of the wall they stand steadily vertical.

The basalt septs, which frequently divide the strata in mines, and appear to be of the same nature with our whynn dykes, are generally attended by a sinking or subsiding of the strata on one side of them, without disturbing the parallelism of these strata. This too is the case with our own whynn dykes at Fairhead; but of the six dykes at Bengore promontory, this fourth is the only one where any thing like a subsiding or depression of the strata can be observed.

This dyke is so accessible, that we are enabled to examine its material and internal construction, from which we are precluded in the former cases; the basalt of which this is composed, though contiguous to, or rather mixed with, the Causeway-pillars, is very different from the Causeway basalt; it is somewhat coarser, more granular in the fracture, and though darker than the gray whynn-stone of the Fairhead pillars, it resembles their colours, more than the fine blue of the Causeway l ısalt.

The Causeway dyke is 15 16 feet thick, sometimes quite solid, sometimes shive it is entirely composed of small trapezoidal prisms, ides about an inch each, and their axes horizonta are strongly agglutinated together; and when this attacked by the sledge, it sometimes breaks into fr composed of an accumulation of the smaller abundance of which are scattered about the foot ecipice.

The fifth dyke is at the n point of the semicircular bay, of which the Giant useway forms the western point; it is inaccessible, a sible only from the water; it cuts vertically three or fo rata of table basalt, also a great stratum of red och matter, and is then lost in the precipice *.

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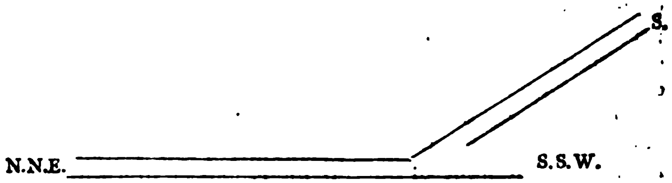
* When I discovered this whynn dyke in the year 1801, I was prevented from examining it accurately by a heavy surf, which deterred me from venturing among the sunken rocks at the foot of the precipice; the next summer I was more fortunate, and enabled twice to reach the bottom of the cliff, where the dyke immerged into the water perpendicularly.

I traced it downwards as it cut the horizontal strata of table basalt vertically, and observed each of these merging into its solid mass without any the least separation of the material; each stratum, having then as it were passed through the dyke, resumed its former position on the other side at the same level it held before, about forty yards from the place where the dyke immerged in deep water; it arose again ten or twelve feet above the surface, continuing its course due north for thirty yards, exactly like a wall, showing the horizontal prisms of which it was constructed, whose bases formed the surface of the wall.

The most curious part of this dyke is discovered by tracing it up the cliff, whose summit it reaches a little to the eastward of its original course; here it projects boldly from the face of the rock like the rectangular corner of a mighty wall about twenty feet thick: yet this curious wall is not entirely dyke, but only its west side, which, at its termination, shows the horizontal prisms composing it; the east side is formed by a range of vertical pillars fifty feet long, part of a great columnar stratum which the dyke there cuts through.

The sixth whynn dyke is at Port Spagna, the third semicircular bay east from the Causeway; this is the only one of our whynn dykes that has ever yet been noticed. Mr. Mills (Phil. Trans. 1790) saw from the top of the cliff *a kind of whynn dyke, which ran into the sea towards the N.N.E*; but he did not go down to examine it, and it is from below only that any observations can be made upon it.

This dyke runs into the sea, like a quay about 20 feet broad, formed of huge black stones; its direction near the water is S.S.W. and its two sides accurately parallel: having proceeded thus about 60 yards from the water, the eastern side deflects a little, forming an obtuse angle, while the western side proceeds further in its former direction; the breadth of the dyke thus increases for a little, but the western side is soon resumed parallel to, and at its former distance from, the other side, and the dyke proceeds now due south: all this is best explained by a figure.



The dyke, after having proceeded a short way in its new direction,

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The upper surface of this tremendous wall is easily approached from the top of the hill, and covered with high verdure. I have frequently dined upon it, as fortunately the surface is hollow in the middle, by which the dread of a perpendicular precipice, above 200 feet high, (and on three sides not more than eight or ten feet distant,) is considerably abated; the height of the point of the wall from the sea immediately under it is 320 feet.

I dwell upon this dyke both because it is so easy of access from above, (for even carriages can drive to the edge of the cliff,) and also because it is so happily marked as not to be mistaken: it forms the middle point between the Giant's Causeway and the solitary pillar called the Chimney, or, in other words, the common horn of the two crescents or semicircular bays next to the Causeway on the east side.

I will add an account of another dyke lately discovered by my friend capt. R. O'Neil: it is situated 3 or 400 yards N. W. from the beautiful villa called Seaport on Port Ballinstay, a mile and a half west from the Giant's Causeway.

The face of the precipice here seems about 50 feet high, composed of horizontal strata of coarse basalt or trap, abounding with zeolite, and of a reddish tinge, friable, and decomposing; all these strata, from the summit to the sea, are cut through obliquely at an angle of about 45 degrees, by a dyke of sound blue basalt, very fine at its edges, but coarser in the middle, and nearly five feet thick: the fine basalt of this dyke and the coarse trapp of the strata, notwithstanding the difference of their grain, unite solidly on both sides of the dyke: this important fact is more easily ascertained here, than in any other dyke I know, it is so accessible. I must observe, that this dyke is not accurately rectilineal.

direction, is lost under the rubble that has fallen from above; but whenever the precipice becomes perpendicular, it appears again in its last direction, cutting the strata vertically from the bottom of the precipice to the top, above 200 feet; the height of the upper part of the cliff above the sea is here 330 feet.

These strata are almost all columnar, and the horizontal prisms of the dyke are strongly contrasted with the vertical pillars of the strata.

The basalt of this dyke is very nearly of the same grain with that of the dyke at the Causeway, rather coarser, its fracture granular, and full of shining points; but it differs materially from it in spect, the latter having but one principle of cons to wit, the minute prisms into which it breaks, gglutination of these forming it into a mere e the dyke at Port Spagna has, like some other f our basalt, a double principle of construction, st formed into huge massive prisms four and five neter, and these again being divided into small qu r prisms whose sides do not exceed an inch.

This property poss ome varieties of our basalt, and other curious ci es attending them,—as for instance, that some matic basalt in thin strata abound with marine shells, and impressions of cornua ammonis*, s, columnar and prismatic, but not articulated, s columnar, prismatic, and articulated, contain ull of fresh water to the amount of a thimble these facts have hitherto escaped notice.

Naturalists, who visit our coast, rarely allow themselves time enough to examine any thing, and, while there, are occupied in looking for arguments to support the theory they patronize, not in studying nature for information: they never examine any of our basalts but that of the Giant's Causeway; this, it is true, has none of the properties I mention, it has but one principle of construction, to wit,

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[*] The nature of this stone is, I know, not yet fully ascertained. Sir Joseph Banks informs me, that the specimens I sent to him are pronounced by his friends not to be *genuine* or *legitimate* basalt. An eminent Scotch naturalist, who visited the spot last summer, I am told, asserts this stone to be *chort petrosilex* or *schistus*.

On the other side, Mr. Kirwan, to whom I gave specimens, asserts in a late publication, that it is basalt: our ingenious Mr. Higgins is of the same opinion, and the celebrated Professor Pictet of Geneva, who did me the honour of a visit last summer, considers it to be basalt containing a greater portion of *silex* than usual. I believe Mons. Pictet is right.

the visible prismatic form so much admired; this afterwards breaks indifferently in all directions.

To return to my subject:—Though the basalt septs in mines in general, and every one of our own whynn dykes at Ballycastle, are attended by a depression of the strata on one side; yet those I have described at Bengore Head are accompanied by nothing similar, except the one at the Causeway; and proceeding further eastward, coasting this promontory, we meet with three depressions of our strata, where nothing like a whynn dyke is to be found.

The first is singular and beautiful; it is near a mile east from the Causeway, and a quarter of a mile beyond the last dyke. The precipice here is uncommonly magnificent, its height more than 350 feet above the water; and the upper part of this, which is accurately perpendicular and extends half a mile on either side, is 150 feet.

This whole face is composed of three strata, two of them formed of superb basalt pillars 45 and 55 feet long, with an intermediate stratum, near 60 feet, of another variety of basalt; the lowermost of these strata, when produced westward, dips, and at its intersection with the sea forms the Giant's Causeway.

This grand façade, together with the whole promontory, is as it were cut down and bisected by a vertical plane, on the west side of which the promontory and all its strata have sunk and subsided about 40 feet, without any other shake or disturbance, all the strata in the subsided part still remaining accurately parallel to the permanent strata, and proceeding westward in their former direction, only from points 40 feet lower.

An account of the variety, arrangement, and alternations of these strata, so completely displayed in the superb face of this precipice, where nature seems to have intended to exhibit to the philosopher the order in which she has disposed her materials, without putting him to the trouble of penetrating into the bowels of the earth, would lead me far beyond the limits of a letter. How these strata, with their ascent, culminations, dip, and immersions, have hitherto escaped the observation of naturalists is quite beyond my comprehension*.

The two depressions further east are much inferior to this; I shall only observe that there is not the least appearance of crack or disruption, the strata on both sides of the depression are all consolidated into one mass.

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* I cannot avoid quoting a passage from a late traveller, who seems to possess two qualities very necessary in a naturalist, to wit, *attentive observation* and *freedom from system*. He says, " No subject is more interesting or useful

When searching for whynn dykes upon our northern coast, I was obliged to omit about four miles of it lying between Bengore-Head and Carrickarede, as being too distant from Portrush and Ballycastle, where I was used to take boat, and totally void of shelter, even for the smallest craft.

To the westward of Ballycastle I saw only one dyke. On the east side of Kenbaan Point, a rock emerges from the water, which I have no doubt is part of a dyke, from the appearance it made; and as I approached it, I perceived it was formed of horiz [illegible] ns: here too a new feature occurred, common in [illegible] he dykes further eastward, but which I had not [illegible] in any of those I had yet examined; the centre [illegible] es of this one were constructed differently, th [illegible] in the centre being larger than those in the sides [illegible] very neat, the grain too probably, as in other [illegible] o differed; but I was precluded from examini [illegible] the circumstances which attended this curious l [illegible] ry rock, by the violent surf which then broke upo [illegible]

Hitherto the precipi [illegible] ough by the whynn dykes, and the rocks from a [illegible] ch they sometimes emerge, were all basalt, unifor [illegible] fied; but the accumulation of these strata, after [illegible] lipping, immerges beneath the sea to the westwa [illegible] ycastle, and a new system of materials arises at th [illegible] the strand to the eastward, to wit, alternate strata o [illegible] ne and coal; these are cut through exactly in the same manner the basalt strata were, by vertical whynn dykes, which all run into the sea, across the beach at the foot of the precipice.

The first of these is about two miles from Ballycastle, and though a rude imperfect one, it is not to be overlooked; the black or dark blue of the basalt being strongly contrasted with the brown colour of the freestone it passes through on the beach; here the high road runs close under the precipice, and affords a good opportunity to examine the contacts of the basalt dyke with the freestone it cuts through.

The next dyke, some hundred yards further east, is more

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useful than an examination of the intestine position of strata and veins:—in short, upon this is founded all our knowledge of geology; it is, however, attended with great labour and difficulty." (Jameson's Mineralogy of Scotland and Arran, page 61.) With us such an examination is attended neither with labour nor difficulty.

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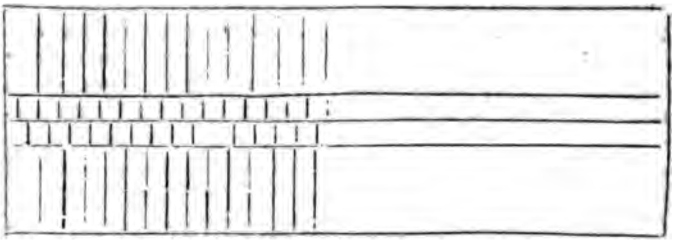
perfect, and so accessible on the beach, that its singular construction can be examined without any trouble; it is of the same breadth with most of the others, that is, about twelve feet; it more accurately resembles a quay than any of them, its surface is flat and its sides perpendicular, it is divided in its whole length by three right lines, one bisecting it through its middle, and one on each side of this, about a foot distant.

These three lines determine the style of masonry (if I may use the expression) with which it is built, to wit, horizontal prisms about five feet long, laid in rows on each side, and in the middle two rows of prisms about one foot square each. I attempt a skecth of these lines thus:



The bases of the long prisms show their polygonal figures on the sides of the dyke, and, if taken up and laid horizontally, would exhibit a rude *pavé:* these prisms are obviously composed of smaller ones like those at Port Spagna, but I had not a sledge sufficiently weighty to ascertain the fact with precision.

When I was on the spot, Mr. Magawly, who is concerned in and superintends the colliery, told me they were then cutting across this dyke 700 yards within the precipice.

The next dyke is of ruder basalt, and more imperfect; it seems to exhibit nothing remarkable.

The fourth Ballycastle dyke, or as it is called there the Great Gaw, emerges from beneath the precipice, of the same breadth and of the same rude material and construction with the first and third; but it is soon joined by what the colliers call its wing, that is a new wall annexed to it on each side, by which it becomes triple; these wings are of a very different material from the centre, being precisely the same in grain with the very fine Portrush stone, which sometimes contains shells and impressions of cornua ammonis, but in these wings I did not observe any.

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* When this dyke enters the water, it accumulates into an island, or rock, of much greater height and breadth, still the two materials keeping distinct, though so united at the contact as to form but one stone: thus the arrangement of the coarse and very fine basalt here and at Portrush, is precisely the same, saving only one difference, that at the latter place the planes of the strata are horizontal, while at the Great Gaw of Fairhead they are vertical, and in both places grow into each other without interrupting the continuity or solidity of the material, yet leaving the line of demarcation distinct.

Though the precipi part of Fairhead be not so accurately perpendicu engore, yet the depression of the strata on one s is dyke is visible from the water; and what is cur nge of massive pillars, near 100 feet each, appears permanent part, while over the depressed part n to be seen; whence it is plain that these strata been depressed by incumbent weight.

The miners tell me so a fifth dyke here, faintly marked without the , while the gaw, or sept, within the mine is to important, and has also its depression on one side he others at Fairhead, while at Bengore head no d is found but in the dyke at the Causeway; all t sions, as well as those at Bengore, where no d d, are on the west side of the line, or plane, se e permanent from the subsided part. I menti rious fact for the information of geologists who may possibly make some use of it.

These singular walls are not confined to the northern coast of our basalt country; its eastern side abounds with them still more. It was not in my power to examine any of those except such as lie in the bay of Belfast, but my ingenious friend Dr. M'Donald (a zealous mineralogist, whose pursuits in that line have of late been much impeded by great success in his profession,) informs me that they commence near Murlogh, where my tour on that side ended; that they are very numerous about Torr point, Garron point, and in general on all projecting points on that coast; and he conceives (I think judiciously) that points being found where the dykes are most numerous, arises from the protection they give the land in those places, preventing the sea from making the same inroads there it did on the adjacent parts.

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* I mentioned before that some naturalists have denied this Portrush stone to be basalt; but its being found here in a whynn dyke seems strongly to support the affimative, as I have never heard of a whynn dyke composed of any material but basalt alone.

Dr. M'Donald and I examined together the dykes at White-house point, four miles from Belfast; several of them are crowded together, three or four run parallel in an E. S. E. direction at about 150 yards from each other, and are in one place crossed by another at acute angles; several of these dykes, I am told, are traced across the county of Down on the opposite side of Belfast lough.

Though these dykes were so near, yet they differed materially from each other; in many the middle part and the sides were not of the same grain, nor constituted on the same principle; in some we found zeolite in the centre, but not in the sides; in others the middle part was formed by cutting it across (no doubt into prisms), while the sides were a rude mass studded with coarse round stones, about the size of an eighteen-pound ball; these last Dr. M'Donald assured me he had often broken, and found them composed of concentric spheres, like the pellicles of an onion; some of the dykes were of solid massive prisms laid quite across, while one or two had a longitudinal division running through their middle, as in the second dyke at Fairhead.

In all, the lines marking the construction of the dykes, whether accurate or faint, were across at right angles to their directions, but the perfection of the workmanship was very different; and when we attacked them with a light sledge, we found some to crumble, being in a state of decomposition, others resisted our efforts, while some broke into small quadrangular prisms, like the dykes at Port Spagna and the Giant's Causeway.

Dr. M'Donald showed me in his cabinet prisms he had taken from a quarry (no doubt a dyke) near Belfast; they were nine or ten inches long, and entirely composed of triangular pyramids of the same length, put together as if to illustrate Prop. 7. lib. 12th Eucl. Elem. I had found two or three small triangular pyramids among the quadrangular prisms at the Giant's Causeway dyke, but at the Belfast dyke triangular pyramids were the sole elementary figure.

As the shore in Belfast lough is low, there are but few opportunities of examining the materials that come in contact with the basalt dykes; in fact I noticed but two, stratified clay and freestone; this clay is very plentiful on the shore and the adjacent country; it is arranged in very thin horizontal strata, and when exposed to the air hardens almost to the consistence of stone.

At the contact the basalt and freestone were strongly united together, and for two or three inches the basalt had in some sort acquired the colour and grain of the sandstone: I was particularly attentive to this fact, as Mr. Werner alleges the transition of basalt into other stones, and Mr. Jameson found in Arran (pages 131 and 135) basalt sometimes mixed with, and at others penetrated by, sandstone; but on this occasion Dr. M'Donald, by some experiments, found that notwithstanding the freestone appearance the stone remained pure basalt.

The basaltic area, fro [illegible] orth and east sides of which these singular walls dive [illegible] such abundance, comprehends a considerable p [illegible] he country of Derry, and a much greater of the c [illegible] Antrim; its breadth varies from 20 to near 30 m [illegible] its length exceeds 35; it seems composed almost [illegible] ely of vast and steady basalt strata accumulated [illegible] ch other; in one place we count 16, in others we [illegible] are more, especially at Magilligan rock, as we kn [illegible] basalt to be 1200 feet deep there. This whole ma [illegible] pon a vast stratum of white limestone about 200 fe [illegible] of the same extent with the basaltic area, but discov [illegible] nly at its periphery, which extends above 80 miles.

This mighty stratum [illegible] to the southwad, until its lower edge acquires on [illegible] side a height of 800 feet, and on the west at [illegible] 0; the country below the limestone stratum, and without it, is on the west side mostly schistus, on the east sandstone and clay penetrated by basalt dykes, which furnish stones in abundance for all purposes.

The Scotch whynn dykes have been generally supposed to originate in Ireland. If this fact be admitted, we can easily trace them by attending to the directions of our own: thus those that issue from the coast west of Ballycastle, proceeding north with a slight inclination to the east, are to be sought for in Islay, Jura, Mull, &c., where Mr. Mills actually found them in great numbers.

Our dykes which are seen at Murlog, Torr, and Cushendun, are obviously those which, having crossed the Mull of Cantyre, were observed by Mr. Jameson in such abundance in the Isle of Arran.

Dr. Hutton also mentions 20 or 30 whynn dykes he found "in the shire of Ayr to the north of Irvine on the coast." These correspond with the numerous dykes abo

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Garron point and its neighbourhood, whose rectilineal course is directed towards that part of the Scotch coast.

The dykes about Larne may be expected to be found on the Mull of Galloway, while those I examined far up in Belfast lough, on account of their S. E. direction, probably do not catch Scotland, nor meet land until they arrive on the coast of Cumberland.

Whether our whynn dykes be *identically* the same with those on the Scotch coast opposite, is not easily ascertained, though highly probable; but even confining ourselves to our own country, we find sufficient matter for astonishment in contemplating our basaltic area, formed by accumulations of horizontal strata, with numberless vertical planes radiating from it: had Dr. Beddoes been acquainted with this structure of our basaltic country, I think he would scarcely have asserted, that "a right knowledge of basaltes is conducting us fast to a just theory of the earth." I think very differently from Dr. Beddoes, and conceive that instead of *assisting*, basaltic facts are throwing new difficulties in the way of cosmogonists, who flatter themselves they have developed the secret of nature; and that those in my country, (to which I confine myself) are utterly irreconcileable to any theory I have met with.

Two sects of naturalists, distinguished by the names of *Volcanists* and *Plutonists*, have of late taken possession of all the basalt in the world, and have divided it between themselves, under the descriptions of *erupted* and *unerupted lava;* and they have so convinced Dr. Beddoes of the validity of their claim, that he says, "I shall assume the origin of basaltes from subterranean fusion to be thoroughly established."

After such a round assumption it may be deemed uncivil to question the igneous origin of our basalt dykes; but natural history is not to be sacrificed out of respect to confident assertion: I will therefore try by the test of *facts* whether that description of basaltes (which your lordship wishes for information upon) ever was in fusion.

Foreigners seem to know little of whynn dykes except in mines. Mr. St. Fond found at Chamarelle in Vivarois what is obviously a whynn dyke, and it embarrassed him more than any fact he ever met with; it will be found entertaining to look into his *Vol. ex. de Vivarois,* and into his *Min. des Vol.* to see the difficulties into which this *courant de lave compacte,* this *ruisseau de basalte en fusion* has thrown him, and the swingeing postulates he is obliged to make, in order to get over them.

Dr. Hamilton on behalf of the *Volcanists*, and Dr. Hutton the great advocate for the *Plutonic* system, are more ready at their expedients; the first of these forms our whynn dykes by pouring in erupted lava at the upper aperture of mighty chasms; while Dr. Hutton conceives these chasms were filled up by his own unerupted lava, forced up at the lower.

In discussing the opinions of these gentlemen, I will make them the most liberal concessions; for instance, I will concede to both, that they have discovered the process by which nature has formed chasms of immeasurable length, immeasurable depth, and of inconsiderable, though uniform, breadth.

I will concede to D[illegible]on that he has brought to the edge of the chasm[illegible], "this foreign substance, which issuing from t[illegible]ass of basaltes that forms the northern extremity[illegible]ad, has descended over the adjoining strata," and [illegible]s it ready "*to fill up each cleft and vacuity*." ([illegible]rim, let. 5, part 1.)

I will also admit in [illegible] Dr. Hutton, that he has his unerupted lava rea[illegible] bottom of these chasms, that he has his machi[illegible]red for forcing it up, and that he has surmounte[illegible] difficulty, and discovered a mode of supporting [illegible]ass when raised; a point upon which, having [illegible]self, he would discourage others from forming co[illegible] (Edinburgh Trans. vol. i. page 285.)

Notwithstanding thes[illegible]ions, it will not be difficult to show that these gentlemen have not discovered the secret of nature in the construction of these singular walls, and that they were not formed by liquid lava filling up mighty chasms.

1st. Many of our contiguous dykes differ materially from each other, yet their proximity is such, that, according to the theory of either Dr. Hamilton or Dr. Hutton, they must have been filled up from the same source, and with the same material.

2dly. Many of these dykes, both in Ireland and Scotland, show a material difference between their middle parts and their sides, both in grain and internal principle of construction; the change too is not gradual, but per saltum, as if the dissimilar parts were separated from each other by planes parallel to their sides: all this is perfectly incompatible with the high state of fluidity in which the lava must have been, to enable it to fill up vast chasms of such diminutive breadth.

3dly. Our whynn dykes come in contact with a great variety of different substances, without producing such effect upon any one of them as might be expected from the contiguity of so glowing a mass; but however this argument may bear against the *Volcanists*, the *Plutonists* will say it does not apply to them, for the chemical operations of nature are carried on in Dr. Hutton's subterranean laboratory very differently from what we see on the surface of our globe: in the former Dr. Hutton says *calcareous strata are consolidated by the operation of heat and simple fusion*, and again, *having proved that these strata had been consolidated by simple fusion*, (page 253.) Dr. Hutton however confesses it is not easy to comprehend this: "and to be convinced that this calcareous stone, which calcines so easily in our fires, should have been brought into fusion by subterraneous heat without suffering calcination, must require a chain of reasoning *which every one is not able to comprehend*." (page 271.)

But it is not necessary on this occasion to enter into the mysteries of a laboratory, to which we have not access, nor to calculate the force of Dr. Hutton's great agent *compression;* for our observations on the contacts of the matter of our whynn dykes with the substances they encounter, being made on the surface of the earth, in the open air, even admitting those dykes to be formed as Dr. Hutton supposes, his unerupted lava is now become erupted, and of course, to use his own words, "those substances which calcine and vitrify in our fires, should suffer similar changes when delivered from a compression which renders them fixed." (Edinburgh Trans. page 280.)

I am aware I must fatigue your lordship by dwelling so long upon the question of the igneous origin of our whynn dykes; but as most modern writers and travellers call them *lava veins*, and the *facts* I have observed with much attention, induce me to combat so general and so popular an opinion, I hope you will excuse me for adding a fourth argument, which I conceive to be conclusive.

All substances, when ignited, are in a high state of dilatation; this is followed, when they cool, by a contraction, *une retraite*, by which they occupy less space than they did when heated; of course, had our dykes been chasms filled up with glowing lava, when this material cooled and contracted, it could no longer fill up these chasms as before, but must crack and separate from their sides, leaving intervals and disruptions; but nothing like this is observed, the dyke and contiguous matter, whatever it be, are solidly united together, forming but one mass.

These whynn dykes suggest other curious questions: Were they formed at the same time with the contiguous materials?

Were they posterior to them, as Dr. Hamilton and Dr. Hutton suppose?

Or, were they antecedent to the stratified masses, that every where come in contact with them?

The inutility of such speculations deters me from entering into them. I must however confess, that the *facts* seem to give stronger negatives to the two first questions, than to the last. But v hazard so bold an opinion, as that these mighty wa the first part of our world that was formed. Wh a must it convey to us of this *frame-work* or *skel* ar globe!

A new theory I con e likely to be a nuisance than an acquisition to istory; and that the road to the advancement of e would be better laid open by destroying some of have already.

Should therefore you think that the arguments I have adduced against ous origin of our whynn dykes are of any weigh probably make further inroads into the territori *lcan*, and question the igneous origin of basalt i

To this your lordship likely reply, that the topic is worn threadbare; th modern writers, without entering into the questio ounce it to be already decided in the affirmative; and that I shall never obtain attention to so stale a subject.

My opportunities, however, to procure information upon it have been superior to those of any other person: I have lived very many summers in the most important basaltic country in the world, and my fondness for the sea, and possession of boats, have enabled me repeatedly to explore our coast, which I know that no other naturalist ever did. It is to this coast and country that the advocates for particular opinions come to look for arguments to support the theories they patronize; it is painful to follow such gentlemen, correcting their statements, and contradicting their assertions: nor are they cursory travellers alone who misrepresent our facts; it will appear that men of science and ability are equally disposed to support their opinions at any expense;—a favourite theory is an adopted child, that must be maintained.

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But it is not by exposing the errors of others that science, and especially *natural history*, is to be advanced; nor is it by puzzling ourselves to find out in what manner, and by what process, nature has executed her work; let us rather examine attentively what she has actually done; let us quit disputing about the whimsies of our own brains, and study the code of *facts*.

In our basaltic country these are curious, as well as abundant; and it will be from such of these alone as have escaped the attention of my predecessors, and from the geological construction of the country, that the arguments to be applied to the question of the igneous origin of basalt will be drawn; and whatever may be their weight, at least they will have the merit of novelty to recommend them.

I am. with great respect,
Your lordship's
Most obedient humble servant,
Portrush. W. RICHARDSON.

P. S. When I found an opportunity for examining the whynn dykes to the northward of Whitehouse-point, I omitted several under the demesne called Macedon, which were much covered by sea-wrack; here I knew the surfaces of the dykes were decomposed, and their distinctive characters defaced.

Between Macedon and Carrickfergus there are many, all as usual differing from each other; some not so rectilineal in their course as those I have hitherto described; in one or two the prismatic construction was scarcely perceivable, while in the greater number the arrangement of these prisms laid across the dyke was most distinct.

In two contiguous dykes I observed, that the axes of these prisms were not horizontal as usual, but in one greatly elevated to the north, and in the other towards the south.

Human attention could not follow the variety which nature has displayed in the formation of these dykes; therefore, not to fatigue the reader, I will describe but two more particularly; I select these, both on account of the new circumstances attending them, and also because they are easy of access, being within a few yards of the great road from Belfast to Carrickfergus.

The first of these runs eastward along the strand, about 400 yards south of the gallery; we approached it from the north, and found it composed of long well-formed horizontal prisms, lined on the north side by a sort of basaltic wall about 18 inches thick: this a military gentleman of our party called its *revetement*; I adopt the word on this occasion for convenience.

After we had traced the dyke eastward for several yards, we observed this revetement separate from it, and diverge at a considerable angle, then, forming a curve, disappear beneath the sand to the north-east; this new circumstance exciting our attention, we traced the revetement back to the dyke, then along it to the westward, when after some time we perceived it entering the dyke at an acute angle, and crossing it diagonally; when across, it formed for several yards a *revetement* on the south side of the dyke, then diverging from it, and curving as before, it was again lost under the sand to the [illegible].

The second dyke I [illegible] be particularly, lies about 500 yards north from [illegible] *stream*, and about three miles from Carrickfer [illegible] med composed of four or five distinct walls, ag [illegible] ogether; in each of these the prismatic constru [illegible] ifferent from that of the others, and in one the [illegible] prisms were not as usual at right angles, but ob [illegible] e direction of the dyke.

A new circumstance [illegible] ere too; this dyke, about 25 feet broad, had a [illegible] f freestone on each side, and was also twice or [illegible] trated by walls of freestone similar to, and in the [illegible] tion with, the basalt walls between which they l [illegible] freestone walls were more than a foot broad, a [illegible] es composed of horizontal laminæ, and at others [illegible].

I have since discove [illegible] ificent dyke in the face of the stupendous precipice [illegible] hill* which it cuts vertically near 200 feet, and is afterwards to be traced a great way down the hill.

Though this dyke be attended by very curious circumstances, I will take no further notice of it, as I hope to see it soon accurately described by my ingenious friend Dr. M'Donald, who was with me when I discovered it, and whose vicinity affords him better opportunities of accurately examining this beautiful and interesting façade.

LVIII. *Notice respecting New Books.*

A NEW edition of Dr. Henry's chemical work is in the press, and will be published in the course of a few weeks.

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* A stratified basaltic mountain, nearly hanging over Belfast; it is well worth the attention of naturalists.

He has found it impossible to give a sufficiently ample and distinct view of the numerous and important discoveries which have been made in the science during the two last years, without extending the work to a second volume. And as its original title would but ill accord with the enlarged form under which the book will now appear, it will be changed to that of "ELEMENTS OF EXPERIMENTAL CHEMISTRY."

LIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

ON the 3d, 10th, and 17th of May, the reading of Mr. Macartney's observations on luminous insects was continued. The result of the author's researches has enabled him to state that 12 different genera of insects, which include an immense number of species, emit light. Seven of these genera belong to the order of *mollusca*, and the other five to the hemipterous, as the *fulgoræ;* the apterous, as the *cancer pulex* and *fulgens*, &c. Mr. Macartney took an historical survey of what has been written on luminous insects, related the discoveries of different voyagers, such as those of Sir Joseph Banks, who discovered two species, the *cancer pulex* near Madeira, in 1774, with capt. Cook, and the *medusa noctiluca*. Capt. Horsburgh also discovered two species in the Arabian sea, which he gave to the author, one of them like a wood louse, the other he called *medusa scintillans*. The same accurate observer also noticed various luminous appearances of the sea, and some insects, which on being pressed emit a luminous fluid. He also gave to Mr. M. a drawing of one of the insects, which he took out of the water at a time when the sea appeared almost white, like a vast field of ice covered with snow. This appearance is ascribed by the author to immense quantities of *medusa scintillans*, which emit flashes of light, and so frequently as to assume a continued brightness. To this species of *medusa* the author attributes the sudden flashes of light which are occasionally seen on our own sea shores. Mr. M. has also discovered three different species of luminous insects on the southern coasts of England. In the course of his inquiries, he appears to doubt the luminousness of the *cancer pulex*, but notices the *pyrosoma atalantica*, a worm-shaped luminous insect, observed by M. Perot, of which only one genus or species has yet been discovered.

In summing up some concluding remarks on the cause of this luminous quality in insects, Mr. M. expresses himself with great diffidence, and from the experiments of Rumford, and his own observations, hesitates in stating whether light is not rather a quality than a substance, as all the phænomena of luminous insects tend to give probability to the former opinion. The *medusa*, he observes, can emit light for any indefinite time: their light and that of other sea-insects appear of no specific use to the animal, but that of glow-worms and flies serves to make them known to each other in t. All luminous animals shun the light of day, the author infers that they cannot have imbibed sc sufficient to emit so much during the night; that ous matter of the sea, or *medusa*, has nothing in oric or inflammable; that the manner of secreti ninous matter (if so it be) is yet wholly unknown e sudden death of the animals, and consequent e f their luminousness; prevent all anatomical or cal observations, and that the number of creatur ing this peculiarity is very considerable. He relate umber of experiments, all of which tended to den hat this light has nothing in it of a phosphoresc , as universally believed, and that, whatever it m s no longer attributable to the presence of phosph ammable matter.

May 24. The introdu a paper on the sexual organs and mode of genera he *squalus* genus, or dog-fish, by Mr. Home, was read. It related principally to a description of those organs in the fishes, of which Mr. H. has before given some general accounts to the Royal Society.

LINNÆAN SOCIETY.

April 17.—The President in the chair. The following papers were read: Observations by Olof Swartz, M.D. on some former Species of Andromeda, properly belonging to the Genus Menziesia:—On the supposed Effects of Ivy on Trees, by Humphry Repton, Esq.:—On the Fasciola Hepatica, by Mrs. Cobbold.

May 1.—A paper was read On the Genus Andræa, with Descriptions of four British Species, by W. J. Hooker, Esq., F.L.S.

Thursday, May 24, being the anniversary of the birthday of Linnæus, the Linnæan Society met at their house in Gerard Street, in pursuance of their laws and charter, to elect a president, council, and officers for the ensuing year: when the following five new members of council were elected:—John Blackburne, esq. Edward Forster, esq. George Milne, esq. Edward Rudge, esq. and Edward Lord Stanley. And the following were chosen as officers: James Edward Smith, M. D. president; Thomas Marsham, esq. treasurer; Alexander MacLeay, esq. secretary; and Mr. Richard Taylor, under secretary.

The society afterwards dined together at Freemasons' Tavern, as usual.

WERNERIAN NATURAL HISTORY SOCIETY.

At the meeting of this society, on the 7th April, Dr. Macknight read a mineralogical notice, on the tract of the Highlands from Killin to Braemar, by the way of Glen Tilt. Ben Lawers is composed of undulated mica-slate, which at the summit is yellowish gray, and in some varieties so full of quartz as to resemble a sandstone. Towards Logierait, beautiful garnets begin to appear. Beyond Mullenearn, gneiss occurs; also limestone, hornblende-slate, and sienite. Besides the substances first mentioned, Glen Tilt is remarkable by a peculiar aggregate of feldspar, hornblende, and occasionally quartz; in which the various proportions of these ingredients exhibit the rock under various aspects of the sienitic and greenstone species. It is distinguished from granite (for which it has been mistaken) not only by the uncrystallized state of the feldspar, but by the presence of hornblende, and the absence of mica. Professor Jameson has entitled it sienitic greenstone. It occurs in conformable beds; particularly one of great size, which intersects the channel of the river at different places, near the lodge. Crossing the mountains from Glen Tilt to the course of the Dee, we find hornstone, feldspar-porphyry, and limestone, subordinate to mica-slate and gneiss; till we reach the Castletown of Braemar, where the granite of the Grampians at length appears.

At the same meeting, a communication from colonel Imrie was read, describing the conglomerate rock of the Grampians, and tracing it from near Stonehaven to the Burn, and again at Callender, 80 miles distant. The position of this conglomerate-rock is nearly vertical; and of this fact, in col. Imrie's opinion, no satisfactory explanation has yet been given.—At this meeting, also, there was laid before the society an accurate section of the coal-field at Alloa, accompanied with interesting remarks, by Mr.

Robert

Robert Bald, civil engineer, and manager of Mr. Erskine of Mar's extensive coal-works. The depth of the section is 704 feet; the alternating strata are 141 in number; and the total amount of the different beds of coal is 59 feet 4 inches.—Captain Laskey likewise presented to the society a series of the remains of a curious fossil Encrinus found in slate-clay near Dunbar.

FRENCH NATIONAL INSTITUTE.

[Continued from p. 317.]

GEOLOGY.

The observations from which geology can derive the most important advantages, are certainly those which are directed to the subjects of fossil animals, but more particularly those which lived upon dry land. M. Cuvier has continued the investigation of this important subject. He has brought to a termination, in conjunction with M. Brongniart, the mineralogical geography of the environs of Paris, a slight sketch of which was given in the account of the labours of the Class for the year 1808*. He afterwards directed his attention to the osseous heaps (*brèches*) on the shores of the Mediterranean. Rocks similar to those which are to be seen at Gibraltar, near Terruel in Arragon, at Cette, at Antibes, at Nice, in Corsica, in Dalmatia, and in the isle of Cerigo, have been found in the fissures of the compact limestone which constitutes the principal soil of these various places, and they are all composed of the same elements: it is a red cement, like brick, which connects in a confused manner numerous fragments of bones and of limestone in which these heaps are contained. All the bones found in these rocks belong to herbivorous animals, most of which are known still to exist on the adjoining soil; they are mixed with fresh-water shells. This inclines us to think that these heaps are posterior to the last continuance of the sea on our continents, but very ancient nevertheless; since nothing proves that similar heaps have been recently formed, and some of them, such as those in Corsica, contain even unknown animals.

Alluvial earths also contain bones of herbivorous animals: they have been discovered in the peat-mosses of the valley of the Somme, with stags' horns and heads of oxen, and in the environs of Azoph, near the Black Sea. These bones have belonged to a species of beaver: the former resemble those of the com[illegible] beaver; the [illegible] which form a complete head, seem to have belonged to a much larger species than we are acquainted with; and M. Fischer, who discovered this animal, has given it the name of trogontherium, which M. Cuvier adopts as the specific name.

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* See page 36 of

Bones of herbivorous animals have also been found in schists. Three kinds have been described. M. Cuvier saw the figure of one which some authors regarded as having appertained to an Indian boar, and others to a polecat. M. Cuvier rather gives it the character of a herbivorous animal; but he has not been able to ascertain the genus nor species.

Among the fossil bones of ruminating animals found in loose strata, M. Cuvier has recognised a kind of elk different from that with which we are now acquainted. The bones of this animal have been found in England and Ireland, near the Rhine, and in the environs of Paris, in beds of marl at no great depth, and they seem to have been deposited in fresh water. Some horns discovered in abundance in the neighbourhood of Etampes, in sand surmounted by limestone of fresh water formation, prove the existence of a small species of rein-deer which seems no longer to exist. M. Cuvier has besides observed remains of horns of goats, fallow-deer and stags, which do not seem to differ essentially from the horns of the existing species: "Nothing," says our author, "is more abundant: all the recent alluviations dug up have furnished them; and if we do not find plenty of testimony as to these fossil bones, it is because from presenting themselves at trifling depths they have not been thought worthy of much notice."

In the fossils of ruminating animals with hollow horns, M. Cuvier has recognised crania of aurochs, discovered in the banks of the Rhine and the Vistula, in the environs of Cracow, in Holland, and in North America. These crania exceed in size those of the aurochs; but, as M. Cuvier observes, this difference may be ascribed to the abundance of food which these animals formerly possessed, when ranging at pleasure through the vast forests and pasturages of France and Germany.

There is another kind of fossil cranium differing only from our present oxen from the size, being larger and the horns being in a different direction. These crania have been found in the valley of the Somme, in Suabia, Prussia, England, and Italy. "If we recollect," says M. Cuvier, "that the ancients distinguished in Gaul and Germany two kinds of wild oxen, the urus and the bison; may we not suppose that one of the two, after furnishing our present race of oxen, was extirpated in his savage state; while the other, which could not be subdued, still subsists in small numbers in the forests of Lithuania alone?"

In loose soils we also meet with bones of horses and of wild boars: the former almost always accompany the fossil elephants, and are found along with the mastodonti, tigers, hyenas, and other bones of fossil animals discovered in alluvial soils: but it was impossible to ascertain if these horses' bones belonged to a species different from our present race. The bones of wild boars have been for the most part procured from peat-mosses, and do not in the least differ from those of the wild boars of the present day.

Other bones have been foun[illegible], which M. Cuvier has ascertained to belong to an unkr[illegible]own species of *lamantin* or *manati*. They have been discovered in strata of coarse marine limestone on the banks [illegible] the Layon, in the environs of Angers; and they were m[illegible]l with other bones, some of which seemed to have belon[illegible] to a large species of *phocas*, and the others to a dolphin.

The skeletons of three spe[illegible]s of oviparous quadrupeds, preserved in calcareous schis[illegible] have also been the object of M. Cuvier's researches.

The first was found in the [illegible]ists of Œnigen, situated on the right bank of the Rhi[illegible]t the mouth of the lake of Constance. It had been [illegible]ribed and engraved as the skeleton of an antediluvian [illegible]; but this error was refuted. M. Cuvier proves by a seri[illegible] osteological inquiries that this reptile was analogous to a salamander, and belongs to the genus *proteus*.

The second, also found in the same place, seems to have belonged to the toad genus, and resembles the *bufo calamita*.

The third, and most singular, which was discovered in the quarries of Altmuhl, near Aichtedt and Pappenheim, in Franconia, and which had been described and drawn by Collini in the Memoirs of the Manheim Academy, is regarded by M. Cuvier as having belonged to a species of otter. The length of its neck and head, its long snout armed with sharp teeth, and its long paws, indicate that this animal fed on insects, and that it caught them when flying: the size of its orbitary sockets also shows that it must have had very large eyes, and that it was a nocturnal animal, like the bat. No beast of the present day has the least resemblance to it.

M. Cuvier, has also published a Supplement to his Memoirs on the Fossils of Montmartre; in which he gives the figure and description of an ornitholite, much more complete than hose which have been hitherto published. It is probable that it belonged to the class of *gallinacei*, and the common quail is the modern species which it resembles.

M. Sage has given us the description of some carpolites, or petrified fruits. One of them was the kernel of a nut become calcareous, and found at Lous-le-Saulnier: another seemed to have been the fruit of a wild nutmeg-tree, which grew at Madagascar and in some of the Moluccas; its substance was also calcareous: the third seemed to have belonged to something resembling the durion of India; it was transformed into jasper. To these new facts he subjoins some of the remarks which had been already made on carpolites, and concludes that the petrified fruits found in our climate are exotics. He also enters into some chemical details, by means of which he explains how these petrifactions took place.

BOTANY.

Order and method will always be two objects of the first importance in natural history, and particularly in botany: they serve at one and the same time to establish the relations which bodies have with each other, and to guide the observer in the midst of the innumerable productions of nature. The most celebrated naturalists have made it the particular object of their studies; and the knowledge which the real science of the various systems requires, could never have been embraced but for them.

M. Jussieu, who has so just a title to be considered as the legislator of botany, has formed a new order of plants under the name of Monimiæ: the genera of which it is composed are, the *ruizia*, the *monimia*, the *ambora*, and perhaps the *citrosma*, the *pavonia*, and the *atherosperma*. This order ought to be placed immediately before the family of the Utriceæ; but after the Monimiæ M. Jussieu places the *calycanthus*, heretofore united to the Rosaceæ; he considers it as the type of a new order, which will serve as a stage between the Monimiæ and the Utriceæ.

M. Palisot Beauvois has proceeded with his inquiries into the order of Gramineæ. He has studied their organs of fructification more exactly than any person had done before him; has founded on the organization of each of the parts of these organs the characters which ought to distinguish them from each other, and obtained the means of dividing the different species of this order into genera, much more natural than those which had been hitherto adopted.

M. Labillardiere has made us acquainted with a new plant of the family of Palm-trees, of which he has made a genus under the name of *ptychosperma*, and placed it next the elati and arecas. This plant was discovered by the author in New Ireland: it rises frequently to the height of 60 feet and upwards, and yet its trunk is only two or three inches in diameter. These proportions induced him to give it the name of *gracilis*. It is astonishing, as M. Labillardiere remarks, that so slender a tree can support itself; but we know that in all the monocotyledons, the hardest of the ligneous part is external, and this structure gives to the plants of this class a strength which those cannot possess whose most solid fibres are in the centre.

M. Lamouroux has pre to the Class a very extensive work on marine plants. ittle or no attention has been paid to these singular v tables, and they have been arranged in rather an a manner: M. Lamouroux, by forming into a single ll the plants known to exist in the sea, seems to wrought an advantageous change.

The little progress whi been made in the study of the algæ, was the cause disagreement among botanists as to the organs whi to the reproduction of these Cryptogamia. M. Cor work written expressly on this subject, had recog le and female organs in the tubercles placed at the ex ties of the ramifications of these plants. M. Lamou artakes of this opinion; but he characterizes with pre he different parts of these organs, and thus throws t deal of clearness on the study of these singular vegetables. This author has besides observed that the kind of algæ which grow on granite, are never the same with those found on calcareous stone or on sand, and *vice versâ*. As to their internal organization, M. Decandolle had ascertained that it was devoid of vessels, and entirely formed of cellular texture. M. Lamouroux distinguishes two kinds of cellules; the one being long hexagons, which form the stalks and the nervous parts (*nervures*) of the ramifications; the other kind is of the same form with the foregoing, but has sides almost equal, and which constitute the membranous or foliaceous substance. M. Lamouroux thinks that the former may be analogous to the vessels, and the second to the utricular texture of the most perfect vegetables. These general labours led the author to form in this family several new genera, which he has also presented to the Class for their approval.

M. Mirbel has continued his researches on vegetable physiology. Formerly it had been ascertained that the albumen of the seed generally served for the nourishment of the young plant after germination: but this opinion perhaps had still need of support from positive observations; and M. Mirbel, by means of an experiment equally simple as ingenious, seems to have dispelled all doubts on the subject. The embryo contained in the grain of the albium cepa becomes curved on being developed, so as to form a tail which issues from the ground, while the radicle and plumule still remain under it. If at this period of vegetation we make any mark at an equal height on the two branches of the germ, we shall see the speck nearest the radicle rise alone in the case where the plant receives no nutriment except from the juices of the earth: if, on the contrary, it be only kept up by the albumen of the seed, the speck of the plumule will rise above the other: lastly, the specks will rise nearly equally, if the earth and the seed concur to the development of the germ. It is this last phænomenon that takes place; it ceases when the albumen is entirely absorbed: in that case the young plant has sufficient strength to derive from the earth, or from the atmosphere, the nutriments which it will immediately require.

This memoir is accompanied by interesting observations on the germination of the asparagus, and on the manner in which the leaves of this plant, sheathing themselves at first like all those of the monocotyledons, become by the growth of the stalk, lateral and opposite, and afterwards lateral and alternate.

In another memoir M. Mirbel has undertaken some new enquiries respecting the germination of the nelumbo. Botanists were not agreed as to the class to which this plant ought to be referred, and as to the nature of the two fleshy lobes in the midst of which it takes its origin. Some, not observing any radicles developed in the germination of this plant, thought that it was entirely devoid of them; some regarded the lobes just mentioned as roots; and others regarded them as peculiar organs, and analogous to the vitellus. It is by means of anatomical observations that M. Mirbel endeavours to dispel the doubts which these various opinions have raised. He recognised in the first place, in the nelumbo, all the characters which distinguish the plants with several cotyledons from the plants with a single cotyledon. He afterwards found in the lobes of this plant vessels analogous to those of the cotyledons, and he observed, at the point where these lobes join, other vessels which are united in the same manner with those which characterize the radicles in the embryos furnished with this organ:

and

and he concludes that the nelumbo does not differ essentially from the other plants of its class.

M. Correa, although he agrees with M. Mirbel that the nelumbo is a plant with two cotyledons, does not share in his opinion respecting the nature of the lobes: he thinks, with Gærtner, that these organs have a considerable analogy with the vitellus, and he compares them with the fleshy tubercles of the roots of the orchis. The plants, as this learned botanist observes, have a double and relative organization.—on the one hand, with the earth in which they ought to take root, and on the other hand, with the air in which their foliage is developed; the roots as allotted to the ascending vegetation, and the leaves to the descending vegetation; and it is at the point where these two systems of organization unite, that the cotyledons are generally placed:—Now the lobes of the nelumbo are at the most inferior part of the plant, and consequently in the system of the ascending vegetation, or of the roots. This view of regarding the nelumbo would indeed take away the means of recognising the cotyledo n it; but the example of many other plants deprived these organs, shows that they are not at all essential to vegetation, and that the characters which have been derived from them, in order to separate the vegetable kingdom into three divisions, are insufficient, and that they ought to be replaced by those which give the direction of the vessels and the medullary radii.

It is also with the view of dispelling the doubts arising from the different opinions of several learned botanists, that M. Poiteau has undertaken a work, which he has submitted to the Class, on the germination of the Gramineæ. Botanists were not agreed as to the part of the seed of these plants which ought to be regarded as the cotyledon; but observing that the escutcheon, which Gærtner took for a vitellus, and M Richard for the body of the radicle, was placed in the point where the plumule and the radicle separate, he considers this organ as a true cotyledon. These inquiries have besides led M. Poiteau to an observation, which, although accidental, is not the less interesting, since it is connected with one of the phænomena which are most general in vegetation. At the moment when the radicle of the Gramineæ is developed, it takes the figure of a cone, and represents the principal root or the pivot of the other plants; but soon afterwards, and the instant the lateral roots acquire a certain growth, this cone is obliterated and destroyed, so that no plant of this family has a pivot: and as M. Poiteau has made the same observation on several other plants with a single cotyledon, we may suppose that this substitution of numerous roots and secondary to a principal root takes place, because each fasciculus of fibres of the monocotyledonous plants has its proper root. This naturally recals to our minds the fine observation of M. du Petit-Thouars on the increase in size of the dracæna, which has been discussed in the reports of preceding years.

ZOOLOGY.

The researches of M. Cuvier respecting fossil animals have generally led him to preliminary discussions as to the species admitted by naturalists, and they have been almost always the source of some valuable observations in zoology properly so called. Thus in his Memoir on the osteology of the lamantin, when considering the organisation of the amphibious mammiferæ, he is led to separate *phoci* and *morsi*, the *dugons*, the lamantins and the species described by Steller which had been confounded with these last animals. These three genera form a family which is distinguished among its members by the absence of the posterior extremities, and by the teeth of herbivorous animals: he reduces to two the four species of lamantins established by Buffon, and gives precise characters to those which he admits into these different genera.

In another Memoir, on cats, the same author gives the osteological character of the head of the chief species of this genus, and he gives an account of one which had not been recognised by modern naturalists. This new species has received the name of leopard, which had become synonymous with panther, for want of a precise application. It differs from this last species in being of a smaller size, and having more numerous spots.

M. Geoffroy had long formed under the name of Atèles a particular division of apes devoid of thumbs, which had been formerly confounded with the sapajons, from the catching tail which is common to all these animals. He has added two new species to those which he had already given an account of, and has given figures and descriptions of them. One of these only, to which he gives the name of Arachnoides, and which is yellow, had been described by Edwards and Brown. The other denominated *Encadré* is entirely new; it is black with white hair around the face.

The same author has given a description of two birds, the one scarcely known, and the other entirely new: this last has a resemblance to the *corvus nudus*, and to the *corvus calvus;* but they differ sufficiently to form three distinct genera, which M. Geoffroy establishes by the names of *cephalopterus*, being his new species, *gymnoderus*, which he applies to the *corvus nudus*, and *gymnocephalus*, by which he distinguishes the *corvus calvus*.

The cephalopterus is black, with a very high crest, which falls forwards upon the beak, and a kind of dewlap, also covered with feathers. The feathers of both these parts are of a metallic violet hue.

The second bird, which like the above is also from Mexico, had been described but imperfectly by Marcgrave under the name of cariama. M. Geoffroy from this description had considered it as closely connected with the agami; but now that it is to be seen in the collection of our Museum of Natural History he regards it as forming a distinct genus, to which he gives th tin name of *microdactylus*.

Tortoises have also been one of the subjects of M. Geoffroy's researches. Having rved in Egypt the tortoise of the Nile described by Fo nl, he was induced to form a distinct genus, of all the ner tortoises which like the latter have the extremity of the sides at liberty and a soft calipash. He calls them *trionix*, and has added several new species to these already known. M. Brongniart in his great work on reptiles had joined the latter to his *Emydes*, observing always the charac which distinguished them from the other species of th enus of which the calipash is complete and covered wit ales. M. Geoffroy, in addition, joins to the genus *Chelys* of M. Dumeril, the tortoise described by Bartram under the name of tortoise with large soft scales, and discovered by this traveller in North America.

These animals present a striking example of the progress of zoology of late years. The number of tortoises known 20 years ago was scarcely 30, and now it has been at least doubled. This among other circumstances has been communicated to us by a work of M. Sweiger, in which he has undertaken to give a general monography of all the tortoises. This fine work, accompanied by precise descriptions of a very extensive synonymy and embellished with figures excellently drawn by M. Oppel, has been submitted to the inspection of the Institute, and highly approved of.

The class of fishes has also been enriched with many new species. Messrs. Risseau and Delaroche, who have particularly directed their attention to this branch of zoology, have communicated their observations to us. Those of the former have been made on fishes in the Gulf of Nice, and those of the latter were made on the fishes in the sea around the Balearic Islands. But the labours of these naturalists have not been confined to bringing new species to light:—from their accounts there are grounds for supposing that each species of fish, like terrestrial animals, has a region, in the midst of which its existence is circumscribed, and that those of the south are never met with in the north, and *vice versâ*. M. Risseau, however, has discovered in the Mediterranean some fishes which had not hitherto been found except in the Indian or in the Northern seas.

M. Delaroche has made some interesting researches as to the depth at which each species of fish lives habitually, as to the modes of fishing, and on the subject of the swimming-bladder.

PHYSIOLOGY.

Physiological experiments of all others are those which require most leisure and patience, while the rigorous exactitude so important and necessary in the sciences is more difficult of attainment in physiology than in any other branch of experimental philosophy. Humboldt, however, while occupied on a voyage in which obstacles and dangers were daily increasing, directed his attention to some delicate experiments on several of the phænomena of life. He has communicated the researches which he made in America on the respiration of the crocodile with the sharp beak: he was led to ascertain "that this animal, notwithstanding the volume of its bronchiæ and the structure of its pulmonary celluli, suffers in an air which is not renewed; that its respiration is very slow:—in the space of an hour and 43 minutes, a young crocodile, three decimetres in length, took up only about 20 cubic centiemes of oxygen from the surrounding atmosphere."

Since his return to France, M. Humboldt, in conjunction with M. Provençal, has made some additional inquiries into the respiration of fishes. The experiments of these gentlemen, which are numerous, and remarkable for their accuracy, have led them to very important results.

The experiments of Spallanzani, and of our colleague, had demonstrated that it is not by decomposing water that fishes breathe, as some naturalists thought; but by taking up the oxygen dissolved in this liquid, or by coming to the surface of the water they collect it immediately from the atmosphere. To these observations all our knowledge on the subject was confined: the nature and quantity of the gases had not been established, which were absorbed by these animals in the act of respiration; and the result of these phænomena, the chief object of the experiments of Humboldt and Provençal, is to elucidate these questions. With this view they first consider fishes in their natural state, and respiring river waters; they next examine the action of the bronchiæ on the ambient water impregnated with oxygen and azote, carbonic acid, or a mixture of hydrogen and oxygen, and they afterwards treat of the changes produced by fishes on different aëriform fluids in which they are plunged.

Seven tench (*ciprinus tinca*) were placed under a bell-glass filled with river water, and which contained 4000 cubic centimetres: after eight l urs and a half respiration, the fishes were withdrawn, a the analysis of the air still remaining in the water show hat in this space of time the fishes had absorbed 145·4 c ygen, 57·6 of azote; and that 132 of carbonic acid h en produced. Hence it results, as observed by our auth s, "that by the respiration of the fishes submitted to this xperiment, the volume of the oxygen absorbed exceeded only by two-thirds the volume of the azote which had disappeared, and that no more than one-eighth of the former had been converted into carbonic acid."

Fishes suffer greatly in water entirely freed of air; and after 20 minutes they fall m less to the bottom of the vessels. In pure oxygen t animals seem to respire with avidity, and open their bronchiæ more widely. In azote and hydrogen, they keep their brônchiæ close, seem to dread the contact of these gases, and die very soon after having been plunged into the water which contains them, Carbonic acid kills them in a few minutes: but fishes do not absorb oxygen and azote by their bronchiæ alone: the whole surface of their bodies has the faculty of acting on these gases, and of assimilating them. After having withdrawn the fishes from water saturated with the deleterious gases, and analysed it, some portion of carbonic acid was found in the liquid; but as there had been no oxygen absorbed, it is probable, as observed by Messrs. Humboldt and Provençal, that it was not the result of respiration, but that it had been exhaled from the surface of the body. Such are the principal points in this work, which besides contains other useful observations and interesting views on the physiology of fishes, but which the limits of this report do not permit us to enter upon.

In speaking of respiration, however, we cannot pass over in silence a memoir read to the Class by M. Provençal, on the respiration of mammiferæ in which the nerves of the eighth pair had been cut. We have already spoken of the experiments which had been made to ascertain the influence of these nerves on respiration, by which this influence was demonstrated: some doubts, however, remained as to the way in which it is exercised. M. Provençal was anxious to ascertain if the animal in which the eighth pair of nerves had been cut, absorbed as much oxygen, and produced the same quantity of carbonic acid, before, as after the operation. Numerous experiments made with care proved that the animal, after the section of the nerves, absorbed less oxygen and produced less carbonic acid than before this section; but these changes are produced by gradation only. At first the respiration does not appear weakened; subsequently, it is effected with less strength; and finally these phænomena cease entirely, but most probably in consequence of the cessation of the mechanical functions of the chest. It was interesting to ascertain if the animal heat diminished in the same proportions with respiration. M. Provençal therefore made all the experiments necessary for resolving this question; and it would seem that in fact the temperature diminishes soon after the nerves have been cut and the respiration has slackened.

The functions of the organs whose action has been just mentioned, are well known; but there exists in animals a certain number of other organs whose functions are not evident, and as to the use of which physiologists are still divided in opinion. Among this number is the swimming-bladder of fishes. This singular organ, which is only to be found in this class of animals, is not met with in all the species; and it exhibits so many varieties in its organization, that at the first glance one would incline to think that it did not perform the same functions in some that it did in others. Generally this bladder is filled with air, and composed of two membranes. Sometimes it communicates with the stomach by a canal: at other times it has no apparent communication, and in this case it contains a peculiar organ of a red colour and of a lamellated structure, according to the observations of M. Duvernoy. There are some bladders, however, which are furnished with these red bodies, and which have a canal of communication; and some, but few in number, have peculiar muscles. The opinions of authors vary as to the use of this organ and of its different parts: in general it has been thought that it was employed to change the specific gravity of fishes, and that for this purpose the animal, by means of its muscles, compressed this organ, and thus varied its dimensions according as it was desirous to remain in equilibrium, to ascend or descend in the medium in which it exists. As to the way in which the air gets admission, it has been thought that it was by means of the canal in such bladders as are furnished with it, and by means of the glands by secretion in those which have no communication externally. Besides, we know from the experiments of M. Biot, that this air is a mixture of oxygen and azote, and that its nature varies according to the depth at which the fish lives: so that the species which are procu m the bottom of the sea contain a greater proporti oxygen, while those which are found near the surface more azote. M. Delaroche having collected a great nu of fishes in the Mediterranean, has described thei ing-bladders, and made us acquainted with some wh e unknown before: he has confirmed the experiments Biot; and as to the uses of this organ, he adopts nearl ame results with preceding naturalists.

This air-bladder has a the subject of some researches by Messrs. F and Provençal. They wished to ascertain wha he relations between this organ and respiration. ef results of their experiments are as follow:—T ai ntained in the swimming-bladder does not depend he air brought into contact with the bronchiæ; the ab f this organ does not affect the process of respiration, but it seems to prevent the production of the carbonic acid gas. Lastly, they have seen tench, from which the swimming-bladder had been taken, swim about, rise and sink in water, with as much facility as those which are furnished with them.

These labours have given rise to a very detailed report of M. Cuvier, in which he brings to view all the inquiries which have been undertaken as to the swimming-bladder of fishes, and in which he again takes notice of the various questions which have arisen on the subject. After a profound discussion, he arrives at some general results which we have mentioned above, and details every thing which still remains doubtful on the subject*.

There are some other experiments from which physiologists may derive great advantage. These relate to the action exercised by substances of various kinds on the bodies of animals when introduced into the circulation. Medicine, indeed, affords many experiments of this description: but they are still too few in comparison with those which might have been tried.

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* See the present volume, p. 291—302.

[To be continued.]

LX. *Intelligence.*

DE LUC'S ELECTRIC COLUMN.

In our last number (p. 317), by a typographical omission we stated that "since that time (viz. 25th March) they (the bells) have been known once to cease ringing."—The sentence should have read "since that time they have *not* been known once to cease ringing."

We have since received notice, that on the 21st of May the small bells connected with De Luc's electric column were still ringing, and it was supposed had continued so without intermission since the 25th of March.

We are also desired to request that those of our readers who may have constructed columns of the kind alluded to, will favour us with such observations upon them as they may make from time to time.

LIST OF PATENTS FOR NEW INVENTIONS.

To Luke Hopkinson, of Holborn, in the county of Middlesex, coach and harness maker, for certain improvements to a bridle bit, or bits for bridles used in driving or riding horses or other animals. May 2, 1810.

To Daniel Beaumont Payne, of the city of Bath, banker, for his new plan or method for more accurately and expeditiously expressing and ascertaining the number, dates, and sums in bank bills, notes, and other securities for money, and preventing forgeries, frauds, and losses, by defacing or altering the same. May 2.

To William Clerk, esq. advocate, for his method for preventing smoke, dust, and the danger of fire, and for increasing and regulating heat from stoves and chimney fireplaces for heating rooms, halls, passages, and stair-cases in public-buildings and dwelling-houses, and all other apartments where regulated heat and cleanliness are desirable, without obstructing the view of the burning fuel. May 2.

To Sebastian Erard, of Great Marlborough-Street, in the county of Middlesex, for certain improvements on piano-fortes and harp. May 2.

To John Maiben, of Perth, in the county of Perth, sadler and ironmonger, for certain apparatus for making carbonated hydrogen gas from pit-coal, and for using the same for lighting mills, factories, houses, lamps, &c. the lights being regulated by means of syphons. May 2.

METEOROLOGICAL TABLE,
BY MR. CAREY, OF THE STRAND,
For May 1810.

Days of Month.	Thermometer.			Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
	8 o'Clock, Morning.	Noon.	11 o'Clock, Night.			
April 27	44	58°	47°	30·16	67	Fair
28	47	64	48	·14	74	Fair
29	48	69	52	·05	76	Fair
30	51	68	54	29·98	86	Fair
May 1	49	67	44	·85	68	Fair
2	44	52	46	·80	45	Cloudy
3	44	49	45	·70	29	Cloudy
4	45	47	40	·76	26	Cloudy
5	40	51	36	·80	36	Fair
6	40	50	40	·80	35	Fair
7	41	48	46	·55	0	Rain
8	49	62	50	·67	51	Fair
9	52	61	50	·80	40	Cloudy
10	52	65	51	·98	42	Cloudy
11	47	55	42	30·08	43	Fair
12	42	56	45	29·97	41	Cloudy
13	50	47	47	·72	22	Cloudy
14	49	59	49	·60	51	Fair
15	49	55	50	·35	15	Cloudy
16	48	61	49	·45	25	Fair
17	49	54	52	·46	0	Rain
18	40	43	44	·56	0	Rain
19	42	59	44	·98	41	Fair
20	45	60	48	·95	47	Fair
21	57	61	52	·61	40	Fair
22	54	60	49	·87	37	Showery
23	50	60	48	30·10	42	Fair
24	49	61	47	·13	76	Fair
25	47	60	49	·05	46	Fair
26	46	68	47	29·93	61	Fair

N. B. The Barometer's height is taken at one o'clock.

ERRATUM.

Page 303, of the present volume—Article Royal Society, Mr. Groombridge's formula—for "tang. $\overline{2-3\cdot36\, r}$" read "tang. $\overline{Z-3\cdot86\, r}$"— Z representing Zenith distance.

LXI. *The Bakerian Lecture for* 1809. *On some new Electrochemical Researches on various Objects, particularly the metallic Bodies, from the Alkalies, and Earths, and on some Combinations of Hydrogen.* By HUMPHRY DAVY, *Esq. Sec. R.S. F.R.S. E. M.R.I.A.*[*]

I. *Introduction.*

I HAVE employed no inconsiderable portion of the time that has elapsed, since the last session of the Royal Society, in pursuing the train of experimental inquiries on the application of Electricity to Chemistry, the commencement and progress of which this learned body has done me the honour to publish in their Transactions.

In this communication I shall, as formerly, state the results. I hope they will be found to lead to some views, and applications, not unconnected with the objects of the Bakerian lecture: and though many of them are far from having attained that precision, and distinctness, which I could wish, yet still I flatter myself, that they will afford elucidations of some important and abstruse departments of chemistry, and tend to assist the progress of philosophical truth.

II, *Some new Experiments on the Metals from the fixed Alkalies.*

In the paper in which I first made known potassium and sodium to the Royal Society, I ventured to consider these bodies according to the present state of our knowledge, as undecompounded, and potash and soda as metallic oxides, capable of being decomposed and recomposed, like other bodies of this class, and with similar phænomena.

Since that time, various repetitions of the most obvious of the experiments on this subject have been made in different parts of Europe. The generality of enlightened chemists have expreſsed themselves satisfied both with the experiments, and the conclusions drawn from them: but as usually happens in a state of activity in science, and when the objects of inquiry are new, and removed from the common order of facts, some inquirers have given hypothetical explanations of the phænomena, different from those I adopted.

MM. Gay Lussac and Thenard, as I have mentioned on a former occasion, suppose potassium and sodium to be compounds of potash and soda with hydrogen; a similar opinion seems to be entertained by M. Ritter. M. Curaudau* affects to consider them as combinations of charcoal, or of charcoal and hydrogen, with the alkalies; and an Inquirer† in our own country regards them as composed of *oxygen* and *hydrogen.*

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[*] From Philosophical Transactions for 1810, Part I.

I shall examine such of those notions only as have been connected with experiments, and I shall not occupy the time of the society with any criticisms on matters of mere speculation.

In my two last communications, I have given an account of various experiments on the action of potassium upon ammonia, the process from which MM. Gay Lussac and Thenard derive their inferences. At the time that these papers were written, I had seen no other account of the experiments of the French chemists, than one given in a number of the Moniteur; and as this was merely a sketch, which I conceived might be imperfect, I did not enter into a minute examination of it. I have since seen a detail of their inquiry in the second volume of the Mem. d'Arcueil, a copy of which M Berthollet has had the goodness to send me, and the publication of which is dated June 7, 1809: and from this detail, it seems that they still retain their opinion; but upon precisely the same grounds as those to which I have before referred. That no step of the discussion may be lost to the society, I shall venture to state fully their method of operation, and of reasoning.

They say that they heated potassium‡ in ammonia, and they found that a considerable quantity of ammonia was absorbed; and hydrogen produced; and that the potassium became converted into an olive-coloured fusible substance; by heating this substance strongly, they obtained three-fifths of the ammonia again, two-fifths as ammonia, one-fifth as hydrogen and nitrogen; by adding a little water to the residuum, they procured the remaining two-fifths, and found in the vessel in which the operation was carried on, nothing but potash.—Again, it is stated, that by treating a new quantity of metal with the ammonia disengaged from the fusible substance, they again obtained hydrogen, and an absorption of the ammonia; and by carrying on the operation, they affirm that they can procure from a given quantity of ammonia, more than its volume of hydrogen.

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* *Journal de Physique*, June 1808.

† Nicholson's Journal, August 1809, p. 258.

‡ *Mem. d'Arcueil*, tom. ii. page 309.

Whence, they ask, can the hydrogen proceed?—Shall it be admitted that it is from the ammonia? But this, say they, is impossible; for all the ammonia is reproduced. It must then come from the water which may be supposed to be in the ammonia, or from the metal itself. But the experiments of M. Berthollet, jun. prove that ammonia does not contain any sensible quantity of water. Therefore, say they, the hydrogen gas must be produced from the metal; and as, when this gas is separated, the metal is transformed into potash, the metal appears to be nothing more than a combination of hydrogen, and that alkali.

It is obvious, that even supposing the statement of these gentlemen correct, their conclusions may easily be controverted. They affirm that all the ammonia is reproduced; but they do not obtain it without the addition of *water*. And of the oxygen which this would give to the potassium, and of the hydrogen which it might furnish, to reproduce the ammonia, they take no notice.

I have shown, by numerous experiments, many of which have been repeated before members of this society, that the results obtained, by applying heat to the fusible substance, are very different from those stated by the ingenious French chemists, when the operations are conducted in a refined and accurate manner.

In proportion as more precautions are taken to prevent moisture from being communicated to it, so in proportion is less ammonia regenerated; and I have seldom obtained as much as $\frac{1}{10}$ of the quantity absorbed. And I have never procured hydrogen and nitrogen, in the proportions in which they exist in ammonia; but there has been always an excess of nitrogen.

The processes which I have detailed in the last Bakerian lecture, and in the appendix to it, show this; and they likewise show that a considerable quantity of potassium is always revived.

I have lately performed the experiments, in a manner which I proposed, page 458 of the last volume of the Transactions [Phil. Mag. vol. xxxiv. p. 344.] and the results have been very satisfactory; as far as they relate to the question of the nature of potassium.

I employed a tube of platina bored from a single piece, which having a stop-cock and adaptor of brass, connected with the mercurial apparatus, could be used as a retort; the potassium was employed in quantities of from three to four grains, and the absorption of the ammonia conducted as usual, in a retort of glass free from metallic oxides; and in a tray of platina.

In some of the processes, in which the heat was rapidly applied, some of the gray matter, which I have formerly described as a pyrophorus, passed over in distillation, and in these cases there was a considerable deficiency of hydrogen, as well as nitrogen, in the results of the experiment; but when the heat was very slowly raised, the loss was much less considerable, and in several cases I obtained more than four-fifths of the potassium which had been employed; and very the whole of the nitrogen, existing in the ammonia t d been acted upon.

I shall give an account ie process, conducted with scrupulous attention. T meter was at $30\cdot2^{\text{in.}}$, thermometer at 54° Fahrenh hree grains and a half of potassium were heated in cubical inches of ammonia, 7·5 were absorbed, ·2 of hydrogen evolved. The fusible substance was posed to the atmosphere, but was covered with dry y, and immediately introduced into the tube; wl ith its adaptors, was exhausted, and filled with gen. They contained together $\frac{8}{10}$ of a cubical in The heat was very slowly applied by means of a fire charcoal, till the tube was ignited to whiteness. N ubical inches of gas were given off, and $\frac{1}{2}$ of a cul nch remained in the retort and adaptors. Of the 9 c inches, $\frac{1}{5}$ of a cubical inch was ammonia, 10 measures of the permanent gas, mixed with 7·5 of oxygen, and acted upon by the electrical spark, left a residuum of 7·5. The quantity of potassium formed, was such as to generate by its action upon water three cubical inches and $\frac{3}{10}$ of hydrogen gas.

Now if this experiment be calculated upon, it will be found, that 7·5 — ·2 = to 7·3 of ammonia, by its electrical decomposition, would afford about 13·1 of permanent gas, containing 3·4 of nitrogen, and 9·7 of hydrogen. But the 3·2 cubical inches of hydrogen, evolved in the first part of the process, added to the 5·8 evolved in the second part of the process, = 9; and the nitrogen in the 8·8 cubical inches of gas (or the 9 — ·2 of ammonia) will be about 3, and if we estimate ·31 of hydrogen, and ·16 of nitrogen, in the ·5, remaining in the retort; there will be very little difference in the results of the analysis of ammonia by electricity, and by the action of potassium; and calculating upon the $\frac{2}{10}$ of hydrogen pre-existing in the tube and adaptors, the loss of hydrogen will be found proportionally rather greater than that of nitrogen.

In another experiment in which three grains of potassium were employed in the same manner, 6·78 cubical inches of ammonia were found to be absorbed, and 2·48 of hydrogen only generated. The distillation was performed, the adaptors and tube being full of common air: 8 cubical inches of gas were produced; and there must have remained in the tubes and adaptors, the same quantity of residual air as in the process last described.

The 8 cubical inches of gas contained scarcely $\frac{2}{5}$ of a cubical inch of ammonia; and the unabsorbable part detonated with oxygen, in the proportion of 11 to 6, gave a residuum of 7·5.—The barometer was at 30·2$^{\text{in.}}$, thermometer at 52° Fahrenheit. Dr. Pearson, Mr. Allen, and Mr. Pepys were present during the whole of these operations, and kindly assisted in the progress of them.

Now 6·78—·4 of ammonia=6·38, and this quantity of gas decomposed by electricity, would afford 11·4 of permanent gas, consisting of 2·9 nitrogen, and 8·5 hydrogen; but there are produced in this experiment, of hydrogen, 2·48 in the first operation, and 4·28 in the second, and considering the nitrogen in the permanent gas as 3·32, ·8 must be subtracted for the common air; which would give 2·52 for the nitrogen generated; and to these must be added, the quantity of hydrogen and nitrogen in the tubes and adaptors.

The quantity of potassium regenerated was sufficient to produce 2·9 cubical inches of hydrogen.

In all experiments of this kind, a considerable quantity of black matter separated, during the time the potassium in the tube was made to act upon water.

This substance was examined. It was in the state of a fine powder. It had the lustre of plumbago, it was a conductor of electricity. When it was heated, it took fire at a temperature below ignition; and after combustion, nothing remained but minutely divided platina.

I exposed some of it to heat in a retort containing oxygen gas; there was a diminution of the gas; and a small quantity of moisture condensed on the upper part of the retort, which proved to be mere water.

I made two or three experiments, with a view to ascertain the quantity of this substance formed, and to determine more fully its nature. I found that in the process in which from three to four grains of potassium were made to act upon ammonia in a vessel of platina, and afterwards distilled in contact with platina, there were always from four to six grains of this powder formed; but I have advanced no further in determining its nature, than in ascertaining, that it is platina combined with a minute quantity of matter, which affords water by combustion in oxygen.

In the processes on the action of potassium and ammonia, in which iron tubes were used, as appears from the experiments detailed in the last Bakerian lecture, and the appendix, there is always a loss of nitrogen, a conversion of a portion of potassium into potash, and a production of hydrogen. When copper t ire employed, the hydrogen bears a smaller proporti the nitrogen; and more potassium is revived.

In these experiments, i platina has been used, there is little or no loss issium or nitrogen; but a loss smaller or greater of n.

It will be asked, On these circumstances depend? Does the affinity tain metals for potassium prevent it from gaining from ammonia, and do platina and copper combi a small quantity of hydrogen, or its basis? iere some sources of inaccuracy in those processes, ich nitrogen has appeared to be decomposed? The discuss on of these difficult problems will be considered part of this lecture, in which the nature of am ill be illustrated by some new experiments. The f the present part of the inquiry is the demonstration of a part of chemical doctrine, no less important and fundamental to a great mass of reasoning, namely, that by the operation of potassium upon ammonia, it is not *a metallic* body that is decompounded but the volatile alkali, and that the *hydrogen* produced does not arise from the potassium, as is asserted by the French chemists; but from the *ammonia*, as I have always supposed; the potassium in the most refined experiments is *recovered*, but neither the ammonia nor its elements can be reproduced, except by introducing a new body, which contains oxygen and hydrogen.

I have made an experiment upon the action of sodium on ammonia, with the same precautions as in the experiments just detailed, a tray, and the same tube of platina being employed.

3·$\frac{3}{10}$ grains of sodium I found absorbed 9·1 of ammonia, and produced about 4·5 of hydrogen, and the fusible substance, which was very similar to that from potassium, distilled, did not give off $\frac{1}{40}$ of the ammonia that had disappeared; and this small quantity I am inclined to attribute to the presence of moisture. The permanent gas produced, equalled twelve cubical inches, and, by detonation with oxygen, proved to consist of nearly two of hydrogen to one of nitrogen. Sodium was regenerated, but an accident prevented me from ascertaining the quantity.

Whoever will consider with attention, the mere visible phænomena of the action of sodium on ammonia, cannot, I conceive, fail to be convinced that it is the volatile alkali, and not the metal, which is decompounded in this process.

As sodium does not act so violently upon oxygen, as potassium; and as soda does not absorb water from the atmosphere, with nearly so much rapidity as potash, sodium can be introduced into ammonia, much freer from moisture, than potassium. Hence, when it is heated in ammonia, there is no effervescence, or at least one scarcely perceptible. Its tint changes to bright azure, and from bright azure to olive green, it becomes quietly and silently converted into the fusible substance, which forms upon the surface, and then flows off into the tray. It emits no elastic fluid, and gains its new form, evidently, by combining with one part of the elementary matter of ammonia, whilst another part is suffered to escape in the form of hydrogen.

It will not be necessary for me to enter into a very minute experimental examination of the opinion of M. Curaudau, that the metals of the alkalies are composed of the *alkalies* merely united to *charcoal;* the investigation upon which he has founded his conclusions, is neither so refined, nor so difficult, as that which has been just examined. This gentleman has been misled by the existence of charcoal, as an accidental constituent in the metals he employed, in a manner much more obvious, than that in which MM. Gay Lussac and Thenard have been misled by the moisture which interfered with their results.

M. Curaudau states, that when sodium is oxidated, carbonic acid is formed. This I have never found to be the case, except when the sodium was covered by a film of naphtha. I burnt two grains of sodium in eight cubical inches of oxygen: nearly two cubical inches of oxygen were absorbed, and soda in a state of extreme dryness, so that it could not be liquefied by a heat below redness, formed. This soda did not give out an atom of carbonic acid, during its solution in muriatic acid. Three grains of sodium were made to act upon water; they decomposed it with the phænomena which I have described in the Bakerian lecture for 1807. Nearly six cubical inches of hydrogen were produced. No charcoal separated; no carbonic acid was evolved, or found dissolved in the water. Whether the metals of potash or soda were formed by electricity, or by the action of ignited iron on the alkalies, the results were the same. When charcoal is used in experiments on potassium or sodium, they usually contain a portion of it in combination; and it appears from M. Curaudau's method of decomposing the alkalies, that his metals must have been carburets not of potash and soda, but of potassium and sodium.

M. Ritter's argument r of potassium and sodium being compounds of h is their extreme lightness. This argument I had ir easure anticipated, in my paper on the decompos he earths; no one is more easily answered. Sodiu s much more oxygen than potassium, and on the s of hydrogenation, must contain much more h yet though soda is said to be lighter than potas proportion of 13 to 17 nearly*, yet sodium is an potassium in the proportion of 9 to 7 at le

On the theory whic dopted, this circumstance is what ought to be Potassium has a much stronger affinity for ox n sodium; and must condense it much more, sulting higher specific gravity of the combinatio essary consequence.

M. Ritter has stated, all the metallic substances he tried for producing potassium by negative Voltaic electricity, tellurium was the only one by which he could not procure it. And he states the very curious fact, that when a circuit of electricity is completed in water, by means of two surfaces of tellurium, oxygen is given off at the positive surface, no hydrogen at the negative surface, but a brown powder, which he regards as a hydruret of tellurium, is formed and separates from it; and he conceives that the reason why tellurium prevents the metallization of potash is, that it has a stronger attraction for hydrogen than that alkali.

These circumstances of the action of tellurium upon water, are so different from those presented by the action of other metals, that they can hardly fail to arrest the attention of chemical inquirers. I have made some experiments on the subject, and on the action of tellurium on

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* *Hassenfratz, Annal. de Chim.* tome xxviii. p. 11.

potassium, and I find that instead of proving that potassium is a compound of potash and hydrogen, they confirm the idea of its being as yet like other metals undecompounded.

When tellurium is made the positive surface in water, oxygen is given off; when it is made the negative surface, the Voltaic power being from a battery composed of a number of plates exceeding 300, a purple fluid is seen to separate from it, and diffuse itself through the water; the water gradually becomes opake and turbid, and at last deposits a brown powder. The purple fluid is, I find, a solution of a compound of tellurium and hydrogen in water; which, in being diffused, is acted upon by the oxygen of the common air, dissolved in the water, and gradually loses a part of its hydrogen, and becomes a solid hydruret of tellurium. The compound of hydrogen and tellurium produced at the negative pole when uncombined is gaseous at common temperatures, and when muriatic acid or sulphuric acid are present in the water, it is not dissolved, but is given off, and may be collected and examined.

I acted upon potash by means of a surface of tellurium, negatively electrified, by a part of the large Voltaic apparatus lately constructed on a new plan in the laboratory of the Royal Institution, an account of which, with figures, will be found annexed to this paper. 1000 double plates were used. The potash was in the common state, as to dryness. There was a most violent action, and a solution of the tellurium, with much heat, and a metallic mass, not unlike nickel in colour, was formed; which when touched by water, did not inflame nor effervesce, but rendered the water of a beautiful purple colour, and when thrown into water entirely dissolved, making a bright purple tincture. It immediately occurred to me, that the whole of the hydrogen, which in common cases would have been furnished from the decomposition of the water, had in this instance combined with the tellurium, and that the *telluretted* hydrogen (if the name may be used) had formed with the oxidated potassium, *i.e.* the potash, a peculiar compound, soluble in water; and this I found to be the case; for on pouring a little diluted muriatic acid into the mixture, it effervesced violently, and gave a smell very like that of sulphuretted hydrogen; metallic tellurium was formed where it came in contact with the air, and muriate of potash was found dissolved in the mixture.

It seemed evident from this fact, that in the action of tellurium negatively electrified upon potash, potassium was produced as in all other cases, and that it combined with the tellurium, and formed a peculiar alloy; and this opinion was further confirmed, by the immediate action of potassium upon tellurium. When these metals were gently heated in a retort of green glass, filled with hydrogen gas, they combined with great energy, producing most vivid heat and light, and they composed an alloy of a dark copper hue, brittle, infusible at a heat below redness, and possessing a crystalline fracture. When the tellurium was in excess in this mixture, or even nearly equal to the potassium in quantity, no hydrogen was evolved by the action of the alloy upon water; but the compound of telluretted hydrogen and potash wa ed, which remained dissolved in the fluid, and v vas easily decomposed by an acid.

The very intense affinity tassium and tellurium for each other, induced me tc ive that the *decomposition of potash* might be easily d, by acting on the oxide of tellurium and potash at me time, by heated charcoal; and I soon proved t is was the case. About 100 grains of oxide of tell , and 20 of potash, were mixed with 12 grains of rnt charcoal in powder, and heated in a green gl rt; before the retort became red there was a vi tion, much carbonic acid was given off, a vivid ligh red in the retort, and there was found in it the alloy rium and potassium.

In attempting to reduc xide of tellurium by charcoal, which Mr. Hatch the kindness to give me for the purposes of these experiments, and which must have been precipitated by potash, or from a solution in potash, I found that a sufficient quantity of alkali adhered to it, even after it had been well washed, to produce an alloy of potassium and tellurium; but in this alloy the potassium was in very small quantity. It was of a steel gray colour, very brittle, and much more fusible than tellurium.

I shall not arrest the progress of discussion, by entering at present into a minute detail of the properties of the *aëriform* compound of tellurium and hydrogen; I shall mention merely some of its most remarkable qualities, and agencies, which, as will be shown towards the close of this paper, tend to elucidate many points immediately connected with the subject in question. The compound of tellurium and hydrogen is more analogous to sulphuretted hydrogen, than to any other body. The smell of the two substances is almost precisely the same[*]. Its aqueous solution is of a claret colour; but it soon becomes brown, and deposits tellurium, by exposure to air. When disengaged from an alkaline solution by muriatic acid, it reddens moistened litmus; but after being washed in a small quantity of water, it loses this property; but in this case likewise it is partially decomposed by the air in the water; so that it is not easy to say, whether the power is inherent in it, or depends upon the diffusion of a small quantity of muriatic acid through it. In other respects, it resembles a weak acid, combining with water, and with the alkalies. It precipitates most metallic solutions. It is instantly decomposed by oxymuriatic acid, depositing a film, at first metallic, but which is soon converted into muriate of tellurium[†].

As arsenic has an affinity for hydrogen, it occurred to me as probable, that it would present some phænomena analogous to those offered by tellurium, in its action upon potassium, and in its operation upon water, when electrified.

Arsenic made the negative surface, in water, by means of a part of the new battery, containing 600 double plates, became dark-coloured, and threw down a brown powder; but it likewise gave off a considerable quantity of inflammable gas.

Arsenic negatively electrified in a solution of potash, likewise afforded elastic matter; but in this case the whole solution took a deep tint of brown, and was pellucid; but it became turbid, and slowly deposited a brown powder, by the action of an acid. When arsenic was made the negative surface, in contact with solid potash, an alloy of arsenic and potassium was formed of a dark gray colour, and perfectly metallic; it gave off arseniuretted hydrogen by the action of water with inflammation, and deposited a brown powder.

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[*] In some experiments, made on the action of tellurium and potassium, in the laboratory of my friend John George Children, esq. of Tunbridge, in which Mr. Children, Mr. Pepys, and Mr. Warburton co-operated, the analogy between the two substances struck us so forcibly, as for some time to induce us to conceive that *tellurium* might contain *sulphur*, not manifested in any other way but by the action of Voltaic electricity, or by potassium; and some researches made upon the habitudes of different metallic sulphurets, at the Voltaic negative surface, rather confirmed the suspicion; for most of the sulphurets that we tried, which were conductors of electricity, absorbed hydrogen in the Voltaic circuit. The great improbability, however, of the circumstance that sulphuric acid, or sulphur in any state of oxygenation could exist in a metallic solution, which was not manifested by the action of barytes, induced me to resist the inference; and further researches, made in the laboratory of the Royal Institution, proved that the substance in question was a new and singular combination.

[†] From the results of one experiment which I tried, it seems that tellurium, merely by being heated strongly in dry hydrogen, enters into combination with it. An accident prevented me from ascertaining whether the compound so formed, is exactly the same as that described in the text.

When potassium and arsenic* were heated together in hydrogen gas, they combined with such violence as to produce the phænomena of inflammation, and an alloy was produced of the same kind as that formed by means of the Voltaic battery.

As tellurium and arsenic both combine with hydrogen, it appeared to me probable, that by the action of alloys of potassium, with tellurium [illegible] arsenic, upon ammonia, some new phænomena wo [illegible] e obtained, and probably, still further proofs of the [illegible] position of the volatile alkali, in this process afford [illegible] d this I found was actually the case.

When the easily fusibl [illegible] of tellurium with potassium, in small quantity, w [illegible] ated in ammonia, the surface lost its metallic spl [illegible], and a dark brown matter was formed, which gave [illegible] ia by exposure to air; and the elastic fluid, which [illegible] nerated in this operation, consisted of four-sixths [illegible] en, instead of being pure hydrogen, as in the case [illegible] ction of potassium alone.

The alloy of arsenic and [illegible] ssium, by its action upon ammonia, likewise produ [illegible] gas which was principally nitrogen; so that if it be [illegible] hat the metal, and not the volatile alkali, is decomp [illegible] processes of this kind, it must be considered in some cases as a compound of nitrogen, and in others a compound of hydrogen; which are contradictory assumptions.

None of the chemists who have speculated upon the *imaginary hydrogenation* of potash, as far as my knowledge extends, have brought forward any arguments of analysis, or synthesis. Their reasonings have been founded, either upon distant analogies, or upon experiments in which agent, which they did not suspect were concerned. No person, I believe, has attempted to show that when potassium or sodium is burnt in oxygen gas, water is formed, or that water is generated when potassium decomposes any of the acids [*]; and no one has been able to form potassium, by combining hydrogen with potash. I stated in the Bakerian lecture for 1807, that when potassium and sodium were burnt in oxygen gas, *the pure* alkalies were formed in a state of extreme dryness; and that 100 parts of potassium absorb about 18 parts of oxygen, and 100 parts of soda about 34 parts. Though, in the experiments from which these deductions were made, very small quantities only of the materials were employed; yet still, from frequent repetitions of the process, I hoped that they would approach to accuracy; and I am happy to find that this is the case; for the results differ very little in some experiments which I have made upon considerable portions of potassium and sodium, procured by chemical decomposition.

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* In reasoning upon the curious experiment of Cadet, of the production of a volatile pyrophorus by the distillation of acetite of potash, and white oxide of arsenic, Fourcroy Connais. Chem. tom. viii. p. 197, I conceived it probable, that this pyrophorus was a volatile alloy of potassium and arsenic. But from a repetition of the process I find, that though potash is decompounded in this operation, yet that the volatile substance is not an alloy of potassium, but contains charcoal and arsenic, probably with hydrogen. The gases not absorbable by water given off in this operation, are peculiar. Their smell is intensely fœtid. They are inflammable, and seem to contain charcoal, arsenic, and hydrogen: whether they are mixtures of various gases, or a single compound, I am not at present able to decide,

When potassium is burnt in trays of platina, in oxygen gas that has been dried by ignited potash, the absorption of oxygen is about $\frac{11}{10}$ of a cubical inch for every grain of the metal consumed; and when sodium is burnt in a similar manner, about a cubical inch is taken up for every grain [†]. The alkalies so formed, are only imperfectly fusible at a red heat; and do not, like the easily fusible alkalies, give indications of the presence of moisture.

M. D'Arcet has shown by some very well conducted inquiries, that potash and soda [‡], in their common state, contain a considerable proportion of water; and M. Berthollet concludes, that 100 parts of potash, that have been kept for some time in fusion, contain 13·89 parts of water, which is lost when the alkali enters into combination with muriatic acid; and the same sagacious observer, from some very minute experiments, infers, that muriate of potash, which has been ignited, contains in the 100 parts 66·66 potash, and 33·34 muriatic acid, a determination which differs very little from that of Bucholz.

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[*] When in October 1807, I obtained a dark-coloured combustible substance from boracic acid, at the negative pole in the Voltaic circuit, I concluded that the acid was probably decomposed, according to the common law of electrical decomposition. In March 1808, I made further experiments on this substance, and ascertained that it produced acid matter by combustion; and I announced the decomposition in a public lecture delivered in the Royal Institution March 12. Soon after I heated a small quantity of potassium, in contact with dry boracic acid, no water was given off in the operation, and I obtained the same substance as I had procured by electricity. MM. Gay Lussac and Thenard have likewise operated upon boracic acid, by potassium, and they conclude that they have decompounded it; but this does not follow from their theory, unless they prove that water is given off in the operation, or combined with the borate of potash: the legitimate conclusion to be drawn from the processes, on their hypothesis, was, that they had made a hydruret of boracic acid.

[†] The quantities of gas given out by the operation of water, are in a similar ratio. See page 43 of the last Bakerian lecture [Phil. Mag. vol. xxxiii, p. 432,] and page 26 of this paper [p. 407 and 408 preceding.]

[‡] *Annales de Chimie*, Nov. 1808, page 175.

To determine the relation of the dryness of the potash, formed from potassium, to that which has been considered as freed from the whole or the greatest part of its water, in muriate of potash, I made several experiments. I first attempted to convert a certain quantity of potassium into potash, upon the surface of liquid muriatic acid; but in this case the heat was so intense, and hydrogen holding potassium in solution was disengaged with so much rapidity, that there was a considerable loss of alkali; yet even under these circumstances I obtained from ten parts of potassium 17·5 of dry muriate of potash. The most successful and the only mode which I employed, that can be entirely depended upon, was that of converting potassium into muriate of potash, in muriatic acid gas. I shall give the results of two experiments made in this manner: five grains of potassium inserted in a tray of platina, were made to act upon 19 cubical inches of muriatic acid gas, that had been exposed to muriate of lime; by the application of a gentle heat, the potassium took fire, and burnt with a beautiful red light *; and the whole mass appeared in igneous fusion; a little muriate of potash in the state of a white powder, sublimed and collected in the top of the vessel in which the experiment was made. Nearly 14 cubical inches of muriatic acid gas were absorbed, and about five of hydrogen were produced. The increase of weight of the tray was about 4·5 grains; and it did not lose any weight by being ignited.

The second experiment was conducted with still more attention to minuteness. Eight grains of potassium were employed; above 22 cubical inches of muriatic acid gas were consumed; the potassium burnt with the same brilliant phænomena as in the last experiment, and the increase of weight of the tray was 6½ grains. The muriate of potash was kept for some minutes in fusion in the tray,

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* As a retort exhausted of common air was used, the small quantity of residual common air may have been connected with this vividness of combustion.

till a white fume began to rise from it, but it did not lose the $\frac{1}{10}$ of a grain in weight. After the muriate of potash had been washed out of the tray, and it had been cleaned and dried, it was found to have lost about a third of a grain, which was platina in a metallic state, and that had alloyed with the potassium where it was in contact with the tray, during the combustion. There was no appearance of any water being separated in the process. A little muriate of potash sublimed; this was washed out of the retort, and obtained by evaporation: it did not equal $\frac{1}{8}$ of a grain.

Now if the data for calculation be taken from this last experiment, 8 grains of potassium will combine with 1·4 grains of oxygen, to form 9·4 grains of potash, and 6·6—1·4=5·2, the quantity of muriatic acid combined with the potash, which would give in the 100 parts in muriate of potash, 35·6 of acid, and 64·4 of potash; but 35·6 of muriatic acid, according to M. Berthollet's estimation, would demand 71·1 of alkali, in the state of dryness in which it exists in muriate of potash, and 71·1—64·4=6·7— so that the potash taken as a standard by M. Berthollet, contains at least 9 per cent. more water than that existing in the potash formed by the combustion of potassium in muriatic acid gas, which consequently may with much more propriety be regarded as the dry alkali *.

After these illustrations, I trust the former opinions which I ventured to bring forward, concerning the metals of the fixed alkalies, will be considered as accurate, and that potassium and sodium can with no more propriety be considered as *compounds*, than any of the common *metallic substances;* and that potash and soda, as formed by the combustion of the metals, are pure metallic oxides, in which no water is known to exist.

Thess conclusions must be considered as entirely independent of hypothetical opinions, concerning the existence of hydrogen in combustible bodies, as a common principle of inflammability, and of intimately *combined water*, as an essential constituent of acids, alkalies, and oxides: this part of the inquiry I shall reserve for the conclusion of the lecture, and I shall first consider the nature of the metal of ammonia, and the metals of the earths.

[To be continued.]

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* Consequently M. Berthollet's fused potash must contain nearly 23 per cent. of water. From my own observations I am inclined to believe, that potash kept for some time in a red heat contains 16 or 17 per cent. of water, taking the potash formed by the combustion of potassium as the dry standard.

LXII. *Observations respecting a New Scale for the Thermometer.* By RICHARD WALKER, *Esq. of Oxford.*

THERE are four different thermometers in use at this time, viz. Fahrenheit's, Reaumur's, Celsius's, and De l'Isle's.

Fahrenheit's scale, which is used in Great Britain, has the *zero*, or commencement of the scale, placed at 32 degrees below the freezing point, and 212 is the boiling point of water.

Reaumur's, or the French scale, modified by De Luc, in which the 0 is placed at the freezing point, and from thence to the boiling point are 80 de es*.

Celsius's scale is used in S len; in this 0 is placed at the freezing point, and from ence to the boiling point are 100 degrees.

De l'Isle's scale is used i ssia; in which 0 is placed at the boiling point of wat l the freezing point is 150.

With respect to Fahrenh scale, it may be considered, now, as having no foundat... whatever in any principle, and is in fact upon that ac nt universally disapproved of, and evidently upon the d ne.

Reaumur's scale, and othe n which the freezing point of water has been adopted r placing the 0, or commencement of the scale, has been hitherto deemed the least objectionable, on account of its being an invariably fixed point.

In the construction of the nometers two fixed points are required: accordingly, the scales of all thermometers have hitherto been, and probably ever will be, adjusted by means of the freezing and boiling points of water; the latter, as is well known, being an equally fixed point with the former, under certain circumstances well known to the philosopher and the artist.

The freezing and boiling points of water, then, may be considered as applicable only to the due arrangement of the proportions and precision of a thermometrical scale, and consequently either of *them* unfit for the place of commencement of it: hence the place where 0 should be placed is yet a desideratum.

Considering the thermometer as a measure of heat, leaving the negative term *cold* out of the question, the proper place for 0 would undoubtedly be that point at which heat commences; but the physical causes, which are known to exist, to prevent that point being ever ascertained, exclude entirely the hope of fixing it there; to say nothing of the inconvenience which must arise from the scale being incumbered with a multiplicity of figures, at that part which is most familiar and in most frequent use.

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* A thermometer called the centigrade has lately been introduced in France, which is in fact no other than that of Celsius, with the addition of decimal divisions.

Hence we are under the necessity here, as in many other instances, of using the positive and negative signs.

Having been for a very long time engaged in thermometrical experiments and observations,—the imperfection of all the scales in use, and the consequent dissatisfaction of various persons respecting them, frequently occurred to me; and it is long since that I was impressed with the opinion, strengthened by that of several of my friends, of the preference which was due to the one I am now about to mention.

It has been ascertained by physiologists as a fact, and of which I have perfectly satisfied myself, by repeated experiments on others as well as myself, that 62 of Fahrenheit is that point at which the human body in a state of health *is unconscious of either heat or cold*, that is, in a state of rest, or when free from any considerable bodily exertion; and this is really the case at all seasons of the year, in this climate, and probably in all other climates; for the temperature of the human body, or blood heat, as it is called, is determined every where to be 98, and it has been found that the vital functions have the power of regulating the sensation of heat; so that any temperature *above* 62 of Fahrenheit, under ordinary circumstances, shall give a sensation of *heat*, and any temperature *below* 62 of Fahrenheit a sensation of cold*.

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* Dr. Cullen, in speaking of the influence of temperature on the human body in this climate, says: "If the temperature at any time applied is under 62 degrees, every increase of temperature applied will give a sensation of heat; but if the increase of temperature does not arise to 62 degrees, the sensation produced will not continue long, but be soon changed to a sensation of cold. In like manner, any temperature applied to the human body lower than that of the body itself, gives a sensation of cold; but if the temperature applied does not go below 62 degrees, the sensation of cold will not continue long, but be soon changed to a sensation of heat." Cullen's Practice of Physic, vol. i. 1784, p. 91, 92.

This point is so nicely balanced, and so accurately just, viz. at 36 degrees below blood-heat, or 98, that a variation of one, or at most two degrees, above or below that point, actually produces a sensation of heat or cold; and which, by experiment properly made, would be found to be equally the case in the torrid and frigid zone as in temperate climates: hence this point may be considered as the actual, or natural *nought*, with respect to heat and cold on the thermometer.

Upon this principle the present scale is founded, which I cannot help thinking is perfectly unexceptionable, being fixed on an unalterable basis; and that in a thermometer thus constructed, there will be ever a coincidence in the central point of this thermometer with that precise temperature which is most congenial to the feelings of the human body, and prove universally and permanently a correct standard for reference, and consequently, I should think, render this instrument more intelligible and interesting, and of course more extensively useful.

With respect to the divisions, I adopted those of Fahrenheit from an opinion of that being the fittest, considering those of Reaumur, the centigrade, &c. as being too few, and decimal divisions un[illegible]cessary in a thermometrical scale.

Hence it will follow, th[illegible]eing placed at 62° of Fahrenheit, 150° will be the b[illegible], and minus 30° the freezing, points of water; and [illegible]ther points on Fahrenheit's scale may be reduced to [illegible] by subtracting 62 for any degree *above* 0 of Fahrenhe[illegible] and adding 62 for any degree *below* 0.

This thermometer may [illegible]nsidered as particularly appropriate to the purpose [illegible]lating the heat of rooms, &c. and showing the ex[illegible]ations in the air from that temperature which is at a[illegible]es most congenial and salutary to the human body.

The difficulties that may occur in the introduction of a new thermometrical scale, are not more, perhaps less, than in many other innovations, viz. the New Style—the New Chemical Nomenclature, &c.

The mechanical method of graduating this thermometer is thus: the freezing and boiling points of water having been taken in the usual way, the freezing point is to be marked 30, and the boiling point 150; this space is then to be divided into six divisions, or parts =30° each, and the 0 placed at the first division above 30, or the freezing point; the scale is then to be completed by marking in the tenths above and below 0, continuing the graduation to any desired extent.

Sir Isaac Newton, who I believe was the person that originally fixed on the freezing and boiling points of water, *conjointly*, for the adjustment of a thermometrical scale, places his zero at the freezing point, from a notion of that being the utmost degree of cold; and Fahrenheit, for a similar reason, placed his zero afterwards at 32 degrees below the freezing point, the degree of cold which he had produced by a mixture of ice and salt, which he at that time believed to be the greatest degree of cold in nature *.

There is no scale which I have met with, which seems to have been founded upon the principle I have adopted: thermometers indeed have been constructed in England, for particular purposes, in which 0 was placed at what was supposed to be the *middle state of the air* in this climate; but this principle is of too local and vague a nature to merit attention.

I know of one objection, only, which can be started against the adoption of the alteration in the scale I have proposed, viz. the too frequent occasion, as might be urged, for the use of the plus and minus characters; but the fact is, that in a scale founded upon this principle, there is less occasion than ever for the use of either of them, as must be apparent to any one upon the least reflection.

For meteorological observations this scale will be particularly appropriate; the zero in this instance being the mean temperature of the greatest heat and greatest cold experienced in the hottest and coldest climates, *collectively*, as well as in the temperate climates †.

At Quito in Peru, which is peculiarly situated between the extremes of heat and cold, the temperature of the air is uniformly, or with very little variation, throughout the year at 62 of Fahrenheit; and this is considered to be the healthiest spot in the world ‡.

If we had no means of correcting or regulating the temperature of our apartments by fire, we should find a few degrees, viz. ten *below* the point of 62, much more uncomfortable than the same difference *above* 62; and in a difference of thirty degrees from that point the cold would be intolerable, in the first instance; whereas at thirty *above*, no considerable inconvenience is experienced: but the difference would be exactly the inverse of each other, were it not that the human body, from a well-known provision in the animal œconomy, has the innate power of resisting, or rather of diminishing, the effect of extreme heat to a certain extent.

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* Sir Isaac Newton's thermometer was constructed in 1701, and Fahrenheit's in 1724.

† In some parts of Africa the thermometer rises sometimes up to 112 *above* 62 of Fahrenheit, viz. 174, and in some parts of North America it sometimes sinks to 112 *below* 62 of Fahrenheit, viz. —50. A similar circumstance takes place in the temperate or middle climates; thus in England the thermometer rises sometimes to 126, and sometimes sinks to — 2, viz. a difference, each way, of 64 degrees.

‡ The mean annual temperature of Quito is 62; and the utmost limits of variation throughout the year are from three or four degrees *below* that point, to as many degrees *above* it.

My chief object in the present paper has been to inforce the propriety of the alteration suggested, in the place of the *zero* in a thermometrical scale. With respect to the fittest mode of division to be adopted, I have preferred that of Fahrenheit, for the reason above stated; but since the mode of decimal divisions seems at the present time to be gaining ground, I have exhibited a scale of the centigrade division likewise in the annexed table. (See Tab. II.)

It will be apparent, [illegible] ting the centigrade division in the stead of Fah [illegible] that there will be a difference, occasionally, an [illegible] to ⅓d of a degree, between them ; but as the *three l* [illegible] *points* correspond precisely, so slight a variation in t [illegible] rs is of little or no consequence; and indeed all [illegible] nts cannot be perfectly accurate upon any uniform [illegible] thus, the precise point at which quicksilver freezes [illegible] scertained to be — 38 and ⅔ of Fahrenheit, but is [illegible] t —39.

There are four differ [illegible] rtions of the scale, which I should recommend to [illegible] ccording to circumstances; viz. 1st, a portable sca [illegible] ding from —30 to +30; 2dly, one for ordinary m [illegible] gical use in this climate, extending from —70 [illegible] *; 3dly, from —100 to +150 †; and 4thly, a t [illegible] eter of *coloured alcohol*, extending from — 162 to + [illegible]

I flatter myself that I have adduced sufficiently satisfactory reasons for the adoption of the scale I have here suggested; and, when the usual prejudices for an old system have subsided, and the apparently insurmountable difficulties attending the introduction of a new one are conquered, that the one I have now offered to consideration will be admitted to be perfectly reconcileable in a philosophical view; convenient and useful in application; not liable to be affected by new discoveries or fresh theories in future; and, in short, a scale dictated as it were by nature, for universal application.

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* A local scale of this kind in every climate would be particularly useful in the place itself, and interesting every where else.

† The freezing point of quicksilver upon this scale is —101; but as quicksilver ceases to measure temperature at —100, the scale of a quicksilver thermometer may terminate there.

‡ Quicksilver is preferable to alcohol for thermometers, as far as its range goes, for several reasons; but, principally, because it is found to be more uniform in its expansion by heat than alcohol or spirit.

P. S.—Before I became acquainted with the circumstance which induced me to adopt the scale which I have now proposed, I endeavoured, with a view of placing the zero at the lowest natural point, to obtain, by inference, the utmost degree of *natural cold*.

In order to effect this, I formed the following table, which is deduced from Mr. Kirwan's table of the *mean annual temperature* of every latitude; in which, of course, the latitudes within nine or ten degrees of the pole were deduced, like mine, by inference drawn from the others: and I presume, by comparing the results in this table with actual observations as far as these have gone, they will be found to accord sufficiently, to show that the principle I have adopted, in deducing the results, may be relied on. (See Tab. I.)

Hence it appears, if the results in this table are admitted to be correct, that the scale would commence at 68 degrees below the present 0 of Fahrenheit, or 100 degrees below the freezing point of water; and that any greater degree of cold than this, which already has or may hereafter be produced by art, would, *alone*, require the *minus* sign.

The greatest degree of artificial cold hitherto upon record is —91 of Fahrenheit: how far future experiments may carry this point, or whether there be a *finite point* attainable by art, is perhaps impossible to say.

Knowing that cold is produced by adding snow to *alcohol*, and presuming that alcohol is the last liquid in nature which would freeze, or whose power of producing cold with ice reaches to a lower temperature than any other substance, I formerly entertained a hope of ascertaining the *ultimate point* to which this could possibly be carried; but partly on account of the difficulties of such an undertaking, but more especially in consequence of other pressing avocations which deprive me of the opportunity, I have relinquished my intention, at least for the present.

The method I should have pursued would have been to have formed mixtures of alcohol and nitric acid, increasing the proportion of the alcohol as the temperatures at mixing became lower; finally, using alcohol alone with snow, till I should arrive at the points where alcohol itself ceased, when mixed with snow, to produce cold.

The attempts hitherto made for ascertaining what is usually called the *natural zero*, carry that point so extremely low, and the results are so very discordant with each other, that any idea of fixing the 0 there, I should think, must be entirely relinquished.

With respest to the freezing point of mercury being, as has been suggested by several philosophical persons, a fit place for the zero, it perhaps may be sufficient to observe, that this would be adapting a scale of heat to a *particular thermometer*, instead of applying a thermometer to a scale of temperature.

The table of temperatures applies more particularly to the different degrees of latitude specified, *from the equator to the north pole in the eastern hemisphere*, where the gradations or diminutions of hea[illegible]re tolerably uniform. In the western hemisphere, the [illegible]inutions of heat from the equator to the north pole [illegible] found, from causes well known, to be very irregula[illegible] anomalous; and in general the cold is considerably [illegible] on the same parallels of latitude in this, than in th[illegible]rn hemisphere.

From the equator proceed[illegible]towards the *south pole*, the diminutions of heat are foun[illegible] be tolerably uniform; the chief difference consisting [illegible] the cold being somewhat *greater* in the higher latitu[illegible]n the same parallel, than on the north side of the eq[illegible]

Notwithstanding the ir[illegible]rity as to diminution of temperature which is observed in the latitudes between the equator and the polar [illegible]ons in the western hemisphere, and the difference b[illegible] mentioned on the south of the equator; there is good reason to suppose that, *at the poles*, as at the equator, the variation of temperature is not considerable, because *the maximum of cold* (or minimum of heat) is *at the poles*, as the *maximum of heat* is at the equator*.

Hence, although every latitude between the equator and the poles may be liable to considerable anomalies or irregularities, with respect to heat and cold, according as they are more or less exposed, so as to receive the currents of air from higher or lower latitudes; yet at those two points where there is *only one of these causes* operating upon each; the variations in the temperature, at the same seasons, in different years, may be but small, comparatively with the others, particularly the middle latitudes.

In latitudes, therefore, even at some distance from the poles, (as in latitudes at some distance from the equator,) the temperature may be nearly or quite the same as at the poles: thus, at Albany Fort, Hudson's Bay, N. latitude 52, W. longitude 82, the thermometer has sunk to —50, which is the greatest degree of natural cold ever *observed;* and is within eighteen degrees of that which I have *estimated* to be the *greatest natural cold.*

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* The maximum of cold at the poles is perhaps *more complete* than the maximum of heat, from a well-known cause, is at the equator: viz. the sun's annual path, respectively to the earth, being *not confined to the line of the equator*.

TABLE I.

A Table of the Temperatures of different Latitudes.

Latitude.	Kirwan's Table.	Mean Temperature.	Greatest Cold.	Greatest Heat in Shade.	Greatest Heat in Sun.	Series of Ninety.	Series of Nine.	Series of Nine.	Series of Nine.
90	31·	31·	—64	52	78	95	21	26	4
80	32·6·	33·	—52	55	82	85	22	27	6
70	37·2·	37·	—38	60	88	75	23	28	8
60	44·3·	44·	—21	68	97	65	24	29	10
50	52·9·	53·	— 2	78	108	55	25	30	12
40	62·	62·	+17	88	119	45	26	31	12
30	70·7·	71·	+36	98	130	35	27	32	10
20	77·8·	78·	+53	106	139	25	28	33	8
10	82·3·	82·	+67	111	145	15	29	34	6
0	84·	84·	+79	114	149	5	30	35	4
A	B	C	D	E	F	*d*	*e*	*f*	*g*

Nota bene.—A is the latitude, in tenths of degrees, from the equator to the pole.

B is Kirwan's table of the *mean annual temperature* of each latitude in A: the heat increasing, *but in a diminishing ratio*, from the middle latitude, viz. 45, the *mean annual temperature* of which is 57·5 to the equator; and the cold increasing, *but in a diminishing ratio,* from latitude 45 to the pole.

C is Kirwan's table B reduced to the nearest whole numbers.

D is the *greatest cold* of each latitude A, deduced by subtracting the number in *d*, from the parallel number in C, up to latitude 40, and from thence up to latitude 90, *subtracting* the number in C from the number in *d;* the first producing the plus degrees, and the latter the minus degrees.

E is the *greatest heat in the shade* of each latitude A;

deduced by adding the number in *e* to the parallel number in C.

F is the *greatest heat in the sun* of each latitude A, deduced by adding the number in *f* to each of the parallel numbers in E.

d is an *increasing* series of *ninety*, in number corresponding with the degrees of latitude from 0 to 90, in column A, viz. from 5 to 95.

e is a *decreasing* series of *nine*, in number from latitude 0 to 90, viz. from 30 to 21.

f is a *decreasing* series of *nine*, in number from latitude 0 to 90, viz. from 35 to 26.

g is a *decreasing* series e in number from the middle latitude of 45 up to 90. the same down from latitude 45 to 0— latitude 45 13.

The temperatures mark columns D, E, F, give the temperatures of *ordinary* in order to give the defect or excess of *extraordinary* *s* the parallel number in *g* must be *subtracted*, or to either of the columns in D, E, F.

Mr. Kirwan has given *an annual temperature* of every latitude, from the to the north pole: in this table *each tenth* only is ut the temperature of each intervening latitude ma ained easily by calculation from those given, viz, b ng one-tenth for each degree of latitude, in the c *d, e, f, g*.

N. B. The thermomet aking observations, is supposed to be placed at ab 6 feet from the ground, and perfectly insulated or detached from any body which can cause reflected heat, when placed in the sun; and for observations in the shade, placed in a north aspect, where the sun never reaches.

Hence, according to this calculation, the greatest cold at the poles, or the *greatest natural cold*, is at —68 of Fahrenheit, or 100 degrees below the freezing point of water; therefore, in order to reduce Fahrenheit's scale to this, 68 must be *added* to every degree of Fahrenheit's *above* the 0; and for any degree *below* the 0 of Fahrenheit, that number must be *subtracted* from 68.

It might be expected, as the gradations in the above table are uniform, and calculated from the equator to the poles, that the temperatures in this table should correspond with considerable exactness with actual observations in every instance; but it should be recollected, that a difference must occasionally arise from various causes, viz.

difference of elevation; vicinity to, or distance from, the sea†; the intervention of mountainous and woody countries, intercepting the currents of hot or cold air, from lower or higher latitudes‡; difference of soil, as to its power of absorbing, accumulating, or reflecting heat, &c. All of which irregularities, however, it might perhaps be possible to calculate and allow for.

From the equator to 23½ degrees on each side of it, the progressive variations in temperature differ somewhat from the other latitudes; this is allowed for in the columns of the mean annual temperatures, and likewise in the three succeeding columns, which may be considered as *emanations* from the first.

I shall now conclude by presenting the following Table, (*see next page*) which exhibits a comparative view of the scale of Fahrenheit's thermometer with mine.

Explanation of the four Columns.

Nota bene.—A is my proposed scale, having the zero at mean temperature.

B is Fahrenheit's scale.

C is the centigrade measure, adapted to column A.

D is my former scale, having the zero at the point I have estimated to be *the greatest natural cold.*

The four additional points marked thus * are appropriate to this climate, viz. the *greatest degree of cold,* which was observed on the morning of December 25, 1796: and the *two greatest degrees of heat,* viz. in the *shade* and in the *sun,* observed in the afternoon of July 13, 1808.—The *temperature of springs,* means of course the *constant* temperature of ordinary (unmediated) springs, which in all climates corresponds pretty exactly with the mean annual temperature of the place, which in London is 51·9.

N. B. Thermometers have a point marked *temperate,* viz. at 56 of Fahrenheit; but 57½ is the *middle point* of the scale, that being the mean annual temperature of latitude 45.

RICHARD WALKER.

Oxford, May 31, 1810.

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† Hence it follows that in islands, particularly *small islands,* surrounded by a large extent of sea, the winters are *warmer,* and the summers *colder,* than on continents.

‡ The winters in those parts of North America, which have been cultivated, are much less rigorous now than formerly, principally in consequence of the destroying of forests, &c. which present several sources of cold, amongst which *evaporation* is no inconsiderable one.

TABLE II.

A Table of the most essential Points in a Scale of Heat.

W.		F.	W.	W.
+150	Water Boils.	+212	+100	280
+114	Spirit Boils.	+176	+76	244
+64	* Greatest Solar Heat in England.	+126	+43	194
+50	Fever Heat.	+112	+33	180
+36	Blood Heat.	+98	+24	166
+30	* Greatest Shade Heat in England.	+92	+20	160
+14	Summer Heat.	+76	+9	144
0	Mean Temperature.	+62	+0	130
−10	* Temperature of Springs in England.	+52	−7	120
−30	Water Freezes.	+32	−20	100
−64	* Greatest Cold in England.	−2	−43	66
−101	Quicksilver Freezes.	−39	−67	29
−112	Greatest Natural Cold observed.	−50	−75	18
−130	Greatest Cold Natural.	−68	−86	0
−153	Greatest Cold Artificial.	−91	−102	−23
	A	B	C	D

LXIII. On

LXIII. *On the Properties of Furze or Whins.* *By Major* SPENCER COCHRANE, *of Muirfield-House, near Haddington* *.

SIR, THE Society having honoured me by publishing in their 25th volume my communication, stating the advantages arising from the culture of poppies, and that seven ounces of fine salad oil were furnished by expression from two pounds of the seed; I now beg leave to add, that I am informed, considerable quantities of poppy seeds have been lately bought up, in different parts of the country, and the expressed oil from them sold at the price of Florence oil; and that emulsions made from poppy seeds answer in every respect the purposes of those made from almonds.

The following communication may perhaps be deemed worthy the notice of the society; it relates to the use of Whins or Furze. Its utility as food for cattle has been long known, though probably not snfficiently appreciated; but as a medicine, I never till within a few years heard of it. My information was first received from a gentleman who has been an officer in the army, a friend and relation of mine; he is seventy-five years of age, and in good health, and what he says may be depended upon. In October 1806, he informed me that his sight had been much strengthened by drinking an infusion of whin or furze blossoms, dried in the sun in summer. The infusion is made from a tea-cup full of the blossoms, in a tea-pot in the manner of tea, and the dose half a tumbler at night; that he never had a cough since he first used it, which was fifty years ago; it acts as a diuretic, and by perspiration, and, when the dose is increased, promotes sleep. In October 1808, he informed me that he still continued the use of the whin-tea, that he had no cough, and that his skin was remarkably fine and soft, which he attributes to its use.

I have also used the whin blossoms with good effect myself, and can safely recommend them.

My friend supposes the young shoots of furze may answer if the blossoms cannot be got; he informs me, that when an epidemical cold came from Germany, and destroyed many horses in England, the east wind continued six weeks, and the infection came over to Ireland, where he had the care of a troop, in so poor a village that he could get neither bran nor malt for mashes, which were ordered for the horses with sulphur, after bleeding: That he ordered the men to cut furze, and ordered them to give it to the horses, after they had beat it well on the pavement: that at first they had to mix it with oats, but that in two days the horses devoured it like clover. That by these means he recovered them all, though every other troop lost two or three; and that his was the only troop in good condition at the review. I remain with esteem, sir,

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* From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, for 1809.

Your sincere and humble servant,

Muirfield, Jan. 22, 1809. **Spencer Cochrane.**

To C. Taylor, M.D. Sec.

LXIV. *Additional Observ*[illegible] *for the Purpose of ascertaining the Value of g*[illegible] *Timber at different and distant Periods of Time* [illegible] Mr. Charles Waistell, *of High Holborn, Lond*[illegible]

Sir, In the Society's last [illegible]e, under the head of Agriculture, are some tables [illegible] servations of mine, on the growth of timber[†]; and [illegible] given one instance of six acres of very bad land, [illegible] with Scotch firs under my directions, which at 29 y[illegible]wth, and at the small price of 1*s.* a foot, had paid the [illegible]er 5*l.* per acre per annum compound interest. M[illegible] in communicating these tables, observations and [illegible] the public, was to promote the planting of inferior a[illegible]nost useless soils, in order to obtain from them timber of our own growth, sufficient for at least many of the purposes for which foreign timber is imported at an immense annual expense. For instance, much of such inferior soils will be found on Connock Heath in Staffordshire, on the moor lands in the north of that county, and on the moors in Derbyshire, Yorkshire, and northwards to Scotland, also on Bagshot Heath, Salisbury Plain, the heaths and wastes in Sussex, Hampshire, and Dorsetshire, and in many other counties in England, and also in Scotland and Wales. In that paper I suggested, that information of very great value on the subject of planting, might be obtained from noblemen and gentlemen to whom the Society had given medals and premiums for planting trees, if they would communicate to the Society their subsequent observations on such plantations;

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[*] From *Transactions of the Society for the Encouragement of Arts, Manufactures, and Commerce*, for 1809.

[†] See Philosophical Magazine, vol. xxxiii.

tations; but it is to be regretted that the Society have not yet received any communications on the subject.

I have been solicited to republish my tables, &c. by different gentlemen, who I have reason to believe are very competent to judge of them, and I have in consequence several additional tables in forwardness with this view; but, previous to such publication, I wish to be furnished with a great variety of facts as to the growth of timber, and the management of plantations; and I shall feel much obliged to any persons who will have the goodness to send me their observations and statements of facts, respecting the growth and management of plantations; or, if they prefer it, they may send them to you at the Society of Arts, &c. The names of those who send such statements will be either given or suppressed as may be requested.

I will recapitulate nearly in the words of my former communication, several particulars on which information is wanted. It would, for instance, be desirable to have the nature of the soil and under strata described, on which plantations have been made; its value per acre; the mode in which it was prepared for planting; the sorts of trees planted thereon, and which of them were found best suited thereto; the distances at which the trees were first planted; at what periods they were thinned, and how many cut out at each thinning; and their measure and value; the present height, distance, measure and value of the trees now growing on an acre; what distances are found most advantageous; also to what proportion of their heights they should be pruned up, and the best and most expeditious mode of performing the operation.

Great loss is frequently sustained by omitting to thin plantations properly, and in due time, but I am not in possession of facts to calculate with accuracy what this loss may be; I will however venture to give a short statement of some calculations I have made, as to the loss that would now be sustained by letting trees grow to a great age.

In Miller's Gardener's Dictionary, it is stated that in a fall of oak timber in Lord Bagot's woods, Mr. Marshall counted the rings of one tree, which was sound at the butt, and found the number to be about 200. Its bole was 22 feet long, and 108 inches in circumference in the middle. Its contents 110 feet, which at 2*s*. amounts to 11*l*. I think it was last year, that a fine sound oak-tree was cut down, between Shrewsbury and Oswestry in Shropshire, of 300 years of age, and sold by auction for 52*l*. 5*s*.—And under my direction, many oak trees were cut down, some years ago, that could not be less than 300, and some of them probably 400 years of age, and even more. In Hunter's Evelyn's Silva is given the circumference of 10 trees, and not one of which was probably less than 500, and some of them probably 1000, years old.

Lord Bagot's tree of 200 years old, above mentioned, would, at the present price of 3*s.* a foot, be worth 16*l.* 10*s.* Supposing that 3*s.* a foot should continue to be the price of oak-timber for the next 200 years, we will inquire what sum might be raised by growing four oak-trees in succession, upon the same spot of ground, each tree to be cut down when 50 years of age, and that their boles should be of the same length as that of Lord Bagot's, viz. 22 feet.

I fix on fifty years of age, s being convenient for my calculation; and nearly the m t profitable period at which to cut down trees of 22 feet l , which have grown at the medium rate of one inch i cumference, and 12 inches in height annually.

After its 52d year, *such* a *le* ceases increasing, after the rate of 5 per cent. per ann n*: but the whole tree, including the top part above the le, may continue increasing after that rate until its 61 year†.

I do not fix on 50 years of ge as being the most profitable age at which to cut c n trees; probably 60 or 70 years of age would in some in ances be preferable. Supposing an oak-tree has increased as above mentioned, its bole of 22 feet would, at 50 years of age, measure 39 inches in circumference at middle, and one-fourth of this, namely 9¾ inches, squ and multiplied into 22 feet, its length gives 14 feet 6 inches for its contents, which at 3*s.* a foot, its present value, amounts to 2*l.* 3*s.* 6*d.* Supposing 2*l.* 3*s.* 6*d.* to be the value of each of the four trees of 50 years of age, grown in succession upon the same spot of ground, in the period of 200 years, we will calculate to what the three first trees would amount, if their value was placed out at compound interest, for the respective terms of 150, of 100, and of 50 years.

£.	*s.*	*d.*		£.	*s.*	*d.*
2	3	6	Accumulating during 150 years, at 5 per cent. per annum compound interest, will amount to -	3,280	0	0
2	3	6	Accumulating as above for 100 years, would amount to -	286	0	0

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* See Table 12 of a bole of 24 feet, in the 26th volume of the Society's Transactions, page 68.

† See my first Table in ditto, page 49.

Accu-

£.	*s.*	*d.*		*£.*	*s.*	*d.*
2	3	6	Accumulating as above for 50 years, will amount to -	24	0	0
			Add the value of the tree to be cut down at the end of 200 years	2	3	6
			Total amount in 200 years —	3,592	3	6
			And carrying forward this calculation, the total amount of the produce in 300 years would amount to —	*£*.472,408	0	0

In former times, when the value of oak-woods was estimated by the number of hogs their acorns would fatten, the great age of trees would be of small consideration; but in the present times, I am persuaded, that if gentlemen who have many trees standing of the age of 150 years and upwards, would give this subject its due consideration, they will be aware of the immense loss to which they are voluntarily subjecting themselves.—And this great loss is much to be regretted, in a political point of view, especially as the produce of this island is insufficient for its necessary consumption.

My motives for troubling you with this hasty production are, to promote the good of the public, by endeavouring to persuade gentlemen to bring forward well ascertained facts, respecting the most profitable management of growing timber trees, and to induce them to investigate, with accuracy, this very curious and important subject. If you think it is likely to have those effects, I shall thank you to lay this paper before the Society of Arts, &c. for their consideration.

I am, sir, your obedient servant,
CHARLES WAISTELL.

No. 99, High Holborn,
Oct. 1809.

LXIV. *A List of about Five Hundred Collieries in and near to Derbyshire. By Mr.* JOHN FAREY, *Mineralogical Surveyor.*

To Mr. Tilloch.

SIR, I INCLOSE a list, of such collieries as I have either visited or obtained information concerning, in the course of my recent examination of the county of Derby, and the borders of the seven adjacent counties. Should you deem the same worthy of a place in your Philosophical and Geological Magazine, it may be proper to observe, that the names of the places in the first column, are not always those of the parish in which the coal-works are situate, but of the nearest or most convenient place, shown in general maps, by which to refer to the precise site of the collieries. Also, that many of these works are now discontinued: yet as in almost every instance, more of this valuable mineral remains still ungotten, in or near to the same spot, the recording of all places where coals have at any time been worked, seems an object of some importance. In the report on the county of Derby, which I am at present employed in [illegible] eparing, for the Board of Agriculture, I intend to giv [illegible] one alphabetical list, the bearings and distances of e [illegible] f these collieries, from the towns mentioned in the fi [illegible] umn, with the particular place in the series of stra [illegible] hich each colliery is to be referred, as far as I am a [illegible] the present instance: the collating of the great mass [illegible] nformation which I have been favoured with, from [illegible] several coal-masters, and their agents and workmen [illegible]; yet unfinished, as well as the reexamination of so [illegible] s of the interesting coal-fields, within the limits o [illegible] intended map, in districts where the alluvial coverings [illegible] faults and stupendous dislocations of the strata, have p [illegible] ented great, but as I trust, not insuperable difficulties [illegible] e complete elucidation of these highly important st [illegible] In the report I shall distinguish such of the above [illegible] ries as now are, or recently have been, in work. Where no county is mentioned to the places in the first column, Derbyshire is to be understood; in which county I find the coal strata or measures, distributed over not less than 190,000 acres of its surface! I shall esteem it a great favour, if proprietors of estates, or others, who happen to be possessed of correct accounts of the sinkings or measures at any of the collieries below, in documents to which I may not have had access, or respecting any ancient collieries within these limits, which may have eluded my inquiries, that they will communicate copies of such particulars (addressed as below) as soon as convenient; carefully distingishing the bearing and distance from one at least of the above places; the time of ceasing to work, old collieries, if known, &c. The borings or sinkings, where trials to any depth have been made, whether successful or not, would be alike acceptable, such being the materials from which, principally, a correct account

count of the subterranean geography of the district, or the knowledge of its strata, is to be drawn.

I am, sir,
Your obedient servant,
JOHN FAREY,

12, Upper Crown Street, Westminster, June 5, 1810.

Places' Names.	**Names of Collieries.**
Alfreton	Alfreton, Four-lane-ends, Greenhill-lane, High-field-lane, Nether-Birchwood, Oakerthorpe, Riddins, Swanwick-green, Swanwick-delves, Somercotes, Somercotes-furnace,
Ashly-de-la-Zouch, Leicestershire ...	Alton-grange, Southwood.
Ashover	Alton, Birkin-lane.
Ashton-under-line, Lancashire	Car-lane, Crickety, Fairbottom, Hays, Hurst-brook, Knoll.
Aston, Yorkshire ..	Bigging, Conduit-moor, Fox-Ears, Lawn (or Casteven or Kesteven), Pidgeon-bridge, Aston-common, Swallow-nest.
Attercliff, Yorks...	Attercliff-common, Bright-side, Darnall, High-hazels, Tinsley-park, Washford.
Awsworth, Notts.	Awsworth, Newthorpe-common.
Barlborough	Barlborough-common, Beighton-field, Hazlewell, Westfield, Knitaker, Pebley-lane, Spinkhill-common.
Barlow	Barlow-common, Cutthorpe, Far-lane, High-ash, Sudbrook, Wilders-green.
Barnsley, Yorks...	Gober-hall.
Baslow	Baslow, Chatsworth old Park,
Beaudesert-Park, Staffordshire ...	Egerton-wood.
Beeley	Beeley-moor, Harwood-Grange,
Beighton	Berley-moor, Nether-field.
Belper	Belper-gutter, Belper-lane-end, Belper Town, Bent, Chevin-side, Dally-gutter, Hopping-hill, Openwood-gate, Swinney.
Biddulph, Staff...	Biddulph-hall, Crabtree, Childerplay, Falls, New-Pool.

Places' Names.	Names of Collieries.
Bilborough, Notts.	Bilborough, Holly-wood, Nuthall.
Blackfordby, Leices.	Blackfordby, Norris-hill.
Blackwell	Blackwell, Dimmings-dale, Newton.
Bolderstone-chapel, Yorkshire	Stock-bridge.
Bolsover	Palterton, Shuttlewood-common, Stanfrey.
Boothorpe, Leicestershire	Little-worth, Milk-hill, Sweet-hill-oak.
Bradfield-Chapel, Yorkshire	Hagen-field, Holes.
Brimington	Brimington, Brimington-moor, Hollingwood-common, Wildens-mill.
Buxton	Goyte-[illegible]ss, Thatch-marsh.
Calke	Brians-[illegible]ppy.
Chapel-en-le-Frith	B[illegible], Cowpasture, Fernylee, [illegible]ss.
Chapel-Town, Yorkshire	Chapel-town, Heasley-Park, Parkins-wood, Smithy-wood-engine, Thorn[illegible]iff.
Cheadale, Staff. ...	Delph-house, East-wall, Eaes, Moverley, [illegible]haw (or Sham), Shaw, Woodhead.
Cheddleton, Staff. ..	Crown-point, Cunsal-wood, Newstead, Shafferlong, Wetley-moor.
Chesterfield	Ash-gate, Boythorpe, Chesterfield-town's-end, Chesterfield-furnace, Calow, Dunston, Grass-hill, Hady, Hasland, Little-common, Lounsley-green, Moor-top, New-Brampton, Newbold-common, Newbold-field, Stone-gravel, Tapton, Walton, Wingerworth-park, Wingerworth-furnace.
Church-Gresley ...	Gresley, Gresley-hall, Round-hole, Woodwards.
Clown	Clown.
Codnor	Benty-field, Codnor nether-park, Codnor upper-park.
Cole-Orton, Leicest.	Cole-Orton, Lount new, Lount old.
Conisborough, Yorks.	Conisborough, Dennaby.
Cossall, Notts.	Cossall, Robinets.
Crich	Plaistow-Green.
Dale-Abbey	Dale-Abbey, Hag, Lower-Hag, Pingle.

Places' Names.	Names of Collieries.
Denby	Denby, Denby-hall, Roby east-field, Roby west-field, Smithy-houses.
Dilhorne, Staff.	Dilhorne, Parsons-field, Swetley (Lowes').
Disley, Cheshire	Allington, Bank-end, Diglee, Furnace-clough, Gee, Hag-bank, Hoo-lane, Lyme, Lyme-park, Norbury, Poynton, Red-acre, Worth.
Dore	Dore, Ringing-low-bar.
Dronfield	Apperknowl-common, Cole-Aston, Dronfield, Hill-top, Ounston, Stubley, Woodhouse.
Duckingfield, Ches.	Duckingfield, Dunkirk, Flowery-field, Hough-hill, Newton-moor, Rabbit-hole, Score-wood.
Duckmanton (long)	Adelphi-furnace, Duckmanton-common, Middle-Duckmanton.
Eastwood, Notts.	Beggerlee, Brinsley new, Brinsley old, Eastwood.
Eccleshall Barlow, (in Sheffield,) Yorkshire	Grey-stones, Mill-house, Moss, Smelting, Trap-lane, Whitley-wood.
Eckington	Arbor-lands, Bole-hill, Bramley-moor, Coldwell, Eckington, High-lane, Mossborough-moor, Troway.
Flash (in Alstonfield), Staff.	Birchen-booth, Black-clough (or Beat), Blue-hills, Chest, Dane-head, Dane-thurn, Diamond-hill, Goldsitch, Hazle-barrow, Notbury, Penny-hole, Whiteshaw.
Foxton, Staffordsh.	Clough-head, Foxton-wood, Stile-shutt.
Fullwood-Chapel, (in Sheffield) Yorks.	Stanage.
Glossop	Combs, Simondley.
Greasborough, Yorks.	Cinder-hill (or Middle-field), Haw-wood.
Greasley, Notts.	Beauvale-abby, Greasley, Limes.
Hansworth, Yorks.	Ballyfield, Gleedless-common, Hansworth, Hansworth-woodhouse, Intake, Orgrave, Woodthorpe.
Harthill, Yorks.	Woodhall-moor.
Hartshorn	Gosley-waste, Hartshorn.
Hathersage	Stanage-pole.

Places' Names.	Names of Collieries.
Hayfield (*in Glossop*)	Aspinshaw, Burn'd-edge, Moor-top.
Heage	Heage, Heage-bent, Morley-park, Town-field.
Heanor	Aldercar, Heanor, Langley, Milnhay, Shipley.
Heath	Heath, High-house.
Heather, Leicester.	Heather.
Higham	Higham.
Holbrook	Holbrook.
Holmesfield	Bank, Salters-sitch, Thickwood.
Horsley	Hor [illegible] Horsley-woodhouse, Slack-fi [illegible]
Hyde-Chapel, Cheshire	Bro [illegible] r, Denton, Hyde-lane, [illegible] -low, Woodley.
Ilkeston	Co [illegible] y-wood, Ilkeston, Ilkeston-c [illegible], Little-Hallam.
Ipstone, Staff.	Cro [illegible] r, Hay-house, Ipstone, l [illegible] Nether-field.
Killamarsh	Ga [illegible] e, Killamarsh, Nether- [illegible] illamarsh old-delph, Over- [illegible]
Kimberworth, Yorks.	Bla [illegible] bank, Bradgate, Kimberw [illegible] Meadow-hall.
Kingsley, Staff. ..	F [illegible] Garstone, Hazles-cross, [illegible] ay, Jack-elm, Kingsley-u [illegible] es, Rake-edge, Ross-bank.
Macclesfield, Ches.	Blake-low, Bollington, Cliff-bank, Eastborough-lane, Hurdsfield, Macclesfield-common, Riley-clough, Shrigley-fold, Swanco, Throtles-nest.
Marple-Chapel, Ches.	Brabins, Chapel-house.
Matlock	Lea, Lumsdale, Tansley-green.
Measham	Donisthorpe, Measham, Measham-fields, Oakthorpe.
Mellor (*in Glossop*)	Bore-lane, Broadhurst-edge, Compstal-bridge, Ludworth, Old-hall-wood, Shaw-hay.
Mexborough, Yorks.	Mexborough.
Morley	Morley.
Mossley, Lancash.	King-bank, Park, Rotches, Scout-mill, Windy-bank.
Mottram, Cheshire.	Hague-bank, Hill-end, Hodge-hall.

Newall

Places' Names.	Names of Collieries.
Newall (*in Stapenhill*)	Bretby, Hall-fields, Newall, Newall-park, Perkins, Stanton, Swadlincote, Water-field, Wooden-box, Wood-field.
New-Mills (*in Glossop*)	Eaves-knowl, Lower-house, Tormine, Warps-moor.
Norton, Staff.	Bow-green, Whitfield.
Norton	Lees-hall.
North-Winfield	Ankerbold, Berrisford-moor, Claycross, Henmore, Loco-lane, Pilsley-lane, Tupton-green, Woodthorpe.
Oakmoor-Mills (*near Alveton*) *Staff.*	Beelow, Car-wood.
Over-Seal, Leicest.	Warren-hill-furnace.
Packington, Leicest.	Packington.
Pelsall (*near Bloxwick*) *Staff.*	Brown-hills, Essington new Colliery, Essington-wood, Goscot, Lords-hay, Pelsall, Wyrley-bank.
Penistone, Yorks.	Bull-house, Flash-house, Fullshaw, Law, Midhope-stones, Paw-hill.
Pentrich	Castle-hill, Harts-hay, Pentrich.
Pinxton	Carter-lane, Pinxton.
Pott-Shrigley, Chesh.	Bakestone-dale, Berristow, Harrop, Pott-hall, Spons-moor, Spons, Styperson.
Rainow-Chapel, Chesh.	Kerridge-east-side, Kerridge-north-end, New-post, Rainow-low.
Rawmarsh, Yorks.	Nether-Hough, Over-Hough, Rawmarsh, Stubbing-lane.
Ripley (*in Pentrich*)	Butterly-park, Butterly-car, Greenwich, Ripley.
Rotherham, Yorks.	Clough, Herringthorpe, Hill-top, Kimberworth-park, Mossborough-common.
Rudgley, Staff.	Bruerton.
Sheffield, Yorks.	Crooks-moor, Deep-pits, Harbourthorn, Manour, Park-furnace, Ponds, Sandy-gate.
Shirland	Shirland, Smithy-moor, Stretton.
Silkstone, Yorks.	Silkstone.
Skegby, Notts.	Shilo, Dirty-Hucknale, Skegby.
Smalley	Smalley, Smalley-common, Simonfield, Woodhouse-lane.

Places' Names.	Names of Collieries.
Smithsby	Pistern.
South-Normanton .	Berristow, South-Normanton.
Stannington, Yorks.	Armitage, Dungworth, Low-ash, Storrs, Wadsley.
Stanton (*by Dale*) .	Hallam-bridge (or Nutbrook).
Stanton-Harold, Leicester.......	Heath-End, Stanton-Harold.
Stapleford, Notts. .	Bramcote.
Staveley	Norbrigs, Staveley, Woodthorpe.
Sutton (*in Scarsdale*)	Sutton, Sutton-common, Wood-nook.
Swepston, Leicester.	Swepsto
Tamworth, Warwickshire	Polesw Waverton, Wilnecote.
Tankersley, Yorks.	Tankersl park.
Taxhall, Cheshire .	Caste ap-sitch.
Temple-Normanton	Grass , Grass-moor (Platt's), Li
Teversall, Notts...	Dun
Thursfield, Staff. .	Bernersle green.
Ticknall	Ticknall. Vhite-holly-coppy.
Todwick, Yorks. ..	Todwic oor.
Tibshelf.........	Biggin, rstoft, Tibshelf.
Treton, Yorks.....	Cat-cliff.
Wales, Yorks.	Wales.
Wath, Yorks......	Abdy, V th-wood.
Wentworth-Chapel, Yorks.	Cortworth, Elsicar, Hoober, Low-wood, Park-gate, Swallow-wood-nook, Wentworth-park.
West-Hallam.....	Stanley-common, West-Hallam, West-Hallam windmill-hill.
Whiston, Yorks. ..	Royds-moor.
Whittington	Glass-house-common, Whittington-moor.
Wickersley, Yorks..	Brecks, Hollings-moor.
Wincle-Chapel, Cheshire	Green-hill, Hay, Latche, Mouse-trap, Robins-clough, Quarnford.
Wirksworth.......	Alderwasley, Wigwell.
Wollaton, Notts. ..	Aspley, Trowel-moor, Wollaton.
Woodhead, Cheshire	Crowden-clough.
Worsborough, Yorks.	Stainborough-park, Worsborough.
Wortley-Chapel, Yorks..........	Deep-car, Hunshelf, Westwood.

LXVI. *The*

LXVI. *The Croonian Lecture.* *By* William Hyde Wollaston, *M.D. Sec. R.S.*[*]

I am aware that the remarks, which I have to offer on the present occasion, may be thought to bear too little direct relation to each other for insertion in the same lecture; yet any observation respecting the mode of action of voluntary muscles, and every inquiry into the causes which derange, and into the means of assisting the action of the heart and blood-vessels, must be allowed to promote the design of Dr. Croone, who instituted these annual disquisitions. And it has always appeared to be one great advantage attending the labours of this society, that it favours the production of any original knowledge, however small, in a detached form; and enables a writer to say all that he knows upon a particular subject, without inducing him to aim at the importance of a long dissertation.

I shall therefore make no apology for dividing the following lecture into three distinct parts.

In the first of which I shall treat of the duration of voluntary action.

In the second, I shall attempt to investigate the origin of sea-sickness, as arising from a simple mechanical cause deranging the circulation of the blood.

In the third, I shall endeavour to explain the advantage derived from riding, and other modes of gestation, in assisting the health under various circumstances, in preference to every species of actual exertion.

Part I. *On the Duration of Muscular Action.*

The necessity of occasional intermissions from a series of laborious exertions, is within the experience of every one; the fatigue of continuing the effort of any one voluntary muscle without intermission even for a few minutes is also sufficiently known; but there is a third view of the duration of muscular action which appears to have escaped the notice of physiologists; for I believe it has not hitherto been observed that each effort, apparently single, consists in reality of a great number of contractions repeated at extremely short intervals: so short indeed that the intermediate relaxation cannot be visible, unless prolonged beyond the usual limits by a state of partial or general debility.

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[*] From Philosophical Transactions for 1810, Part I.

I have been led to infer the existence of these alternate motions from a sensation perceptible upon inserting the extremity of the finger into the ear. A sound is then perceived which resembles most nearly that of carriages at a great distance passing rapidly over a pavement.

The rapidity of the motion varies according to the degree of force with which the finger is retained in its place. The sound thus perceived is not at all dependent on the degree of pressure upon the tympanum; for, on the contrary, the vibratory sound is most distinct when that pressure is slight, if the finger be at the same time rendered rigid by the forcible action of antagonist muscles; and when the ear is stopped with gre without the presence of muscular action, no such s is produced. For instance, if the head be rested up ind in such a position, as to press with its whole upon the ball of the thumb applied to the ear, no nois :eived, unless the extremity of the thumb be at the s e pressed against the head, or unless the action of s ier muscles be communicated to the ear, by any i nce in the method of conducting the experiment.

When I endeavoured to ate the frequency of these vibratory alternations, they ared to be in general between 20 and 30 in a sec but it is possible that the method I employed may ind defective, and it is to be hoped that my esti iy be corrected, by some means better adapted to rmination of intervals that cannot actually be measu

It was by imitation alone that I was enabled to judge of their frequency. For this purpose I contrived to render the vibration itself, and the imitative sound, both audible by the same ear.

While my ear rested on the ball of my thumb, my elbow was supported by a board lying horizontally, in which were cut a number of notches of equal size, and about one-eighth of an inch asunder. Then, by rubbing a pencil or other round piece of wood with a regular motion along the notches, I could imitate pretty correctly the tremor produced by the pressure of my thumb against my head; and by marks to indicate the number of notches passed over in five or ten seconds, observed by my watch, I found repeated observations agree with each other as nearly as could be expected; for I could not depend upon exerting the same degree of force in different trials.

That I might not be deceived by the resemblance of tremors, which coincided only at alternate beats, and therefore

fore might be considered as octaves in music to each other, I sometimes employed notches at greater and sometimes at less distances from each other, but the result was nevertheless the same; and in order to avoid any error that might be caused by some accidental quality of the sound arising from the length of the muscle employed, or length of the bones concerned in conveying the imitative sound to my ear, I made the following variation of the experiment. My ear was stopped by a cushion pressed upon by the end of a notched stick that rested on my foot, and thus conveyed the vibration from the muscles of my leg to the ear, along with the tremor produced by friction upon the notches; and still the results were nearly the same; varying in frequency between 20 and 30 in a second, according to the degree of force exerted in the experiment *.

As a further proof that I was not much deceived in my judgement of the frequency of these vibrations, I requested two or three of my friends to repeat the same experiment for me, and our agreement was such as to confirm me in opinion, that there could be no very considerable error in the estimate.

The greatest frequency that I think I have observed, was about 35 or 36 in a second, and the least was as low as 14 or 15; but in attempting to lessen the number of vibrations, there appears to be a degree of unsteadiness which prevents any accurate measurement of the real number.

It is very probable, that in cases of great debility the number may be even considerably less, and may be the reason of that visible unsteadiness, which is known to occur in persons enfeebled by age, or much reduced by disease.

Possibly the foregoing observation may not be altogether new to some members of this society, as it is now about 17 or 18 years since it first occurred to me, and I was then accustomed occasionally to mention it in conversation with my friends; but I am not aware that any other person has made the same remark respecting the vibratory nature of muscular action, although I find that Grimaldi had observed

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* The resemblance of the muscular vibrations to the sound of carriages at a distance, I apprehend to arise not so much from the quality of the sound, as from an agreement in frequency with an average of the tremors usually produced by the number of stones in the regular pavement of London, passed over by carriages moving quickly.

If the number of vibrations be supposed 24 in a second, and the breadth of each stone be about six inches, the rate of a carriage thus estimated would be about eight miles an hour; which agrees with the truth as nearly as the assumptions on which the estimate is founded.

served the sound that occurs upon stopping the ears, but ascribed it, according to the notions that prevailed in his time, to the hurried motions of the animal spirits*.

Part II. *On Sea-Sickness.*

The second remark which I have to offer to the society relates to sea-sickness, the cause of which has not hitherto been fully explained; and although the explanation which I am about to propose, may not appear altogether satisfactory to persons who, when at sea, are also rendered giddy by the incessant motion of the waves, and are consequently liable to consider as cause and effect phænomena which in their minds are constantly as iated; yet the observation on which it is founded ma serve to be recorded, on account of the degree of reli at may be obtained in that most distressing affection.

After I had been harass [illegible] sea-sickness during a short voyage for some days, and [illegible] in vain attempted to account for the difference betw the inexperienced passenger, and those around him m accustomed to the motion of the sea, I imperceptibly ac red some power of resisting its effects, and had the d fortune to observe a peculiarity in my mode of r tion, evidently connected with the motion of the vess ut of which, in my then enfeebled state, I was unable nvestigate either the cause or consequence. In waking m a state of very disturbed sleep, I found that my r ons were not taken with the accustomed uniformity, but were interrupted by irregular pauses, with an appearance of watching for some favourable opportunity for making the succeeding effort; and it seemed as if the act of inspiration were in some manner to be guided by the tendency of the vessel to pitch with an uneasy motion.

The mode by which I afterwards conceived that this action could primarily affect the system, was by its influence on the motion of the blood; for, at the same instant that the chest is dilated for reception of air, its vessels become also more open to the reception of the blood, so that the return of blood from the head is more free than at any other period of a complete respiration. On the contrary, by the act of expelling air from the lungs, the ingress of blood is so far obstructed, that, when the surface of the brain is exposed by the trepan, a successive turgescence and subsidence of the brain is seen, in alternate motion with the different states of the chest. It is probably from this cause that, in severe head-aches, a degree of temporary relief is obtained by occasional complete inspirations.

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* Vera itaque ratio experimenti prædicti est, quia in digito et brachio totoque corpore continuato fiunt multi motus ac tremores, ob spirituum agitationem huc illuc perpetuo accurrentium. *Grimaldi*, Physicomathesis de Lumine, p. 383.

In sea-sickness also the act of inspiration will have some tendency to relieve, if regulated so as to counteract any temporary pressure of blood upon the brain; but the cause of such pressure requires first to be investigated.

All those who have ever suffered from sea-sickness (without being giddy) will agree that the principal uneasiness is felt during the subsidence of the vessel by the sinking of the wave on which it rests. It is during this subsidence that the blood has a tendency to press with unusual force upon the brain.

If a person be supposed standing erect upon deck, it is evident that the brain, which is uppermost, then sustains no pressure from the mere weight of the blood, and that the vessels of the feet and lower parts of the body must contract, with a force sufficient to resist the pressure of a column of blood, of between five and six feet from the head downwards.

If the deck were by any means suddenly and entirely removed, the blood would be no longer supported by its veſsels; but both would fall together with the same velocity by the free action of gravity; and the same contraction of the vessels which before supported the weight of the blood would now occasion it to press upon the brain, with a force proportional to its former altitude.

In the same manner, and for the same reason, during a more gradual subsidence of the deck, and partial removal of support, there must be a partial diminution of the pressure of the blood upon its vessels, and consequently a partial reaction upon the brain, which would be directly counteracted by a full inspiration.

The consequence of external motion upon the blood will be best elucidated by what may be seen to occur in a column of mercury similarly circumstanced.

A barometer, when carried out to sea in a calm, rests at the same height at which it would stand on shore; but, when the ship falls by subsidence of the wave, the mercury is seen apparently to rise in the tube that contains it, because a portion of its gravity is then employed in occasioning its descent along with the vessel; and accordingly, if it were confined in a tube closed at bottom, it would no longer press with its whole weight upon the lower end. In the same manner, and for the same reason, the blood no longer presses downwards with its whole weight, and will be driven upwards, by the elasticity which before was merely sufficient to support it.

The sickness occasioned by swinging is evidently from the same causes as sea-sickness, and that direction of the motion which occasions the most piercing sensation of uneasiness, is conformable to the explanation above given.

It is in descending forwards that this sensation is perceived; for, then the blood has the greatest tendency to move from the feet towards the head, since the line joining them is in the direction of the motion. But when, in the descent backwards, the is transverse to the line of the body, it occasions omparative inconvenience, because the tendency to the blood towards the head is then inconsiderable.

The regularity of the in swinging, afforded me an apparently favourabl unity for trying the effect of inspiration; but alt e advantage was manifest, I must confess, it did n qual the expectations I had formed from my experie ea. It is possible that the suddenness of the desc in this case be too great to be fully counteracted means; but I am inclined to think that the cont the intestines are also affected by the same cau e blood; and if these have any direct disposition itate, this consequence will be in no degree count y the process of respiration.

A friend of mine informed me that he had endeavoured to counteract this mechanical effect upon the stomach, and had experienced immediate relief from a slight degree of sea-sickness, by lying down upon the deck with his head towards the stem of the vessel; by means of which, upon pitching, he was in the attitude of a person descending backwards in a swing.

Whether the stomach be or be not thus primarily affected, or only by sympathy with the brain, the sensation of sinking is in all cases referred directly to the stomach, which is seized with such instantaneous retching, that no person who has not been so situated can form a just conception of it[*].

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[*] There is one occasion upon which a slighter sensation of this kind is perceived, and it appears to indicate the direction of the motion from which it arises, to be downwards. "In a country subject to frequent returns of earthquakes," it is said[†] that "a few minutes before any shock came, many

[†] Phil. Trans. vol. xlii. p. 41.

In thus referring the sensations of sea-sickness in so great a degree to the agency of mere mechanical pressure, I feel confirmed by considering the consequence of an opposite motion, which, by too quickly withdrawing blood from the head, occasions a tendency to faint, or that approach to fainting, which amounts to a momentary giddiness with diminution of muscular power. At a time when I was much fatigued by exercise, I had occasion to run to some distance, and seat myself under a low wall for shelter from a very heavy shower. In rising suddenly from this position I was attacked with such a degree of giddiness, that I involuntarily dropped into my former posture, and was instantaneously relieved, by return of blood to the head, from every sensation of uneasiness.

Since that time, the same affection has frequently occurred to me in slighter degrees, and I have observed, that it has always been under similar circumstances of rising suddenly from an inclined position, after some degree of previous fatigue. Sinking down again immediately removes the giddiness; and then, by rising a second time more gradually, the same sensation is avoided.

Part III. *On the salutary Effects of Riding, and other Modes of Gestation.*

In the preceding instances of disturbing the circulation of the blood, by external motion, the effect is disagreeable, and proportionally prejudicial. There may indeed be cases of disorder, in which it will be salutary, but these are probably less frequent than is generally supposed.

In the observations which follow, general opinion will concur with me, on the benefit derived from external or passive motion, and I hope that, in ascribing its good effects to their true cause, I shall enable others to make a valuable distinction, which has not yet been preserved with due care, between one motion which is salutary, and another which is very frequently pernicious. For, although the

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many people could foretel it by an alteration in their stomachs; an effect which (it is added) always accompanies the wave-like motion of earthquakes, when it is so weak as to be uncertainly distinguishable." (Michell, Phil. Trans. vol. li. 610.)

It seems that the vapours to which these tremendous concussions are owing, immense in quantity, and of prodigious force, being for a time confined on all sides, elevate the surface of a country to a vast extent until they either find vent, or meet with some partial cause of condensation; and hence the alternate heaving and subsidence of the ground will produce much the same effects as the rising and falling of the swell at sea.

the term *gestation* is employed by medical writers, as a general term comprehending riding on horseback, or in a carriage, and although the merits of such motions, especially the former, were clearly noticed, and perhaps even over-rated, by the discernment of Sydenham, I believe that no explanation has yet been given, of the peculiar advantages of external motion, and am persuaded, that the benefits to be derived from carriage exercise are by no means in so high estimation as they ought to be.

Under the common term *exercise,* active exertion has too frequently been confounded with passive gestation, and fatiguing efforts have consequently been substituted for motions that are agreeable, even directly invigorating, when duly adapted to the s th of the invalid, and the peculiar nature of his ind ion.

The explanation which ibout to offer of the effects of external motion upon rculation of the blood, is founded upon a part of th :ture observable in the venous system, the mechani dency of which cannot be doubted. The valves w re every where dispersed through those vessels, allo passage to the blood, when propelled forward by any m 1 that assists its progress; but they oppose an immed ibstacle to such as have a contrary tendency. The c tion is consequently helped forward by every degree o e agitation. The heart is supported, in any laboric rt that may have become necessary, by some obsta ts exertions; it is assisted in the great work of resto system, which has recently struggled with some violent attack: or it is allowed, as it were, to rest from a labour, to which it is unequal, when the powers of life are nearly exhausted by any lingering disorder.

In the relief thus afforded to an organ so essential to life, all other vital functions must necessarily participate; and the various offices of secretion, and assimilation, by whatever means they are performed, will not fail to be promoted during such comparative repose from laborious exertion.

Even the powers of the mind itself, though apparently least likely to be influenced by mere mechanical means, are manifestly, and in many persons most immediately, affected by these kinds of motion.

It is not only in cases of absolute deficiency of power to carry on the customary circulation, that the beneficial effects of gestation are felt, but equally so when comparative inability arises from redundancy of matter to be propelled. When from fulness of blood the circulation is obstructed, the whole system labours under a feeling of hurry and agitation, with that sensibility to sudden impressions which is usually termed nervousness. The mind becomes incapable of any deliberate consideration, and is impressed with horrors that have no foundation but in a distempered imagination.

It is in moderate degrees of this species of affection that the advantages of carriage exercise are most sensibly felt. The composed serenity of mind that succeeds to the previous alarm, is described by some persons with a degree of satisfaction that evinces the decided influence of the remedy. With this steadier tone of mind, returns its full power of cool reflection; and if the imagination becomes more alive than usual, its activity is now employed in conceiving scenes that are amusing and agreeable.

As an instance of direct relief to a circulation labouring from mere fulness of blood, I may adduce that of a person, whose friends, as well as himself, were apprehensive, from the violent and visible throbbing of his heart, of the existence of some organic mischief, and were in some measure alarmed for the consequences.

He was persuaded, and not reluctantly, to go without delay for medical advice, and was accordingly conveyed in a carriage to the house of some physician of eminence, but did not succeed in finding him at home. As the symptoms did not appear to admit of delay, and were at least not aggravated by the motion, it was hoped that the wished-for advice might be obtained at a part of the town which happened to be at some distance. But the second attempt proved as fruitless as the former, and a third was made with the same event. Since the throbbing had by that time considerably abated, he was contented to postpone any further efforts to the following day, and directed the carriage homewards. By the time that he returned to his friends, he found that the motion of travelling over several miles of pavement had apparently removed the complaint. The pulsation of the heart and arteries had subsided to their natural standard, and he congratulated himself, that his search of a remedy had not been ineffectual, although he had been disappointed as to the source from which he thought he had most reason to expect relief.

If vigour can in any instance be directly given, a man may certainly be said to receive it in the most direct mode, when the important service of impelling forward the circulation of his blood is performed for him by external means. The main spring, or first mover of the system, is thereby, as it were, wound up; and although the several subordinate operations of so complicated a machine cannot be regulated in detail by mere external agency, they must each be performed with greater freedom, in consequence of this general supply of power.

In almost every treatise on the subject of chronical diseases, are to be found numerous instances of the benefit produced by the several modes of gestation which have been most generally adopted; as riding on horseback, in carriages, sea-voyages, and swinging. And in many cases which might be adduced, it has appeared too clear to
admit of a doubt, that t re of the patient has been
owing *solely* to the exte itation of his body, which
must be allowed, at least ve had the effect above ex-
plained; that of relievi heart and arteries from a
great part of their exe propelling the blood, and
may therefore have contr to the cure, by that means
only.

The different modes ab entioned are adapted from
their nature to different d of bodily strength; and if
there are cases in which hich appears most eligible
may not suit the situatio cumstances of the patient,
it cannot be difficult to ve other means of giving
motion, so as least to iode, and yet to give the
greatest relief. A very id long continued, or even
incessant motion, may me cases better than any
more violent and occas itation; and in this way,
probably, it is, that sea-voyag have sometimes been at-
tended with remarkable advantage.

LXVII. *Comparative Tables of the Beats of the Tempered Consonances in M. Kirnberger's and the Isotonic or Equal Temperament Systems of Tuning; with Remarks on the common System used by Organ Tuners, compared with that of M. Kirnberger. By the Rev.* C. J. Smyth, *Minor Canon of the Cathedral, Norwich.*

The opinions of profound theorists are ever entitled to attention; but should not be received with implicit faith. If such a man as Sir Isaac Newton was capable of a mistake, so is an Emanuel Bach, or a Kollmann. To the latter gentleman the musical world is under the greatest obligations for reducing the theory of Composition to a degree of simplicity, before his writings unknown. But the opinions he has advanced, with respect to the temperament of the musical scale, require that minute investigation and submission to calculation, which, if they had been advanced by a person of less celebrity, might have been passed over in silence.

One of the grand objects at which he appears to aim, is to establish an *equal temperament* on the piano-forte (that is, that all chords of the same kind shall be alike, as to their degree of imperfection): to this I raise no objection; the rage for modulation at present exerting its energies, to their utmost possible extent; a rage very favourable to the talents of those, who have not invention sufficient to produce novel and beautiful melodies, and yet aspire to the character of interesting composers.

I shall first presume to offer a few observations on the *unequal temperament* of Kirnberger, which Mr. Kollmann *supposes* "one of the best hitherto known." Not having the happiness to be able to read German, I know not what M. Kirnberger has offered in its behalf; or those important observations which major Templehoff (in an Essay in that language published in 1775, Berlin) is said by Dr. Robison in the Encyc. Brit., art. *Temperament*, to have made, on Kirnberger's system.

Preferring at all times experiment to theory, I tuned my piano-forte according to Mr. Kollmann's printed directions, with a view to hear the effect of one of "the best unequal temperaments." I cannot speak favourably of the result. I will below subjoin a table of *the beatings* of the tempered consonances, in order to give those professors who may feel no inclination to submit to the drudgery of calculation, some idea of what the effect of this temperament would be on an organ, where those *beatings* are most distinctly heard: a formidable host of foes, inimical to correct, and even tolerable tune. It appears to me, in the outset, doubtful whether a tempered system should have any *perfect* chords (but the octaves); as those chords, whenever they are heard, will render the ear less disposed to be pleased with the imperfect harmony which follows.

We will suppose the performer on the organ, to begin with Kirnberger's system in the key of C, answering to the tenor clif; here is a chord absolutely *perfect;* so also is the chord of G, the fifth of the key; but the chord of the fourth of the key ranks, in point of importance, next to the key-note and its fifth; and here unfortunately is a chord of which A, the major third to F, beats 149 times in 15″. From the key of C a modulation will naturally be expected into the key of G; which modulation will require the dominant of G, viz. the chord of D with a major third, fifth, and seventh. Now the A is half a comma too flat, as a fifth above D, and will beat 75 times in 15″. And this we may take, as the first instance of "a very fine variety of perfection." Surely Mr. Kollmann would have been more consistent in the use of terms, if he had said "varieties of imperfection." The next chord which occurs is that of E, the fifth of which is perfect, but the major third beats 255 in 15″. The chord of F has already been spoken of; proceed we then to examine the chord of Bb; the fifth is perfect, the major third beats 399. This chord on Eb beats 266.

Now let us compare this temperament with that upon organs tuned in the usual manner. I do not give the beats in extreme keys, because they are too rapid to be heard as *beatings*, except in the lowest part of the scale, and have a *rough* effect, which is more tolerable than beatings not too rapid to be perceived as beatings. One chord, viz. that of Ab or G* has a peculiar character, (it is called the *wolf*) the fifth being almost the fifth part of a minor tone *too sharp*, and the beatings are distinctly heard in the middle and lower part of the scale.

Chords on the Organ, as tuned in the usual Manner.

C, G, D, A, and E, good.
B, F*, C* and G*. The major thirds almost one fourth of a minor tone too sharp, and Ab (or G*) has also a fifth almost one-fifth of a minor tone too sharp, as before observed.
F, Bb and Eb, good.

When we hear an organ tuned in this manner, we may consider ourselves at a feast, in which there are dishes of various qualities; while in M. Kirnberger's feast of *exquisite viands*, but *eight dishes* are very palatable, and those who are fond of sour crout and olives, and, like many of our best composers, have no objection to a slice of wolf, though they would not choose to dine entirely upon that outlandish animal, have an opportunity of gratifying their peculiar palates. Until, therefore, some irrefragable arguments are produced, to prove the superiority of M. Kirnberger's temperament to that in common use, I presume our organ-builders and organ-tuners will, in spite of any charges of obstinacy, ignorance, or policity, continue to tune as their ancestors did before them: as I cannot flatter myself the public will ever go to an enormous expense, for many additional pipes, in order that our old and young organists may perform their wonderful feats of modulation (which require as little genius as application), without torturing those who prefer tolerable tune, to the parade of science. Further observations, on mean-tone temperaments, may be offered hereafter.

C. J. SMYTH.

M. KIRNBERGER'S TEMPERAMENT, Beats in 15″.

(The Vibrations communicated by Mr. FAREY.)

Keys.	Vibrations in 1″.	3ds.	IIIds.	4ths.	Vths.	6ths.	VIths.
c	480						
B	450	0	383	0	0	0	383
Bb	426·6667	474	399	0	0	574	399
A	402·4922	224	153	150	112	299	188
Ab	379·2593	383	355	0	0	510	355
G	360	399	0	0	0	533	0
Gb	337·5	188	287	0	17	0	287
F	320	355	149	0	0	474	300
E	300	0	255	112	0	0	255
Eb	284·4444	287	266	0	0	383	266
D	270	300	0	0	75	399	0
Db	252·8395	255	237	17	0	153	237
C	240	266	0	0	0	355	112
		3131	2484	279	204	3680	2782
		2484 279 204 3680 2782					
		12560	Sum total.				

THE

THE ISOTONIC SCALE*, Beats in 15″.

(The Vibrations communicated by Mr. FAREY.)

Keys.	Vibrations in 1″.	3ds.	IIIds.	4ths.	Vths.	6ths.	VIths.
c	480						
B	453·0613	366	269	30	22	428	308
*	427·6307	345	254	28	21	403	291
A	403·6312	326	240	27	20	381	274
*	380·9784	308	226	25	19	360	259
G	359·5939	291	14	24	18	339	244
*	339·4127	274	01	22	17	320	230
F	320·3612	25	90	21	16	302	218
E	302·3819	2	80	20	15	285	205
*	285·4090	2	69	19	14	269	194
D	269·3913	2	60	18	13	254	133
*	254·2725	2	51	17	12	240	173
C	240	1	42	16	12	226	163
		326	96	267	199	3807	2692
		239					
		26					
		1					
		38					
		26					
		1262	sum total				

LXVIII. *On Crystallography. By M.* HAUY. *Translated from the last Paris Edition of his* Traité de Minéralogie.

[Continued from p. 363.]

GEOMETRICAL CHARACTERS OF CRYSTALS.

16. *Forms. Nucleus or primitive form.*

IT is very rare to find a mineral under its primitive form given immediately by nature, and there is a certain number of species in which this form is known only from the results of mechanical division and by theory. The just measurement of actions susceptible of producing it is only as it were a point which frequently escapes in the process of crystallization, amid that multitude of circumstances which influence in so many ways the progress of this operation.

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* See our xxviiith volume, p. 65, and our xxixth volume, p. 347.—EDIT.

The diversity of the primitive forms ought to be regarded as a certain indication of a difference in nature between two substances, and the identity of primitive form indicates that of nature, at all times when this form is not one of those which have a marked character of regularity, such as the cube, the regular octahedron, &c.

Secondary forms. In order to describe more easily the secondary forms, we shall suppose them always situated in such a manner that the line which may be considered as their axis has a vertical position, and then the faces parallel to this axis will themselves bear the name of *vertical faces*; we shall call *horizontal faces* those which will be perpendicular, and *oblique faces* those which will be inclined towards it.

We are sometimes in the situation of indicating the incidence of a face which is presented in front in the projection of a crystal, on that which is adjacent to it behind the same crystal. We shall then give to the latter the name of *returned facet*. Suppose, for example, that in the distich topaz represented (fig. 61, Pl. VII) it is requisite to indicate the angle formed by one or other of the panes *o*, *o*, with that which is contiguous to it in the posterior part, we shall say that the incidence of *o* on the returned pane is 93° 6′.

The forms of crystals are subject to various kinds of alterations purely accidental. One consists in certain faces being nearer to, or more distant from, the centre in one crystal than in another which belongs to the same variety, in such a way, however, as constantly to preserve a certain character of symmetry. In several cases these variations only fall on the dimensions of the faces, and not on the number of their sides. This happens with certain dodecahedral garnets, which in the case of perfect symmetry would have their surface composed of twelve equal and similar rhombs, and which are lengthened in the direction of an axis which would pass by two of their opposite solid angles taken among those which are formed of three plane angles. The dodecahedron is then presented under the appearance of a solid with six panes which are elongated rhombs, with summits of three faces each which are true rhombs. In other cases, the faces themselves, or some of them at least, change their figure, by the increase or diminution of the number of their sides. Thus, upon the hypothesis that the cube performing the function of the primitive form undergoes a decrement by a simple range around its eight solid angles, it may happen that the effect of the decrement remains interrupted, at the term at which all the faces which it produces are equilateral triangles far enough removed from the centre to avoid meeting, and then the faces parallel to those of the primitive cube will be octagons. If, on the contrary, the same faces come in contact, the primitive planes will preserve the form of the square: finally, if they intersect each other, they will be changed into hexagons, without the primitive faces ceasing to be squares, and these variations might pass throug n infinity of degrees which will be as many approximati s, with respect to the form of the complete octahedron, ich is the object towards which the law of decrement ds.

But amid all these diversit of positions, the mutual incidences of the faces of th rystal are constant. This truth, which has been plac eyond all question by the numerous observations of R é de l'Isle, is a necessary consequence of the integrant olecule being itself invariable in its form, and also fr the law of decrement in its turn having a constant p ss, which is only arrested more or less far from its lim the different crystals relative to one and the same va ty.

A second cause of varia is that which disturbs the symmetry and regularity of crystalline form, and the effect of which is to destro e equality of the analogous faces, in such a way that some take a very visible extension, while others almost entirely escape the eye. The theory ought to make an abstraction of these variations, and regard them as null: but they are visible enough to confuse the mind of a person not much habituated to these exercises, and who cannot easily distinguish the type from the true form through the traits which disfigure it, and this is the source of the greatest difficulties which the study of crystallography presents. The projections traced from regular crystals, and the copies in *relievo* of these bodies, may be of great use to the naturalist, in order to bring back the rest, by an exercise of imagination, to the symmetry from which they are separated.

These imitations of the work of nature will serve to obviate a difficulty of another kind, namely, that which arises from the grouping of crystals partly concealed by each other, or from their slight projection above the matrix,

trix, in which they seem to be more or less fastened, so that it behoves an observer to complete, in his imagination, each of these partial forms.

In short, I have been more than once surprised to see with what facility young mineralogists, who have joined to a taste for the science an aptitude for geometrical conceptions, have referred every thing to its right place in crystals the faces of which were the most deranged, or have profited from the trifling part of a crystal sunk in its matrix, in order to guess at the rest. It would even seem that there is a peculiar satisfaction attached to the solution of these small problems: every person is pleased with giving proofs of sagacity, and with understanding nature as if by half a word.

In order to determine the mutual incidences of the faces of a crystal, or of its salient angles, an instrument is used which was invented by M. Carangeau. This instrument, which strongly resembles the graphometer, is composed of a semicircle M T N (fig. 77), of brass or silver, divided into degrees, and which has two arms A B, F G, one of which F G is slit from *u* to R, excepting at K, where a small piece is left unslit in order to give more solidity to the instrument. This arm is attached at R and at *c* to a brass rule situated behind, and which is of a piece with the semicircle. The junction of the arm with this rule is produced by means of two screws which are inserted into the slit. The other arm A B is slit in the same manner from *x* to *c*, where it is attached above the former by means of the screw at this place, and which traverses the two slits. On loosening the screws, we can shorten at pleasure the parts *c* G, *c* B, of the two arms, as circumstances require.

The arm A B having only a single point of attaching at *c*, where the centre of the circle is, has a movement around this centre, while the arm G F remains constantly in the direction of the diameter which passes by the points zero and 180°.

It may be useful to remark, that the upper part of the arm A B ought to be bevilled off towards its edge *sz*, the direction of which being prolonged below, passes by the centre *c* of the instrument. The reason of this is, that this edge is what is called *the index line*, i. e. that which indicates on the graduated circumference the measurement of the angle wanted.

Let us now suppose that we wish to measure on a crystal the angle formed by two adjoining planes. We know that this angle is equal to that of two lines drawn from one and the same point of the edge which joins these planes, with the condition that they are perpendicular to this ridge and laid down on the same planes. In order to have this angle, we shall arrange the instrument so that the portions *c* G, *c* B of the two arms may leave no light between them and the planes in question, and at the same time their edges may be perpendicular to the edge of junction. In this case, the faces which embrace the crystal are tangents to the two planes whose incidence we seek for. This being done, we shall seek on the circumference of the instrument, the degree which the index line *sz* marks, or the angle which this line forms with th [illegible] which passes by the centre *c* and by the zero point, [illegible] h angle is equal to that formed by the two portion [illegible], *c* B of the arms, since it is opposite to it at the summ [illegible]

It is an advantage to be [illegible] to shorten these parts at pleasure, to avoid the obs [illegible] s which would render the operation impracticable, an [illegible] hich might be occasioned either by the matrix to whic [illegible] e crystal adheres, or from the adjoining crystals in w [illegible] t is partly fastened.

But there are cases in wh [illegible] this precaution is not sufficient, and in which we s [illegible] l find ourselves constrained by the part of the semic [illegible] situated towards M, if its position was invariable. [illegible] ngenious inventor of the instrument has guarded [illegible] this inconvenience by the following contrivance.

The stalk at *c* has, besid [illegible] e two arms, a stay or rod of steel placed below the copper rule on which the arm G F is immediately applied. The upper extremity of this rod, or that which is situated towards O, has a hole into which a steel peg also enters, furnished with a screw in a similar manner. In addition to this the semicircle is divided at 90°, so that, by means of a hinge with which it is provided at the same place, the quarter of the circle T M is folded below the quarter of the circle T N, and is as it were suppressed. When we wish to execute this movement, we must loosen the screw which fastened the upper part of the rod *c* O, we must disengage the hole at the end of this rod from the screw which is inserted into it, and we must pull down the rod until it is beneath the copper rule which has the arm G F. When the angle measured exceeds 90°, we must return the quarter circle T M to its place, in order to ascertain its value.

It will be easy to appreciate the utility of the goniometer, if we reflect how interesting it is that descriptions of crystals

stals should indicate the angles which their faces make with each other. Such are the indications which make the description start up, as it were, by palpable and truly characteristic traces. Without these requisites, a description would be a rude and imperfect sketch, which might be referred to many different objects.

Thus we do not describe dodecahedral zircon when we merely say that it is a prism with four panes terminated by summits with four rhombs which arise on the longitudinal ridges. This character would also suit the harmotome (the cruciform hyacinth), the stilbite, oxidized tin, &c.: but if you add that the panes form right angles with each other, and the faces of the summit are inclined to each other by 124° 12′, the description will be restrained to zircon. If you say that the inclination is 121° 57′, it will be the harmotome; or, if you say that there are two different inclinations, the one 123° 32′, and the other 112° 14′, it will be the stilbite.

There are several varieties of one and the same substance which may present forms of the same kind, and which will only be distinguished by the measurements of their angles. Of this description are the six rhomboids on one hand, and on the other the two dodecahedrons with rhombic faces which are found in carbonated lime. How can we exactly describe all the varieties which differ from each other more or less, if we do not precisely mark the differences? And there are even cases in which the use of the goniometer is the only way to avoid an error which would not fail to slide into the description. Thus the calcareous rhomboid, the angles of which only differ in about 2° 18′ from the right angle, was at first taken for a cube, and would have continued to be called *cubical calcareous spar*, if geometrical measurements had not rectified this denomination, doubly defective, either in itself, or with reference to the theory which demonstrates that the existence of the cube does not agree here with that of any symmetrical laws of decrement.

One of the principal causes of this neglect of goniometry arises from the kind of rule to which some mineralogists are restricted, of confining themselves to characters susceptible of being determined solely by a reference to the senses: and on this account we are deprived of the resources presented by the instruments which give to our organs a new degree of delicacy, and render them capable of attaining, in the determination of the distinguishing characters of minerals, that precision which is in its turn the principal character of the sciences. I have known some admirers of simple and unaided ocular demonstration, nevertheless, approve of using an eye-glass. Now what is a goniometer but a kind of geometrical eye-glass, which enables us to perceive those minute differences, and imperceptible gradations, which escape the eye?

With respect to plane angles, we have sometimes indicated them also*, particularly those of primitive forms, and those which imprint on secondary forms a character of simplicity and regularity, such as the angles of 90°, 60°, &c.

We shall conclude from what precedes, that every crystalline form, when we consider only what is invariable in it, *i.e.* the number and the respective inclinations of its faces, is so truly characteristic that it may serve of itself to determine, independently of every other consideration, the species to which the crystal belongs that presents it, provided it be not a cube, a regular octahedron, a regular tetrahedron, a rhomboidal dodecahedron, or a regular hexahedral prism. Thus the form of the dodecahedron with triangular scalene faces inclined among each other alternately by 144° 20′ 26″, and 104° 28′ 40″, indicates by itself a variety of carbonated lime.

Hence it would be possible to compose a method, by means of which, any crystalline form being given, we might succeed in ascertaining in what species it ought to be placed.

It is easy to perceive, that by considering the faces of crystals relative to their number which varies from four to 60 and upwards, with their vertical positions, inclined or horizontal, with the other modes of existence of which they are susceptible, we should have divisions and subdivisions so much the clearer, if geometry was called in to determine and circumscribe them. A method of this kind would be purely factitious, but it would fulfil its principal object: and we might even conceive that a geometrician with its assistance, who was no naturalist, and who had only before his eyes the collection of crystalline forms executed in wood, might succeed in arranging this collection. There would only be the forms common to various species which would lead to several names, among which we could only choose from the inspection of the natural crystals, by combining with the form a second character on which the last step would depend, by which to attain our object. Thus the taste joined to the cubical form would instantly indicate muriate of soda. A metallic colour of a bronze yellow reflected by a body of the same form would characterize sulphurated iron.

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* We may measure these angles by means of a card properly cut, or by two very thin rules of steel, which turn on each other by means of a hinge,

17. *Structure. Mechanical Division.* The character furnished by this operation is, as we have already remarked, the only one which does not participate in the variations produced by the mixture of heterogeneous substances, the influence of which modifies the hardness, specific gravity, fusibility, &c. and even the results of the analysis. It may perhaps disappear in the unshapen masses which have undergone a confused crystallization; but wherever it is possible merely to have a glimpse of it, it is susceptible neither of more nor of less. It removes in some measure every thing which is merely accessary in the composition of a substance; and while in all other respects this substance marches through a succession of shades, the measurement of the primitive angles stops at the same degree; and as soon as the substance changes its nature, there is an abrupt leap in the value of the angles.

We may venture to hope, that those who peruse this treatise with attention will perceive the advantage which we have made of the character in question, for the determination of the species. In our first researches we had nothing further in view than to make it the basis of a theory fitted for throwing lights on crystallography. But the various applications which we have made of this theory led us to exclude from such a species crystals which had been referred to it, and which rejected the laws of structure of which the forms relative to this species were susceptible; whereas other crystals, hitherto placed in different species, were subject to laws which solicited their intimacy; and from that moment we conceived that this theory, which at first appeared restricted to a simple branch of mineralogy, could extend its influence to the whole science, and contribute to give more regularity and justness in the distribution of the subjects which it embraces.

18. *Fracture.* This ought not to be confounded with structure. Having broken a mineral, when we perceive internally a scaly, granulous or fibrous texture, this is the effect of an arrangement which preexisted in the body. But if we find an undulated surface, or a species of small scales, which are nothing else than very thin fragments, still partly adhering to the substance, this aspect is the effect of fracture. But as it depends originally on a certain

mode of aggregation, it is generally discovered in all the pieces of the same substance, and it is this which may serve as a character for recognising it.

Minerals in which there is wanting some one of the sections necessary for completing the primitive form, present a fracture properly so called at the place where these sections ought to exist. For example, in the amphibole, the joints parallel to the panes of the prism are very distinct, whereas we perceive none in the direction of the bases; so that the crystal is broken, instead of allowing itself to be divided in the same direction. There are therefore, in these cases, longitudinal joints with a transverse fracture. In other cases the s are parallel to the bases, and the fracture is longitu We shall point out the different directions accor which the fracture takes place; and when there wi o joints visible in any direction, as takes place wi pect to agate-quartz, we shall say that the fracture i finite.

[To b ued.]

LXIX. *Proceedings Learned Societies.*

ROYAL IETY.

May 31.—The conclusio Mr. Home's paper on the organs of generation in ovi arous animals, particularly the *squalus* or shark genu the opossum, was read. Mr. H.'s observations chie plied to the *squalus acanthias*, or picked dog-fish, common on the shores of Norfolk; and the kangaroo, particularly the latter, the young of which not deriving its nutriment by a navel-string, or from the uterus of its mother, is supported by external agents, of which air forms an essential part. He also noticed the fact, that fish deposit their eggs on rocks and plants near the surface of the water, which there contains more atmospheric air, and that this air is necessary to the life of the young fish, which are enveloped in a gelatinous fluid.

June 7,—was occupied in reading Mr. Brande's appendix to Mr. Home's paper, consisting of a chemical analysis of the peculiar gelatinous-like matter in which the ova of sharks, spawn of frogs, &c. are nourished. By the friendly assistance of Sir Joseph Banks, Mr. B. obtained some of what is called star-shot jelly from Lincolnshire (that substance found near marshes, which Mr. Pennant justly conjectured to be the excrement of herons after feeding on frogs); the matter which envelops frog-spawn, and that which includes the ova of sharks: these he respectively analysed, and found them to possess similar properties, but all very different from gelatin, however analogous in their external appearance, and which he concluded to be a peculiar animal matter not yet described. This jelly-like matter is insoluble by water, but it absorbs water in great quantities, and becomes proportionally enlarged in consequence: acids and alkalies, however, dissolve it; but in none of its characters does it evince any identity with gelatin or albumen.

A mathematical paper on multi-nomials, by Mr. Knight, was communicated to the secretary (Mr. Davy), and laid before the society; but it was not of a nature to be read.

Mr. Hubbard communicated a letter from Sir John ——, containing a plan for purifying the air of coal-mines. The author, having observed that workmen descend into wells with the greatest safety after throwing a quantity of water into them, proposes the like expedient to purify coal-mines, by projecting water, in quantities sufficient to absorb the choke-damp (carbonic acid gas), against the cieling of mines, by means of an instrument like a fire-engine, made with an end like that of a watering-pot, to throw the water like a shower-bath, and thus present the greatest possible surface to the noxious air. This machine, the writer concludes, might be both supplied with water and worked by the steam-engines in all coal-mines. Several other minute operations and less important advantages were stated as likely to result from the adoption of this plan; on concluding which, the society adjourned till

June 21,—when a part of a paper by M. Delille, translated from the French, was read, describing the *bohan upas*, or poison-tree, of Java. The author is a French physician, a member of the National Institute of Egypt, and transmitted this paper from the East Indies to the Royal Society, by means of an English lady. The botanical account of this poisonous plant he received from one of the French naturalists who accompanied Capt. Baudin, and who resided some time in Java; where he visited the interior of the country, and with much difficulty succeeded in prevailing on the natives to show him the different poison-plants, which they carefully conceal in order to use them during war. Hence the reason of so many fables as have been repeated respecting the extraordinary destructiveness and influence of the *upas*, which in the language of the Javanese signifies vegetable poison, and is applied only to the juice of the bohan tree, and another twisted-stemmed plant. The bohan is a large tree, which this writer considers a new genus: the other plant, yielding an equally powerful poison, is of the woodbine genus. The *upas*, or poisonous juice, is extracted by an incision in the bark with a knife, and carefully collected and preserved by the natives to be used in their wars. As to its diffusing noxious effluvia in the atmosphere, and destroying all vegetation around it, the absurdity of these stories is best exposed by the fact, that the climbing species requires the support of other plants to attain its usual growth. Dr. Delille made several experiments with the *upas* on dogs and cats. An incision was made in the thigh of a dog, and eight grains of *upas* dropped into it: shortly afte e dog began to vomit, and continued vomiting at interv till he became convulsed, the muscles of his head gre distorted, and he died in 20 minutes. Six grains were t into the thigh of another dog, which also vomited fir s undigested food, next a white foam, and died contra and convulsed in 15 minutes.—A cat was also treated i like manner; but she was still sooner and more convu d, and her muscles contracted: she continued leaping for a few minutes, and fell down dead. All these ani ls died crying and in great agony.—After repeating a nun er of experiments on the deleterious and prompt effects of this powerful poison when applied externally; the a hor gave a grain and a half to a dog, which he took int stomach, but it only produced a slight purging. To her four grains were given, which in about four hours pro d both vomiting and purging, and the dog died in the course of half a day. On examining the bodies of these animals after death, no very extraordinary appearances were discovered; the ventricles of the heart were full of blood, and some slight traces of inflammation appeared in the stomach; but the derangement was not so great as might have been expected from such a violent and sudden death. From this circumstance, the author concluded that the absorbents had transmitted the poison to the nerves of the stomach, and that this peculiar vegetable poison acts exclusively on the nerves.

⁂ In the account we gave (in our last number) of Mr. Macartney's paper on luminous animals (not insects merely) there were some mistakes. He stated that they belong to several classes, as *mollusca*, *insects*, *worms* and *zoophytes*.—There is but one species of mollusca luminous, the pholas dactylus. The medusæ that were ranked by Linnæus amongst mollusca are now placed more properly with zoophytes.—The medusa scintillans, one of the luminous species, was not given to Mr. Macartney by Capt. Horsburg, but discovered by himself. The pyrosoma atlanticum of Peron was called, in our account, the pyrosoma *atlantica* of *Perot.*

A great part of Mr. Macartney's interesting paper is taken up with the anatomical description of the organs from whence the light issues in certain species.

ROYAL INSTITUTION.

In the concluding lecture at the Royal Institution, the large Voltaic apparatus, consisting of 2000 double plates of four inches square, was put into action for the first time. The effects of this combination, the largest that has ever been constructed, were, as might have been expected, of a very brilliant kind.

The spark, the light of which was so intense as to resemble that of the sun, struck through some lines of air, and produced a discharge through heated air of nearly three inches in length, and of a dazzling splendour. Several bodies which had not been fused before, were fused by this flame; the new metals discovered by Mr. Tennant, iridium, and the alloy of iridium and osmium. Zircon and alumine were likewise fused;—charcoal was made to evaporate, and plumbago appeared to fuse in vacuo. Charcoal was ignited to intense whiteness by it in oxymuriatic acid gas, and volatilized in it, but without effecting its decomposition. A large Leyden battery, containing 24 coated jars, was charged by a momentary contact of the wires to a degree that required from 20 to 30 turns of Nairne's electrical machine of eight inches diameter. All the electrical phænomena of the passage of electricity to a distance; the discharge through a Torricellian vacuum; the attractions and repulsions of light bodies, were demonstrated in a distinct way by means of this apparatus. It may be hoped that the application of so powerful an instrument, and such easy methods of producing the most intense heat, will lead to some new facts in analytical science.

WERNERIAN NATURAL HISTORY SOCIETY.

At a meeting of this Society, 19th May last, the Rev. John Fleming, Bressay, read an account of several rare animals found by him in Shetland, particularly Pleuronectes punctatus, a specimen of which he exhibited to the Society; Lucernaria quadricornis; Echinus miliaris, &c.; and some undescribed species, particularly a Flustra, which he proposed to call *Flustra Ellisii,* in honour of Mr. Ellis, the illustrator of the corallines.—At the same meeting, Dr. John Barclay read remarks on some parts of the structure of the large marine animal cast ashore in Stronsay last year.

At a meeting on the 26th May, Dr. John Yule read a summary of experiments and observations on the germination of the Gramineæ, in which he stated some new facts respecting the œconomy of this useful class of plants, illustrated by a series of drawings and specimens of the germinated seeds of the Cerealia or cultivated species; and of the buds of the stem, and panicle of viviparous Grasses.—And the secretary read a communication from William Fitton, esq. on the porcelain- th of Cornwall.

FRENCH NATI INSTITUTE.

[Concluded f p. 399.]

Messrs. Majendie and De have communicated to the class their experiments made o animals by means of the matter with which the natives f the Isles of Java and of Borneo poison their arrows. See ROYAL SOCIETY, two pages back.]

M. Vauquelin has also made some experiments of this kind: at the end of his chemic analysis of the juice of the belladonna, he speaks of the e ects of this substance on animals. Those which he fo d to swallow it, fell down as if intoxicated, in a deliriu ecisely similar to that produced by opium.

M. Sage has reported on the same subject some more experiments, which chance threw in his way, or which he collected from authors, and which confirm the action of this juice on the nervous system, and particularly on the brain.

A young practitioner in medicine, whose name has been mentioned in former annual reports, M. Nysten, has attempted to ascertain the effects of different gases injected into the blood-vessels of animals: he used the greater part of the gases with which we are acquainted. Atmospheric air, oxygen gas, the oxidulated azotic, carbonic acid, carbonic, phosphuretted and hydrogenated gases, &c. are in no respect deleterious. The oxy-muriatic, nitrous acid, and ammoniacal gases seem to act by very violently irritating the right auricle and the pulmonary ventricle. The sulphuretted hydrogen, oxide of azote, and azotic gases injure the contractile power of these parts: others also change the nature of the blood so completely, that respiration can no longer conver it from venous blood into arterial, &c.

MEDICINE AND SURGERY.

M. Desessartz has communicated the history of an epidemic disease, which raged in three adjoining villages at the same time. Although generally depending on the inclemency of the weather, and on the bad quality of the fruits of the season, this epidemy presented a sensible variety in the nature and intensity of the symptoms, which necessarily gave rise to essential modifications in the treatment. He shows that the differences depended on the exposure peculiar to each of these villages, on the quality of their respective soils, productions, and way of life of the inhabitants.

M. Sage has presented to the class some reflections on the best means of remedying the bite of the viper, and a description of the effects of the poison of the tarantula, with the means employed in Spain for remedying it. All these remedies consist in the internal and external use of the volatile alkali.

M. Tenon continues to enrich the art of surgery. He has communicated to the class three memoirs, one on the exfoliation of the bones, another on the operation of trepanning the cranium, and a third on some kinds of hernia. In the first he inquires if the bones of the great extremities of the body are exfoliated after amputation; and it results from his numerous experiments on dogs, hares, and sheep, that after all amputations, the denuded extremity of the long bones is exfoliated, as also happens to the flat bones when laid bare, before being covered with a cicatrix. In the second he gives the description of all the phænomena which take place in the cure of a wound in the head, in consequence of which the trepan was resorted to, and which was cured after 151 days' treatment.

In the third, he describes an ingenious method resorted to by himself, for the reduction of two crural herniæ, and gives some observations on the operation for an inguinal hernia. In order to succeed in the reduction of these crural herniæ, "I directed an assistant to get upon the bed of the patient, and place himself between the knees of the latter, making him raise them as high as possible: the pillows being withdrawn, I employed another person to hold the leg and foot on the ruptured side, and to turn strongly inwards the great toe as well as the knee and thigh." When matters were thus arranged, M. Tenon succeeded by degrees in returning the intestines into the abdomen, so that the patient had no occasion for any operation.

M. Pel-

M. Pelletan has communicated to us some interesting observations on aneurisms, and on the chirurgical operations which these diseases require.

M. Larrey has submitted to the class a memoir, on which a report has been made, and which suggests, in cases of gunshot wounds followed by gangrene, that we should not expect to put a stop to the gangrene by performing amputation.

AGRICULTURE AND RURAL ŒCONOMY.

M. Sylvestre, in the name of a committee, has made a report to the Institute on a work by M. Yvart, entitled, Method of improving Agric : by Manures (*assolemens*). " The science of manurin 's the report, " has for its object to render any soi le of yielding crops constantly in the most profi ianner, and without being deteriorated. This work, lds, fulfils the important object which the author p d, and merits the approbation of the class."

M. De Cubiere has read noir on the bald cypress (*cypress chauve*) : it has bject to enlighten agriculturists, to give them new s to the vegetation of this fine tree, and to make t quainted with all the advantages which they may from its culture. The report which has been ma is work, by our colleague, M. Mirbel, has obtained f De Cubiere the approbation of the class.

M. Leblanc, who spent ral years in America, has communicated to us his views with respect to the facility of naturalizing the Vigonia sheep in the Alps and Pyrenees, and on the uses of their wool.

M. Poyfere-de-Ceré read a notice on the washing of the superfine wools in Spain, and on the great washing-house at Alfaro, near Segovia; a memoir, in which will be found an account of an expeditious, easy, and œconomical method of cleansing wools, and constructing washing stations.

Finally, our colleague, M. Percy, having collected in Spain some curious observations on the manufacture of the amphori and alcazaras used by the Spaniards for keeping their liquors cool, has communicated them, with the addition of some important reflections as to the utility of these vessels, and on the influence which they exercise on the liquids they contain.

LXX. *Notices respecting New Books.*

Mr. Leybourn, of the Royal Military College, has just published the tenth number of the Mathematical Repository, containing solutions to the mathematical questions proposed in the eighth number, and a series of new questions to be answered in a subsequent number; an essay on polygonal numbers; a new demonstration of the binomial theorem; an illustration of the forty-seventh proposition of the second book of the Principia; a curious indeterminate problem; solutions to a curious problem in dynamics; and a continuation of Le Gendre's memoir on elliptic transcendentals.

Mr. W. Moore, of the Royal Military Academy, has in a good state of forwardness, A Treatise on the doctrine of fluxions; with its application to all the most useful parts of the true theory of gunnery, and other very important matters relating to military and naval science. The fluxions will be preceded by such parts of the science of mechanics as are necessary for reading the work without referring to other authors; and the whole will be so arranged, that any person moderately skilled in algebra, geometry, and trigonometry, and having a knowledge of the most common properties of the conic sections, may proceed to these inquiries with every interest and success. The whole will be printed in one volume octavo, and will be particularly adapted to all military institutions of eminence.

Mr. Marrat, of Boston, Lincolnshire, has in the press a work on mechanics, which is principally intended for the use of schools, &c.

The author's principal aim in composing this work, has been to make the subject easy to be understood by students, to facilitate the business of instruction, and to condense as much useful matter as possible into a small compass. In order also to blend theory with practice, and to remove the irksomeness which students mostly complain of in studying the theory, a great number of examples will be given in almost every section; these will be found of service in fixing the principles in the mind, and cannot fail of rendering the subject more easily attainable, and, consequently, of creating a stimulus to further exertions.

The work will be divided into five books; the first of which treats of statics, the second of dynamics, the third of hydrostatics, and the fourth of pneumatics. In these four books the subjects are prosecuted as far as could be done without introducing the fluxional calculus; but to render the work of more general utility, and to accommodate students in the higher classes, a fifth book is added, in which several branches in the preceding books are very much extended. The work will be comprised in one octavo volume, and will be accompanied by thirteen copper-plates.

M. VIBORG, professor in the Royal Veterinary School, in Sweden, has published a dissertation "On the use of the flesh of horses."—The publication of this paper has had the effect of introducing se of this article as food throughout Sweden, and th hers' shops are now supplied with the carcases of , in addition to those of oxen. M. Viborg assure ders, that the flesh of those animals, when roasted, is ble to that of oxen.

LXXI. *Intelligen iscellaneous Articles.*

DE LUC'S IC COLUMN.

THE small bells notice last three numbers, still continued to ring on th nstant (June), as they had done since the 25th of without being known to have once ceased ringi are happy to be enabled to add, that those who wi ess electric columns, fitted up in the form of rods, as d in our number for March last, may obtain them of Mr. Blunt, optician, Cornhill.

The success of the several charitable institutions for the relief of the indigent blind, has suggested the humane idea of bringing forward, for the equal relief of their opulent brethren in this country, a plan, similar to that by which M. Haüy, in Paris, taught them, several years ago, reading, writing, arithmetic, music, and the rudiments of the sciences generally.

LOCUSTS.

Rome, 29th May, 1816.

For some days past crowds of people, excited by curiosity, have been thronging the banks of the Tiber to witness a singular phænomenon. A wind from Africa has brought into these countries an immense swarm of locusts. These insects, having wasted the country, and now unable to find subsistence, have waged war among themselves, and devour each other. The weaker party take flight, and, pursued by the vanquishers, throw themselves in myriads into the Tiber. The day before yesterday this river was covered with them.

Report to the Committee of the Honourable House of Commons, on the Petition of the Trustees of the British Museum; respecting the Purchase of Mr. Greville's Collection of Minerals.

London, May 9, 1810.

We the undersigned, having been requested by the committee of the honourable house of commons, on the petition of the trustees of the British Museum, to make a careful examination of the collection of minerals belonging to the right honourable Charles F. Greville, and to put a value upon the same with as much fairness and accuracy as possible;—have now to report:

First.—That on the 2d of this month we assembled at the house of the late Mr. Greville on Paddington Green, commenced our inspection of the collection of minerals; and continued the same, day after day, up to the 9th instant.

Second.—That we have found the specimens scientifically arranged, for the greater part, in glazed drawers, which are contained in cabinets made of beautiful mahogany.

Third.—That, exclusive of these cabinets, there are two others, containing models in wood and in clay, the former having been most accurately made by the Count de Bournon for the late Mr. Greville, exemplifying and elucidating the various figures and modifications of crystallized mineral substances; a series of great importance to mineralogical science.

Fourth.—That, in addition to the minerals contained in the drawers, there are arranged on the upper part of the cabinets many large and magnificent specimens, several of which are uncommonly rare and highly valuable.

Fifth.—That the whole collection consists of about 20,000 specimens.

Sixth.—That the specimens in general throughout the collection appear to us to have been selected with very great judgement, both as to their utility and beauty.

Seventh.—That the series of crystallized rubies, sapphires, emeralds, topazes, rubellites, diamonds, and precious stones in general, as well as the series of the various ores, far surpass any that are known to us in the different European collections.

Eighth.—That we consider the entire collection to be equal in most, and in many parts superior, to any other similar collection which any of us have had opportunities of viewing in this and other countries.

Ninth.—That having accurately examined and separately valued the different cabinets and detached specimens, we find the total amount to be *thirteen thousand seven hundred and twenty-seven pounds.* £13,727.

Wm. Babington.
L. Comte De Bournon.
Richard Chenevix.
Humphry Davy.
Robert Ferguson.
Charles Hatchett.
Wm. H. Wollaston.

We whose names are u itten, and who have signed
the foregoing report, thi ut an act of justice on our
part, to request permis state to the committee
the very great services v ave been rendered by the
Count de Bournon, du whole of the inspection
and valuation of the col with which he alone was
well acquainted, having ally contributed to form it,
and having been occupied everal years in arranging it
for the late Mr. Grevill thout the able assistance of
the Count de Bournon, s celebrated for his profound
knowledge in mineralo inspection and valuation
would have required a ver length of time, and after all
would most probably ha less accurately performed.

We therefore unanim ncur in giving this public
testimony to the merits a ices of Count de Bournon.

We also have to make our acknowledgments to Mr. Lowry and Mr. Jonville, who obligingly attended the inspection at our request, and rendered us much valuable assistance.

Wm. Babington.
Richard Chenevix.
Humphrey Davy.
Robert Ferguson.
Charles Hatchett.
Wm. H. Wollaston.

LIST OF PATENTS FOR NEW INVENTIONS.

To Joseph Halliday, master of the band belonging to the Cavan regiment of militia, for certain improvements in the musical instrument called the bugle horn.—May 5, 1810.

To William Chapman, of the town and county of Newcastle-upon-Tyne, civil engineer, for a wheel or wheels to be moved by water, steam, or any other suitable fluids or gases, and to be applicable to mechanic or other purposes where a moving force is required.—May 9.

To John Bosworth, of Birmingham, in the county of

Warwick, coal dealer, for improvements in carriages to facilitate the unloading of heavy coals and other things.—May 9.

To Sir Issac Coffin, bart. vice admiral of the blue squadron, who, in consequence of a communication to him by a certain person residing abroad, is become possessed of a new invention of a perpetual oven for the making of all kinds of bread, a patent for the same, dated the 15th of May.

To James Bell, of Fieldgate-street, Whitechapel, sugar-refiner, for his certain improvements in the manner of refining sugar, and of forming sugar loaves of a particular description.—May 17.

To Charles Stewart, of the parish of St. Martin in the Fields, in the county of Middlesex, cabinet-maker, for certain improvements in the construction of dining- and other tables.—May 22.

To John Onions, of Broseley, in the county of Salop, iron-master, for his machine for thrashing corn and other grain, on a new construction.—May 22.

To William Docksey, of the city and county of Bristol, millwright, for improvements in the process of manufacturing an article commonly called ivory black, and for pulverizing, grinding, or reducing to a subtle and fine powder, all articles capable of a more easy separation of their parts or constituent principles by torrefaction, heating, or calcination in open or close kilns, ovens or furnaces, especially potter's clays, flints, colouring and glazing materials. —May 22.

To Joseph Anthony Berrollas, of Cowper's-row, Clerkenwell, in the county of Middlesex, watchmaker, for his warning watch on a new construction.—May 26.

To George Hickford, of Chadacre-Hall-Farm, in the parish of Strumpling, in the county of Suffolk, farmer, for his improvements upon the plough heretofore used for draining land, and the machine for drawing the same through the ground, whereby a horse will be able to perform a much greater quantity of work in the same time than by the methods now in use.—June 8.

To John Williams, of Cornhill, in the city of London, stationer, for certain apparatus or additional parts to be applied to and used with wheel carriages in order to render the same more safe and commodious.—June 8.

To Mary Townley, of Ramsgate, in the county of Kent, for the prevention or cure of smoky chimneys.—June 8.

To Arthur Woolf, of Lambeth, in the county of Surry, engineer, for certain improvements in the construction and working of steam engines, calculated to lessen the consumption of fuel.—June 9.

To Joseph Warren Revere, of Boston, in the United States of America, at present residing in London, for a new and improved method of splitting hides and shaving leather —June 19.

To Joseph Chsild Daniell, of Frome, in the county of Somerset, clothier, for certain improvements on machines called gigs and shearing frames, used for dressing cloths, and in the clothing manufactory.—June 19.

To Malcolm M'Gregor, of Bell Yard, Carey-street, in the county of Middlesex musical instrument maker, for certain flute or musical nstruments with improved keys, which keys are al icable to flutes and various other wind instruments n ise.—June 19.

To George Adams, of oodlands, in the parish of Lindridge, in the county rcester, farmer, for an improved method of cultiv land, and of feeding and consuming the produce t by cattle and sheep, and of preserving and applying nure of and made by such cattle and sheep, by mea tain houses made and built for the protection of ca sheep from weather, and feeding the same thereon veable by means of wheels, slides, iron railways, or se, invented and found out by him.—June 19.

To John Lindsey, (l -col. of the 71st regt.) of Grove House, in the c f Middlesex, for his boat and various apparatus, wh eavy burthens can be conveyed on shallow water on rivers wherein shoals and other difficulties impede navigation, whereby the lives of men will be saved from wrecks and other situations of imminent danger at sea or on rivers, whereby the apparatus above specified may, in its consequences and constructions, embrace other important results highly beneficial to the British navy and commerce, by enabling the bottoms of ships to be examined with accuracy and expedition without the necessity of moving the masts or cargo.—June 19.

To William Bell, of Handsworth, near Birmingham, in the county of Warwick, engineer, for his improved machine for the purpose of cutting pasteboard, or cards out of pasteboard or paper, and for cutting various other articles.—June 19.

To James Frost, of Little Sutton-street, Clerkenwell, brass-founder, and James Frost the younger, his son, also brass-founder, for improvements upon cocks, or an improved lock cock.—June 22.

METEOROLOGICAL TABLE,

BY MR. CAREY, OF THE STRAND,

For June 1810.

Days of Month.	Thermometer.			Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
	8 o'Clock, Morning.	Noon.	11 o'Clock, Night.			
May 27	47	60°	46°	29·95	40	Fair
28	48	56	45	30·13	53	Fair
29	46	51	46	·40	71	Fair
30	49	65	51	·30	66	Fair
31	52	66	50	·30	60	Fair
1	53	65	52	·29	77	Fair
2	54	69	54	·25	79	Fair
3	53	66	50	·22	63	Fair
4	52	65	55	·20	90	Fair
5	54	55	50	·24	41	Cloudy
6	51	70	55	·19	79	Fair
7	54	67	56	·11	55	Fair
8	56	70	60	·05	76	Fair
9	59	73	55	29·94	52	Fair
10	59	70	55	·72	41	Cloudy
11	58	68	54	·82	53	Fair
12	54	66	53	·94	59	Cloudy
13	55	60	50	·85	0	Rain
14	50	63	54	30·15	70	Fair
15	54	66	51	·19	79	Fair
16	52	62	46	·03	55	Fair
17	50	60	54	29·95	51	Fair
18	56	70	60	·95	65	Fair
19	60	70	60	·95	51	Cloudy
20	63	72	63	·95	42	Cloudy
21	64	77	66	30·20	92	Fair
22	66	73	55	·30	55	Cloudy
23	54	69	55	·38	57	Fair
24	56	73	60	·25	61	Fair
25	59	73	66	·09	77	Fair
26	59	61	55	·05	30	Cloudy

N. B. The Barometer's height is taken at one o'clock.

ERRATUM.

In Mr. Davy's article (*see page* 409) for Plates IX and X, *read* Plates XII and XIII.

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Riding,

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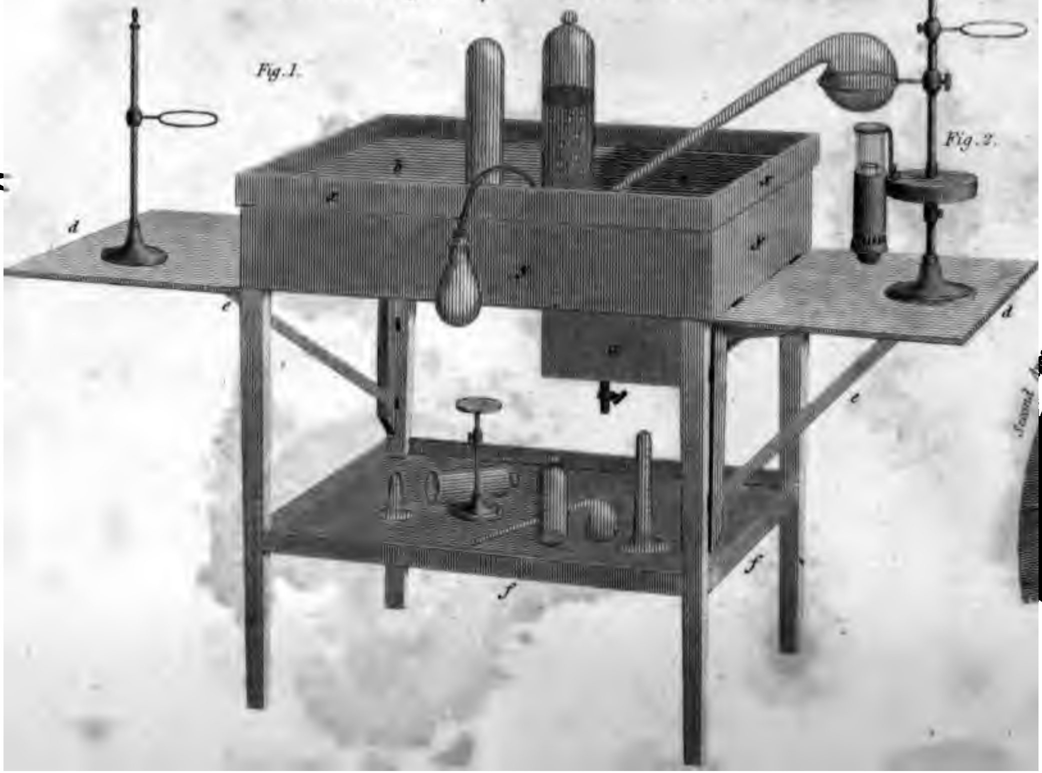
Accum's Hydro-pneumatic Table.

First Appearance.

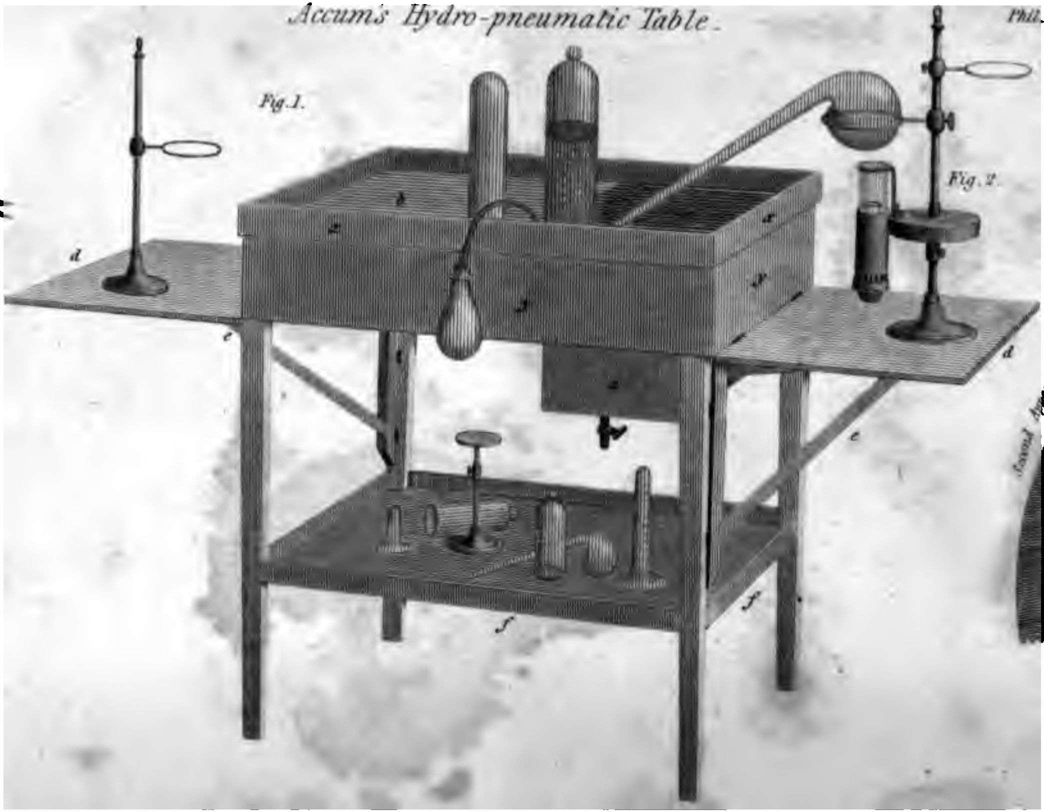


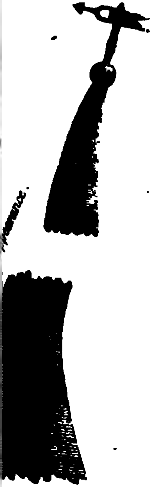
Second Appearance.

UNIV. OF MICH.

Accum's Hydro-pneumatic Table.

First Appearance.

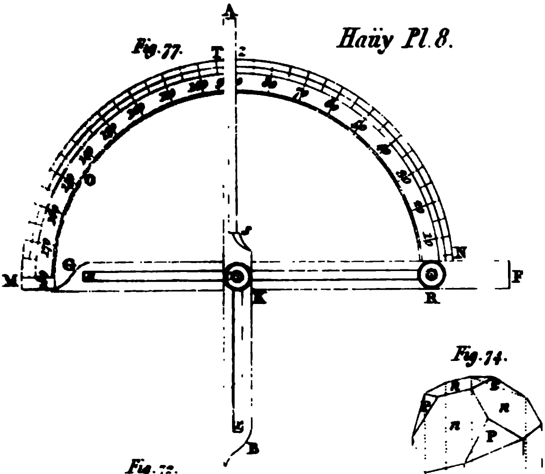




Second Appearance.

UNIV. OF MICH.

Fig. 77.



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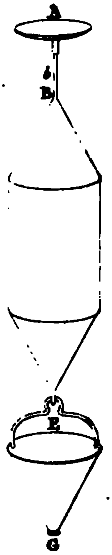

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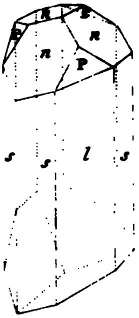

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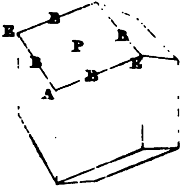


Fig. 73.

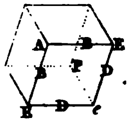
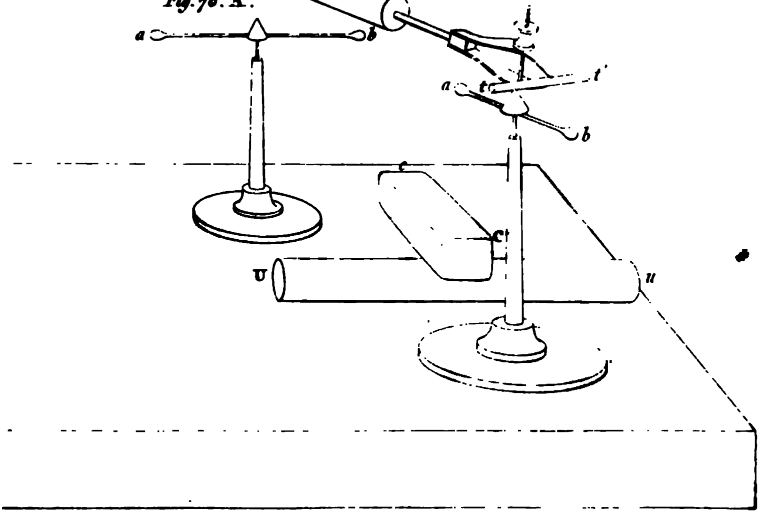


Fig. 76. A.

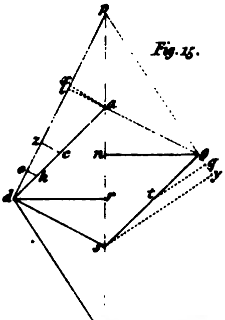
Fig. 76. B.



Lowry Sc.

UNIV. OF MICH.

Haüy Pl. 10.



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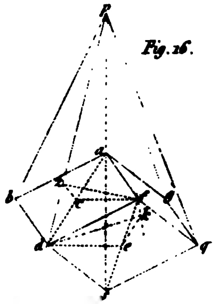

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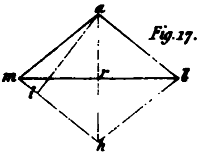

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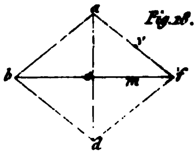

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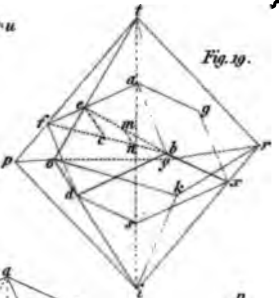

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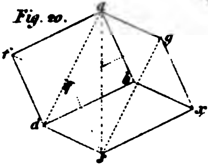

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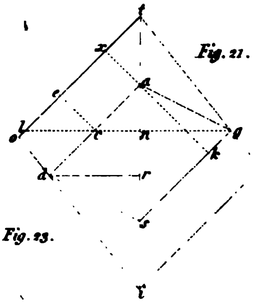

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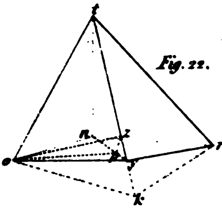

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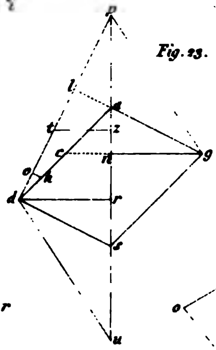

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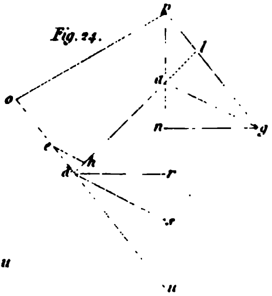

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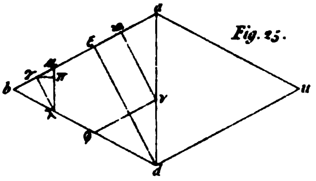

Fig. 25.

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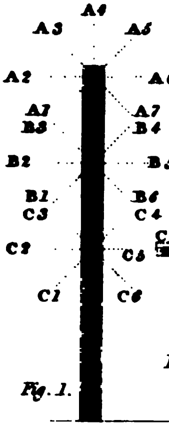

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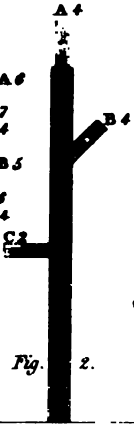

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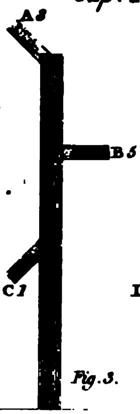

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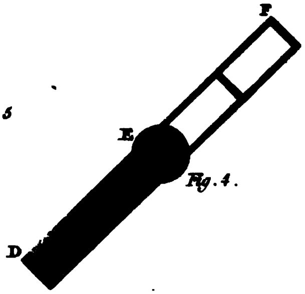

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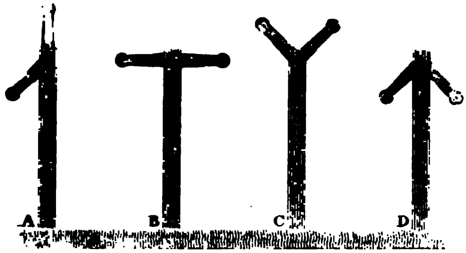

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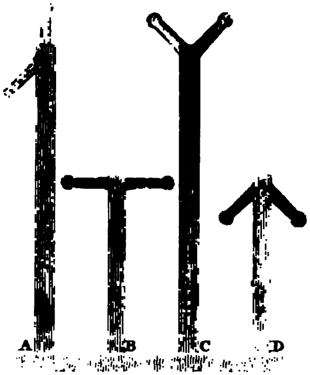

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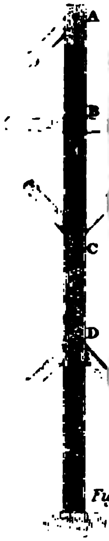

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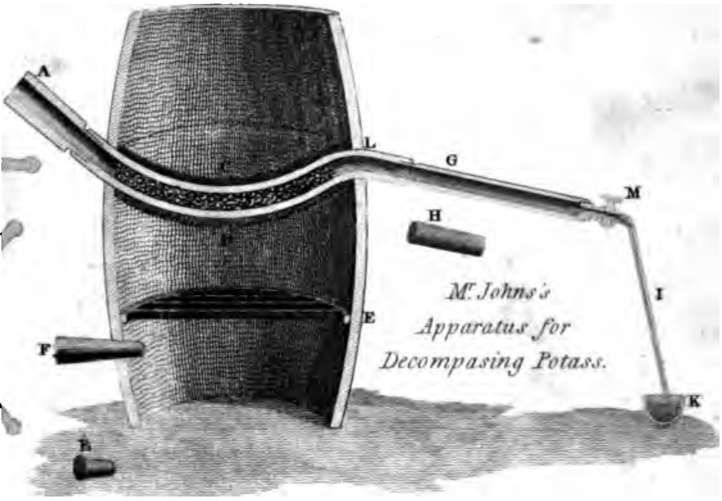
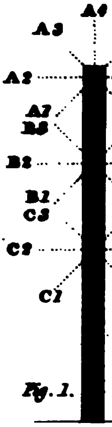


Mr. John's Apparatus for Decomposing Potass.

Capt. Pasley's Telegraph.



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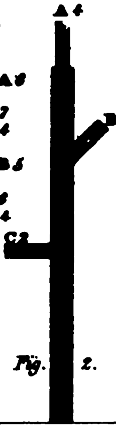

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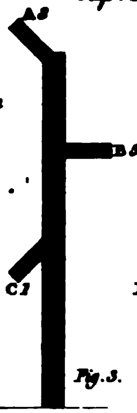

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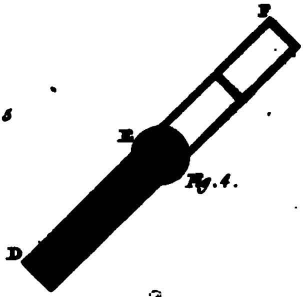

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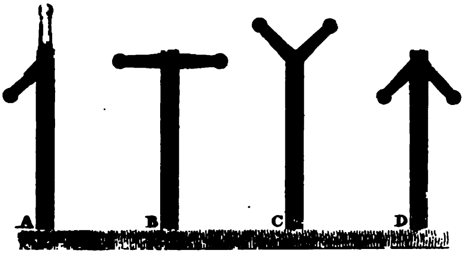

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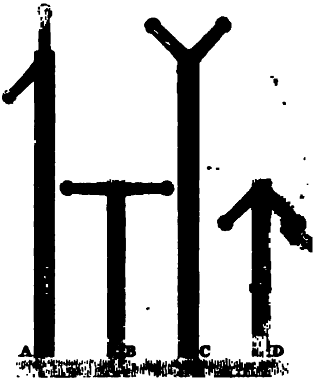

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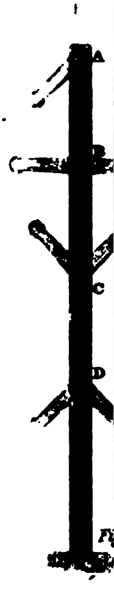

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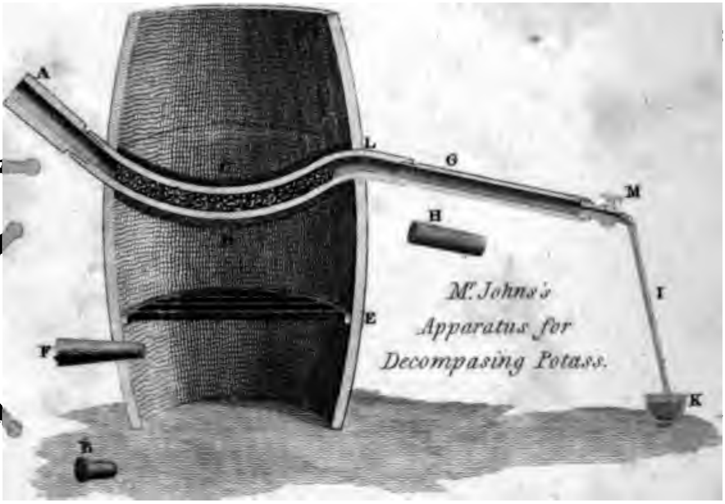

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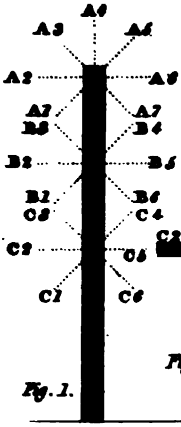

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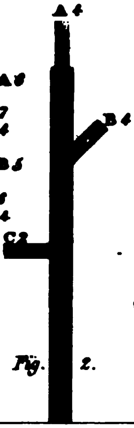

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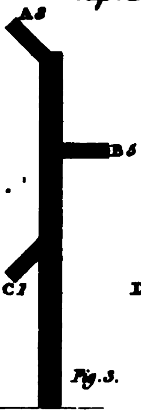

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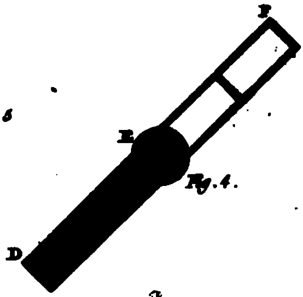

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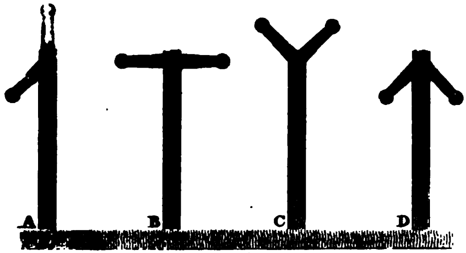

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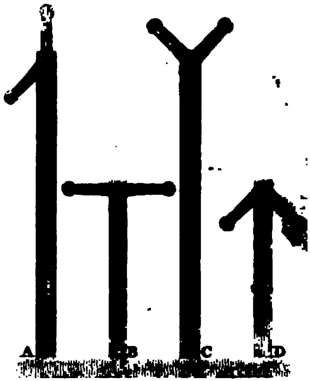

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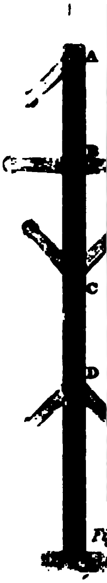

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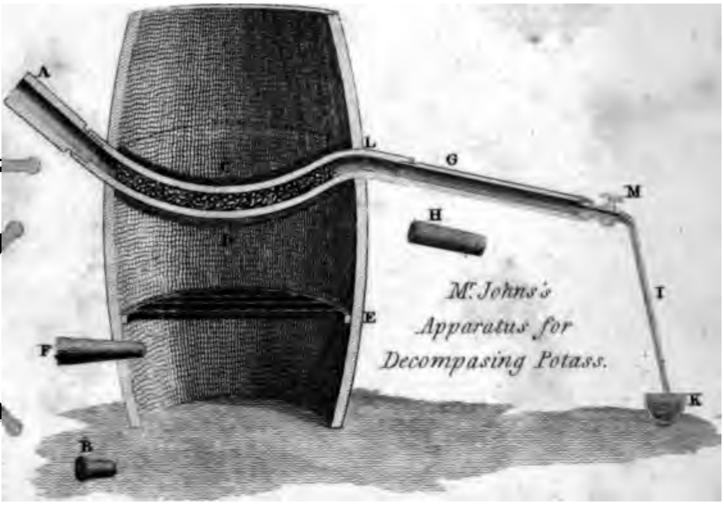

Mr. Johns's Apparatus for Decomposing Potass.

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Composition & Decomposition of Forces.

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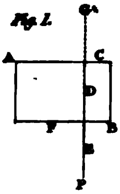

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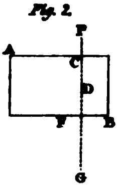

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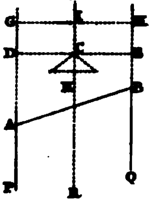

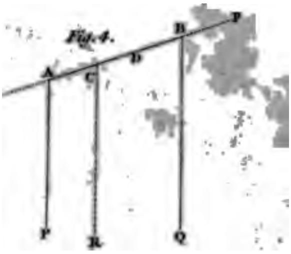
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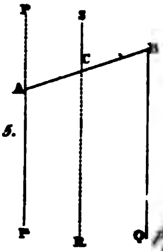

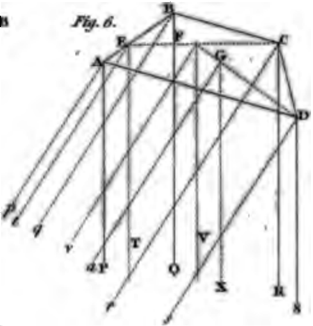
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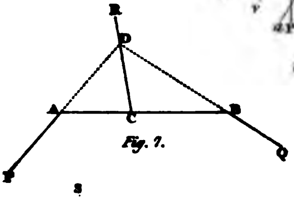
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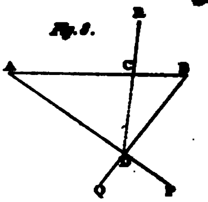

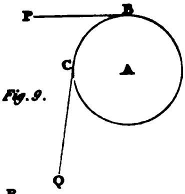
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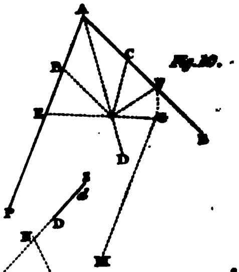
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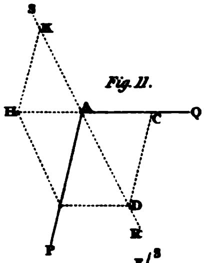
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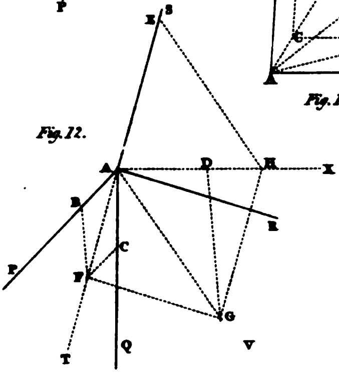

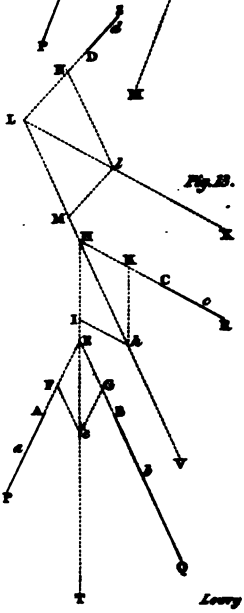
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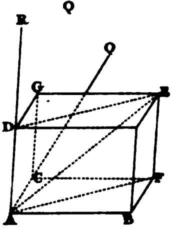
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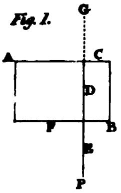

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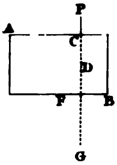

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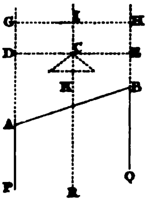

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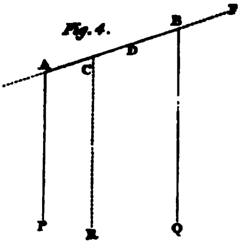

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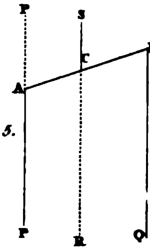

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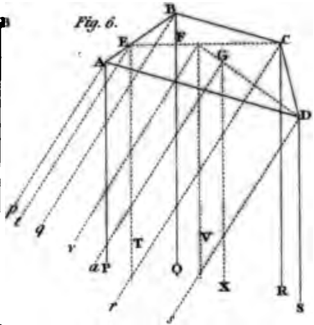

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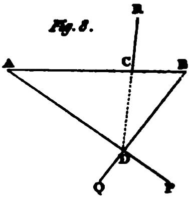

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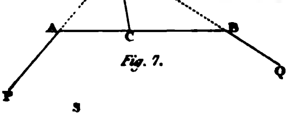

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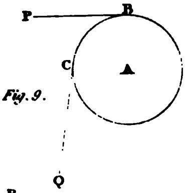

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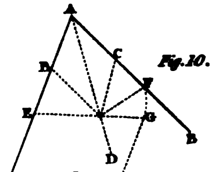

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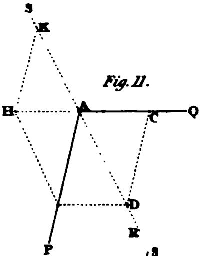

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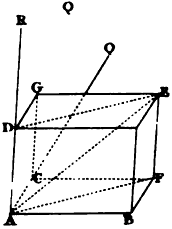

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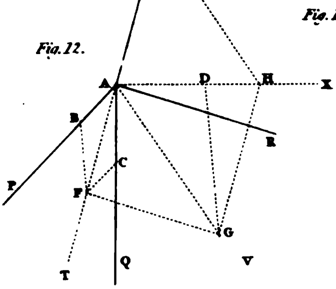

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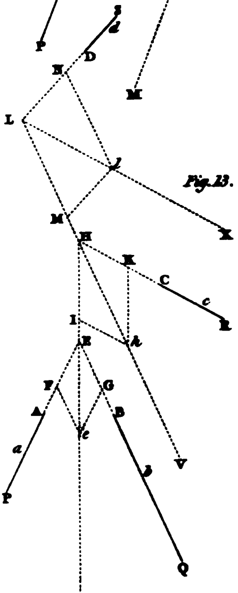

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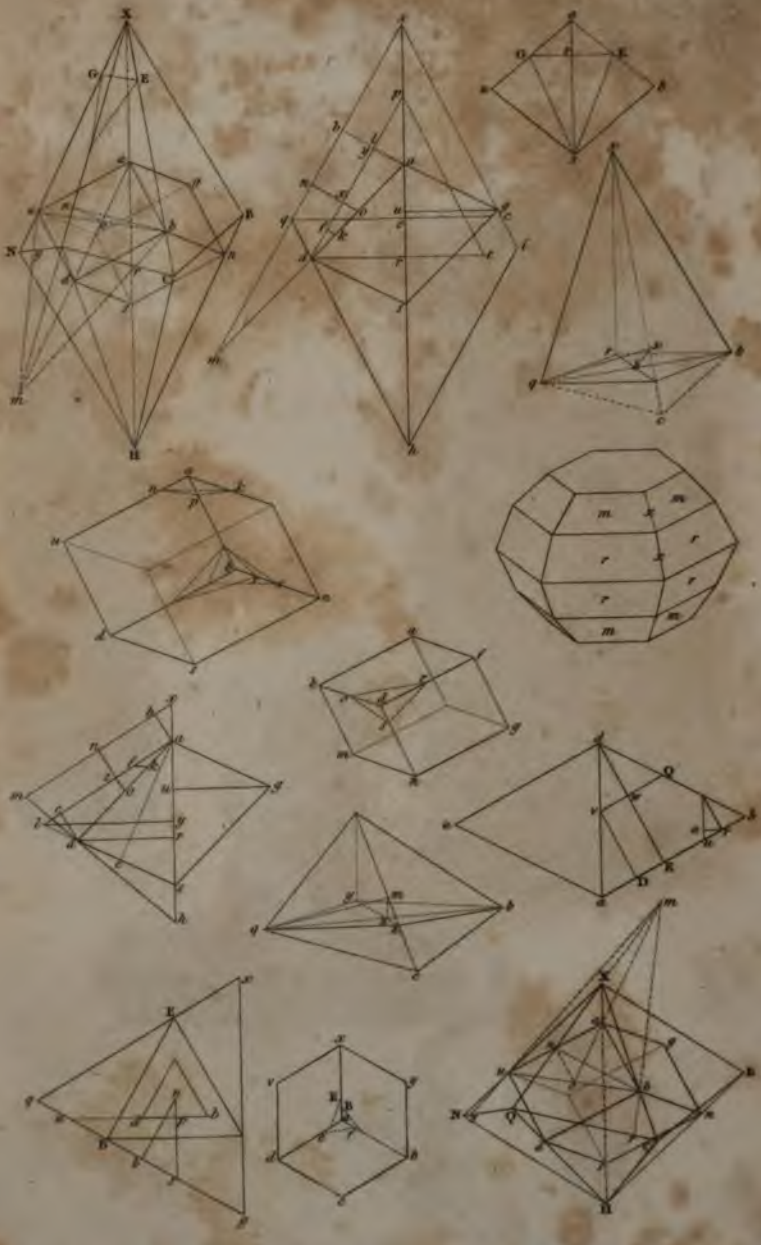
HAÜY PLATE II.



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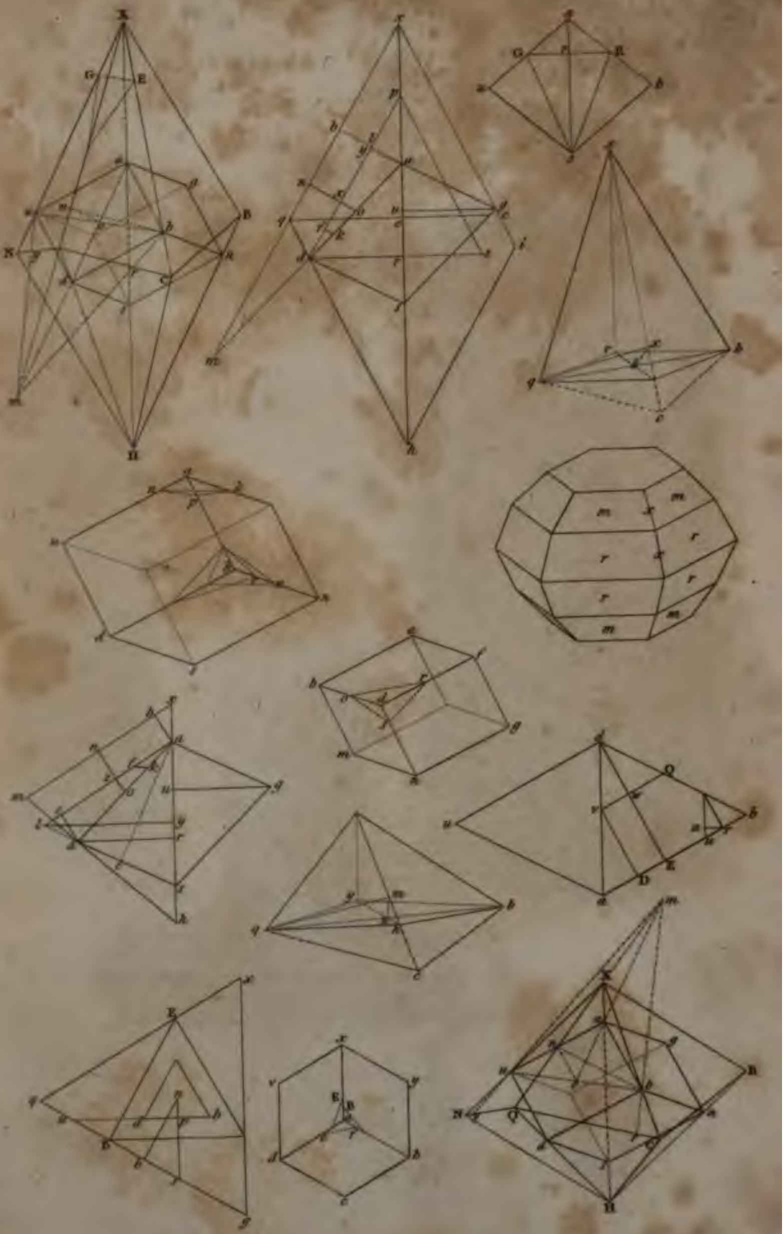
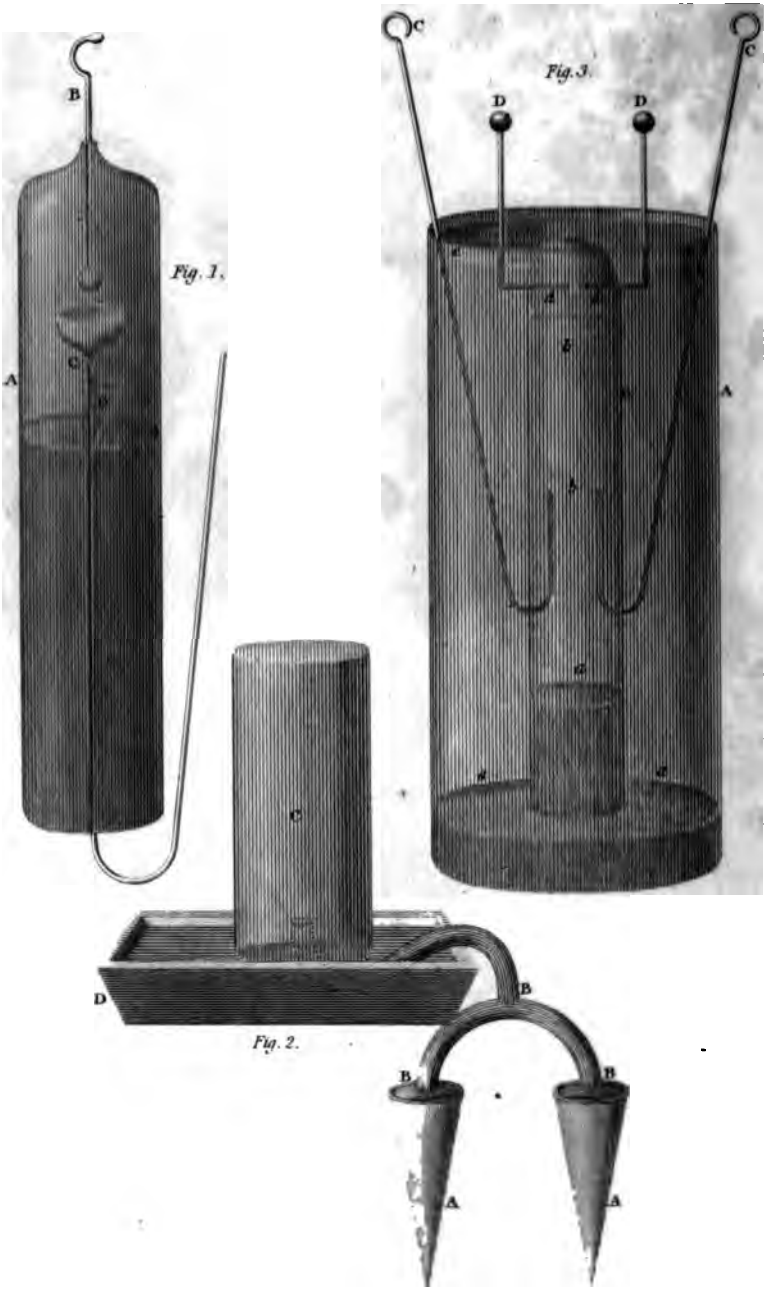
HAÜY PLATE II.



Lowry sculp.

Professor Davy's Electro-chemical Apparatus.



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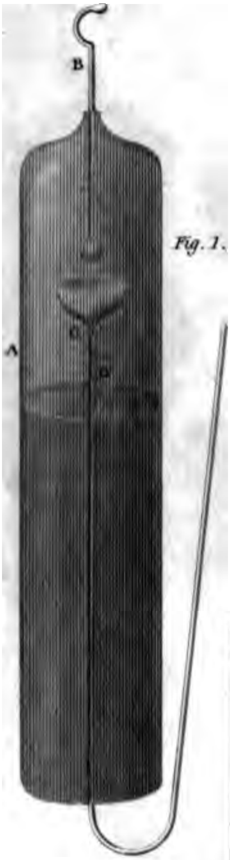
Professor Davy's Electro-chemical Apparatus.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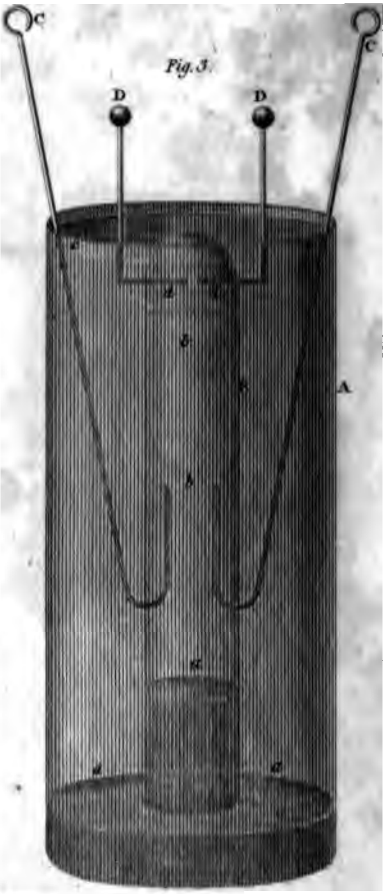
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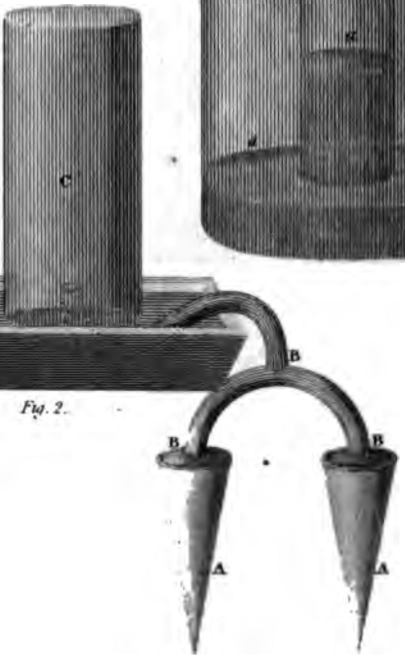
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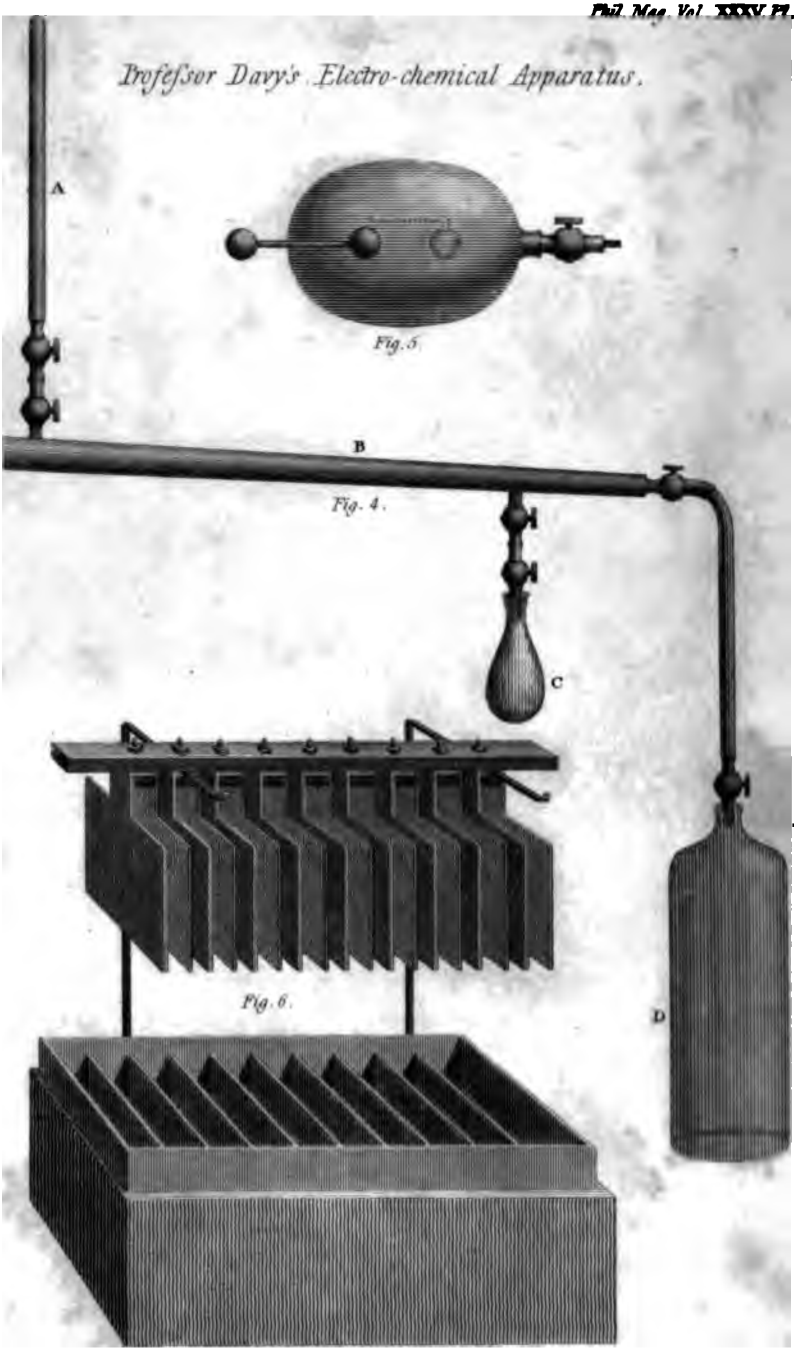

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Fig. 4.

Fig. 6.

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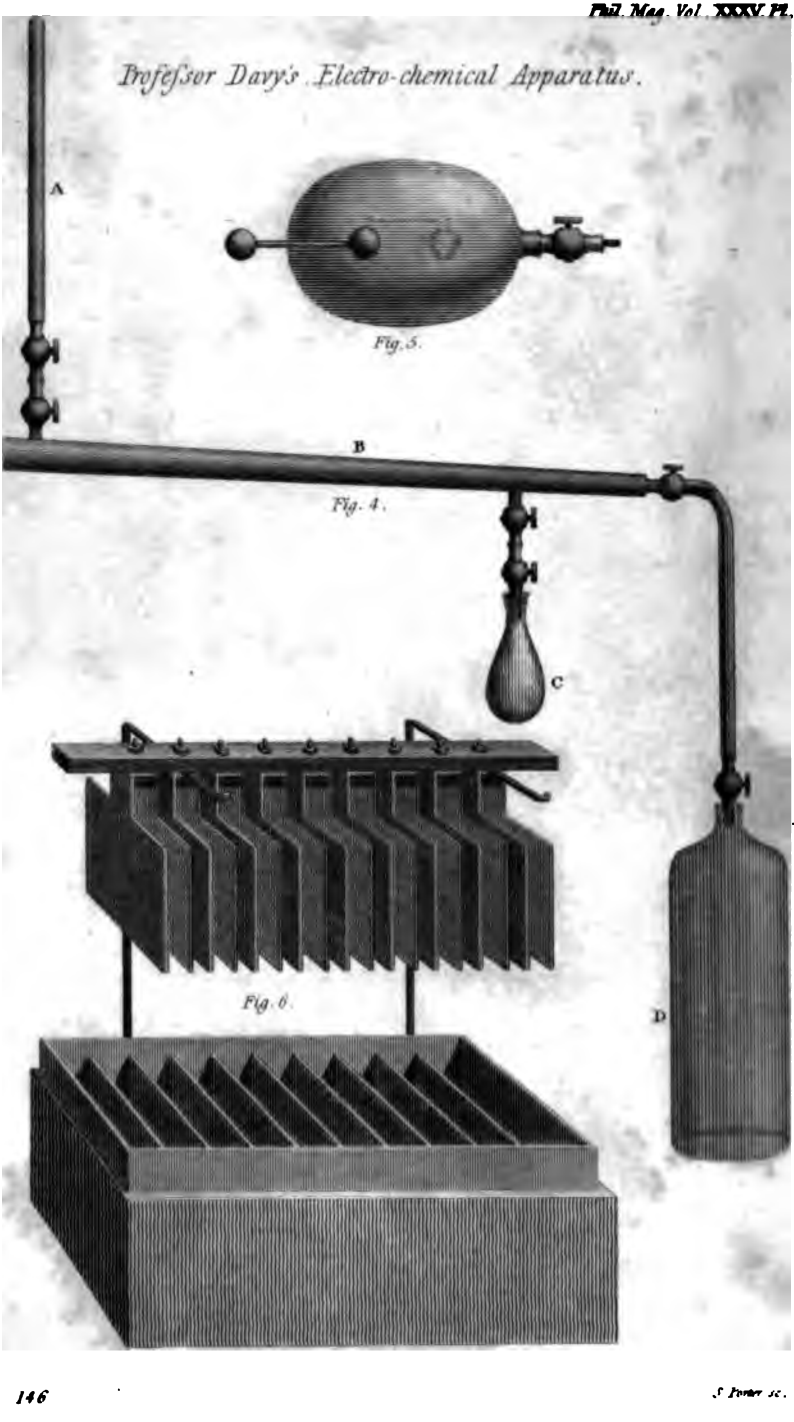

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COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

GEOLOGY, AGRICULTURE,

MANUFACTURES AND COMMERCE.

BY ALEXANDER TILLOCH,

M.R.I.A. F.S.A. EDIN. AND PERTH, &c.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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THE

PHILOSOPHICAL MAGAZINE.

I. *A Review of the First Volume of M.* J. A. De Luc's *Geological Travels in the North of Europe: with Remarks on some of the Geological Points which are therein discussed. By a* Correspondent.

To Mr. Tilloch.

Sir, As a reader of your Magazine, I have been much gratified of late, by the extracts, remarks, and observations which have been given by you, or communicated by your correspondents, on subjects connected with Geology, or Geognosy, as it seems now the fashion with many to call it; and having just finished the perusal of a very useful as well as entertaining work, the first volume of M. J. A. De Luc's Geological Travels in the North of Europe, very lately published, I beg to communicate a short account of the same. The travels which are detailed in the present volume, were, it seems, undertaken, for collecting an extended body of facts in refutation of certain tenets of the late Dr. Hutton, and other geological writers, and in confirmation of the doctrines advanced by the author in his "Elementary Treatise on Geology," lately published. The route of our author commenced on the 23d of July 1804, at Berlin; he proceeded by way of Zehdenick, Furstenberg, Strelitz, Malchin, Lague, Rostock, Wismar, Travemünde, Lubeck, Eutin, Kiel, and Sleswigh, to Husum; from whence, on the 26th of August, he embarked for Harwich, to pursue a similar course of investigations in England, the details of which are to form the subject of his second volume, the publication of which will not I sincerely hope be long delayed.

The objects of M. De Luc's travels here detailed, are first stated in 27 propositions or heads: the first eleven, and the 13th, 14th, and 15th of which, relate to the question, *Whether the present rivers and streams of water, have excavated the valleys through which they flow?* a question, which some months ago exercised the pens of two of your correspondents, I recollect. M. De Luc seems to consider it as fully proved, by the numerous facts which he adduces in the volume before me, that rivers and brooks have no tendency to deepen their channels, but, on the contrary, are in every instance, with greater or less rapidity, filling up the bottoms of the valleys with the matters which they sweep away from the feet of the falling cliffs and steep banks that they undermine, in the more rapid parts of their courses: and that the whole of the matters so removed by the currents of rivers, are dropped before they reach the depths of the ocean, and are even met by and mingled with large quantities of pure sand, thrown up by the tides, from such deep parts of the ocean. The growth and operation of *peat*, in filling up some vales, is exemplified by instances at pages 131, 137, 146, 247, 334, 344, &c.; and in lessening, if not at length entirely filling up and obliterating, lakes, and natural pools of water in other instances, at pages 138, 141, 144, 145, 146, 147, 171, 185, 187, 190, 191, 233, 275, 281, 344, 347, &c.

The 12th, 16th, and 17th heads, relate to the question *Whether the bolders or blocks of granite and other stones, have emigrated on the surface of the earth?* The instances are very numerous which M. De Luc describes, through his whole route from Berlin to Husum, of blocks of granite and other primordial stones some of stupendous size, and of various species, found on the tops of gravelly eminences, in the face and at the feet of gravelly cliffs by the sea side, and on some plains, at pages 123, 127. 129, 135, 137, 156, 160, 172, 173, 178, 180, 181, 194, 195, 205, 213, 220, 223, 224, 226, 228, 234, 237, 238, 248, 251, 249, 255, 256, 257, 265, 272, 273, 274, 277, 278, 279, 285, 291, 293, 301, 302, 304, 313, 323, 329, 333, 334, 344, 367, 373, 381, 384, 386, 387, 388, &c. These instances are considered by our author as concurring in proving, the impossibility of such blocks being transported along the present or any former *surface* of the earth, to the places where they are now found, and as establishing or generalizing the supposition made by Mr. Wraxall and M. De Saussure in particular instances elsewhere, that such blocks "*seemed as if they had fallen from the sky.*"

I confess, however, sir, that I was somewhat disappointed in finding no attempt in all these details, at pointing out the exact places of the crater-like orifices, whence these primordial stones and gravel were projected *from beneath*, by the explosions of gases and torrents of water, that the descending masses of strata forced from the caverns previously existing under them, at the time that the dislocations of the strata and formation of mountains, hills and valleys, by the subsidences and angular motions of the strata, took place, according to the theory of M. De Luc: who, at page 64 of the present volume, speaks of "certain circular ridges of hills, which if seen from a distance by those unacquainted with their nature, might be taken for the bases or circumferences of large volcanic cones, which had fallen in;" as existing along the course of the Rhine, covered by such primordial blocks. I naturally expected, I say, that many similar spots would have been pointed out, in the vast tract of gravel and bolders in the north of Europe, which he has so well described in this volume.

At pages 123, 127, 129, 277, &c. my author speaks of flints, which had belonged to the chalk strata, as constituting part of the gravel, and considers such, as the remains of *dissolved strata of chalk*; but wherever he speaks of the undisturbed strata, below the gravel and bolders, in his route on the south side of the Baltic, they are uniformly said to be of sand, clay, and marle, and seem to me to answer either to the upper part of the Paris strata, described in your 35th volume, p. 56, &c. or to strata covering them, and answering to some that cover the chalk in England, and in the Netherlands also, according to the opinion of your correspondent, Mr. Farey, p. 131 of your same volume.

And here it may be proper to remark, that M. De Luc, in the volume before me, p. 248 and 250, speaks, on the authority of M. Von Willich, of similar strata of sand and clay in the island of Rugen in the Baltic, as being, if I rightly understand him, upper strata to the cliffs of chalk, with layers of flints and marine bodies, 200 and even 360 feet high, in the peninsulas of Wittow and Jasmund, in the north part of that island: and at page 387, on the authority of M. Hartz, he mentions, cliffs or strata of chalk and flints in Hedding in the island of Zeland, and also in the promontories of Maglebye and Mandemark in the island of Moen or Mona belonging to Denmark: all of which seem, I think, to confirm Mr. Farey's opinion above referred to, viz. that the chalk strata (instead of being dissolved) still underlay all the country, across which M. De Luc travelled: the coal and sandstone strata in the island of Bornholm (p. 387), probably answering, to those

of

of Leige and other continental collieries, above the chalk, and not to any part of the British series of strata, of which the chalks seem nearly the uppermost.

The important question, as to whether the chalk underlays the south-eastern shore of the Baltic, might, I think, receive an answer, by a minute examination of the strata of the higher parts of the islands of Hiddensee and Ummantz on the west of Rugen and of Greifswaldiska Oe on the south-east, which are said (p. 249), on the authority of M. Von Willich, to consist of calcareous strata, which contain marine bodies, and are intermixed with strata of porcelain earth; since these would, seemingly, answer to the very characteristic limestones and potter's clays of the basin of Paris, page 44, &c. of your 35th volume.

If Mr. Farey be right, in asserting (p. 130 of the above volume), that three miles, (or 5280 yards) in thickness of strata are *known* in England, below the chalk strata, of which I have been speaking, without the intervention of granites of any kind, or of any of what M. De Luc calls primordial stones, in such strata (as I have understood to be the case), does it seem probable, that the numerous cavernous blasts and torrents, to which M. De Luc resorts, for projecting his primordial blocks towards the sky, whence they fell to the earth's surface, could have failed to have projected along with them, numerous and large specimens of the hard British limestones, sandstones, basalts, and others? but of which we read nothing in the volume of geological travels before us, except of a few flints.

From numerous passages in this volume, and in the Geology of M. De Luc lately published, it is plain, that at the time of writing them, this veteran geologist was unacquainted with the suggestions of Mr. Farey, in various parts of your late volumes, as to the *reversed action of gravity*, and the tidal currents, occasioned by a former and large satellite of this planet, whereby the extensive and vast *abruptions*, as Dr. William Richardson calls them* (or the denudations as Mr. F. calls them), which the earth's surface in so many places has sustained, are supposed to have been occasioned, as well as the transportation of its alluvia, and the excavation of its numerous valleys also, if I mistake not the drift of Mr. Farey's arguments with Mr. Carr, in your 33d and 34th volumes.

I very much wish, sir, that I could call the attention of

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* See page 114 and page 258 of our 33d volume.—Editor.

M. De Luc to the above subjects, and that we might hear, what his long experience in studying nature, and in the consideration and discussion of geological systems, would offer, on this new hypothesis, for explaining the disruptions of the strata, the transportation of alluvial matters, and others of the vast and most mysterious operations, to which the terraqueous globe has been subjected.

But it is perhaps high time that I should mention the remaining subjects of the volume, which I undertook to explain, and as soon as possible conclude this desultory letter.

The 18th, 19th, 20th, 21st, and 22d propositions, or heads, are employed on the question, *Whether the gulfs and steep cliffs on the coasts of the ocean, were occasioned by its action?* At page 333 it is said; "that the indentations of the coasts have not been formed by the sea, but are simply the extremities of *vales*, or other original inflections of the surface, which lay below the level of the sea, when it came first to occupy its present bed;" and in numerous parts of the volume before me, facts are adduced, to show that, so far from the sea being now capable of excavating gulfs, such are almost uniformly in the process of *filling up*.

I am yet by no means satisfied, with the evidence adduced, to decide the other part of this proposition as M. De Luc has done, viz. that the *cliffs* on the sea-shore did not originate with the action of the sea. A necessary consequence of numerous valleys having opened into the sea *below its present level*, as stated above by M. De Luc, is, that separating ridges or points of hills equally numerous, projected or run out into the sea; and from having often and attentively observed, the great effect which waves propelled by a high wind, oblique to the shore in particular, have upon all projecting points, and the powerful tendency which the beach or strand *has to assume a regular line* without sudden indentations, in almost all situations, I am inclined to ascribe to the *waves* a power, of commencing and carrying on the ravages which most points of hills projecting into the sea have suffered: yet, without at all invalidating or calling in question the Mosaic chronology, which M. De Luc is so properly intent on supporting; and I am further disposed to assume, that so correct an observer and reasoner as M. De Luc, could not have overlooked these circumstances, had he not too much relied on his position, of the original marine cliffs or façades, being the mere effect of the subsidences and angular motions of the strata: and not duly considering, that the *valleys* being of the same or usual form, which descend into and much *below the level of the sea*, the separating *hills* must have been of such usual form also; and since, on following any ridge of hill on land, we scarcely ever meet with a façade or perpendicular rise in or across its height, it follows, that the numerous and almost invariably abrupt endings of hills at the sea, is the effect of their abrasion, or being worn away by the waves, and not of faults or depressions.

The 27th head discusses the question, *Whether the level of the present sea has ever changed?* Here I cannot but admire the address, v
h M. De Luc has brought
the numerous and, ind
iable instances of the hori-
zontality of the new
modern alluvial lands, to
bear on the question, a
as several others of the facts
which he relates also c
the sea has remained at its
present level, or very n
er since the present race of
men, animals, or plan
xisted upon the earth; and
I may add perhaps, ev
e present race, of fish also,
have existed in its wat

The remaining four
z. the 23d, 24th, 25th, and
26th, relate to the que
at is the age of our present
continents? From a
facts, too numerous for me
here to particularize, a
through the whole book,
my author endeavours t
and successfully, as I think,
and have hinted alrea
natural appearances and the
state of society concur
g that, our continents can-
not at the most, be mor
than the Scripture chronology represents them to be. I wait with some impatience for the appearance of the 2d volume of these Travels, which is to treat of scenes somewhat more familiar to me, and am, sir,

Yours, &c.

July 1, 1810.

A. B.

II. *Observations on the Effects of Magnesia, in preventing an increased Formation of Uric Acid; with some Remarks on the Composition of the Urine. Communicated by Mr.* William T. Brande, *F.R.S. to the Society for the Improvement of Animal Chemistry, and by them to the Royal Society* *.

Mr. Home's inquiries into the functions of the stomach, and his discovery of liquids passing from the cardiac portion, into the circulation of the blood †, led him to consider,

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* From the Philosophical Transactions for 1810, Part I.

† Phil. Trans. 1808.

sider, that the generality of calculous complaints might possibly be prevented, by introducing into the stomach, such substances as are capable of preventing the formation of uric acid, and that this mode of treatment would have many advantages over the usual method, which consists in attempting to dissolve the uric acid after it is formed.

He consulted Mr. Hatchett on the substance most likely to produce this effect, and asked if magnesia, from its insolubility in water, was not well adapted for the purpose, as it would remain in the stomach, until it should combine with any acid, or be carried along with the food towards the pylorus.

Mr. Hatchett knew of nothing more likely to produce the desired effect; and on putting this theory to the test of experiment, it was found by a very careful examination of the urine, that in several instances where there was an increased formation of uric acid, magnesia diminished it in a much greater degree than had been effected by the use, and that a very liberal one, of the alkalies in the same patient.

This circumstance led Mr. Home to wish for a more complete investigation of the subject, and he requested me to assist him in the prosecution of it. Since that time many opportunities have occurred of carrying on the inquiry during an attendance on patients labouring under calculous complaints.

It is proposed to lay the results of our joint labours before this society, with a view to establish a fact of so much importance in the treatment of those diseases.

The four following cases include the principal varieties of the disorder, which have been met with, and are therefore selected from among many others, to prevent unnecessary repetitions. In each of them the urine was occasionally carefully analysed.

CASE I.

A gentleman, sixty years of age, who had been in the habit of indulging in the free use of acid liquors, had repeatedly passed small calculi composed entirely of uric acid; his urine immediately after being voided, deposited at all times a considerable quantity of that substance, in the form of a red powder, and occasionally in large crystals.

Nine drachms of subcarbonate of soda, dissolved in water highly impregnated with carbonic acid, and taken in the course of the day at three doses, appeared to have no effect whatever on the formation of uric acid; the red sand was deposited as usual, and the small calculi continued to form.

On account of the inefficacy of this medicine, he was advised to try the vegetable alkali, and three drachms of subcarbonate of potash, dissolved in water slightly impregnated with carbonic acid, were taken at similar intervals.

The deposit of uric acid in the urine was now somewhat diminished; but during this free use of alkalies, which, with little interruption, was persevered in for more than a year, the small calculi still continued to be voided.

The very unusual di to form uric acid, and the complete failure of th n alkaline medicines, rendered this case particu rable for the trial of magnesia, as it would affo portunity of comparing its effects with those of th .

Previous to giving nesia, the urine was examined, to ascertain th of uric acid it contained: this being done, the as directed to take fifteen grains of magnesia th a day, in an ounce and a half of infusion of g a week the uric acid was found, by examining , to have diminished in quantity, and after th ee weeks it was only occasionally met with.

The use of magnesi n persevered in for eight months, during which calculi have been voided, nor has there been any deposit in the urine.

This patient was extr bject to heartburn, and he likewise complained of a sense of weight and uneasiness about the region of the stomach, both of which symptoms have disappeared.

CASE II.

A gentleman, about 40 years of age, had during four years occasionally voided considerable quantities of uric acid, in the form of red sand, and had once passed a small calculus.

His urine was generally more or less turbid, and after taking any thing which disagreed with his stomach, even in a slight degree, the red sand often made its appearance. He had never used the alkalies nor any other medicine, to alleviate his disorder: he was consequently desired to take a drachm and a half of subcarbonate of soda, dissolved in a pint and a half of water highly impregnated with carbonic acid, in the course of the day, and to persevere in this treatment for some time.

On the 30th of January 1809, he left London, and returned on the 6th of March following.

During his absence he had voided rather less uric acid than usual, but had had one severe attack, in consequence of which, twenty drops of the solution of pure potash were added to each dose of the soda water: this, however, had not the desired effect; for on the 10th of March, having taken more wine than usual on the preceding day, he was attacked with pain in the right kidney, and voided with his urine a considerable quantity of uric acid, in the form of minute red crystals. During the succeeding day, he made but little water, which deposited a copious sediment of red sand.

For the removal of this symptom, he was directed to take magnesia, in the dose of twenty grains every night and morning, in a little water; for three successive days his bowels were unusually relaxed, but afterwards became regular. He persevered in its use for six weeks without intermission: his urine was several times examined during that period, and contained no superabundant uric acid; and he has not had the slightest return of his complaint, although he has put himself under no unusual restraint in his mode of living.

Case III.

About the middle of October 1808, a gentleman, forty-three years of age, after taking violent horse exercise, was seized with pain in the right kidney and ureter. In the course of the night he passed a small uric calculus. For some months previous to this attack, he had felt occasional pain in the kidney, but had never voided either calculi or sand. His urine was now always turbid, and occasionally deposited red sand.

On the 28th of October he began the use of soda water, and for a time his urine was much improved in appearance, but the uric acid gradually returned, and at the end of December, notwithstanding the continued use of the soda water, he voided more sand, and his urine was more loaded with mucus than it had ever been before.

In consequence of these symptoms, on the 3d of January 1809, he was directed to take twenty grains of magnesia every night.

The urine was examined after the third dose, and the deposit of red sand was diminished in quantity, but it did not disappear entirely, after the magnesia had been taken for three weeks.

About this time (on the 26th of January) he caught cold, and his urine was again very turbid, but this was found to be wholly the effect of mucus, and the symptom soon left him.

On the 30th of January he took twenty grains of magnesia, and repeated it every night and morning, until the 1st of March, when his urine was perfectly healthy, and he left it off.

On the 1st of June he again voided a little uric acid, in the form of red crystalline sand: this attack was attended with a slight pain along the right ureter. He returned to the use of the magne he took twice a day for three weeks, in the sa before, and from that time to the middle of Nov re had been no symptoms of a return of the co

V.

A gentleman, aged after recovering from a severe fit of the gout, v nstantly a large quantity of mucus in his urine, a which he had never before noticed. There was ionally, abundance of red sand, consisting princi ric acid, but he had never voided a calculus.

His stomach was un y weak, he was often affected with heartburn, most constant pain in the neighbourhood of the ney. He had been in the habit of taking tincture , and other spirituous medicines, from a belief pain in his right side arose from gout in the stomach.

He had already attempted to use the alkalies, which had produced such unpleasant sensations in the stomach, that he could not be prevailed upon to try them again in any form.

Under these circumstances, he readily acceded to a new plan of treatment. He was directed to omit the use of spirituous medicines, and take twenty grains of magnesia three times a day in water; but this operating too powerfully upon the bowels, the same quantity of magnesia was taken twice a day only, with an addition of five drops of laudanum to each dose.

This plan was pursued without intermission for three weeks, and he received considerable benefit, as far as concerned the state of the stomach, and pain in the region of the kidney. The urine, which was examined once a week, was also, on the whole, improved; but it occasionally deposited a very copious sediment, consisting of uric acid, with a variable proportion of mucous secretion.

After a further continuance of the use of the magnesia for three weeks, the urine was often much loaded with uric acid and mucus; but these appearances, which before the use of the magnesia were constant, are now only occasional, so that the disposition to form a redundant quantity of uric acid is much diminished: it is also deserving of remark, that there has not been the slightest symptom of gout from the time of the last attack, which is more than a year back, a longer interval of ease than this patient has experienced for the last six years.

He has now omitted the regular use of the magnesia; but on perceiving any unpleasant sensation in the stomach, he returns to it for a week or ten days, and then again leaves it off.

From the preceding cases it appears, that the effects of magnesia taken into the stomach, are in many respects different from those produced by the alkalies, in those patients in whom there is a disposition to form a superabundant quantity of uric acid.

With a view to ascertain their comparative effects on healthy urine, when taken under the same circumstances, the following experiments were made.

Experiment 1. *On Soda.*

Two drachms of subcarbonate of soda were taken on an empty stomach at nine o'clock in the morning, dissolved in three ounces of water, and immediately afterwards, a large cup of warm tea.

In six minutes, about one ounce of urine was voided; in twenty minutes six ounces more; and after two hours, a similar quantity.

The first portion became very turbid, within ten minutes after it had been voided, and deposited a copious sediment of the phosphates, in consequence of the action of the alkali upon the urine. It slightly restored the blue colour to litmus paper reddened with vinegar: the alkali, therefore, was not merely in sufficient quantity to saturate the uncombined acid in the urine, and consequently to throw down the phosphates; but it was in excess, and the urine was voided alkaline.

The urine voided after twenty minutes, also deposited a cloud of the phosphates; but the transparency of that voided two hours after the alkali had been taken, was not disturbed.

Here, therefore, the effect of the alkali upon the urine, was at its maximum, probably in less than a quarter of an hour after it had been taken into the stomach, and in less than two hours the whole of the alkali had passed off.

Experiment 2. On Soda, with excess of Carbonic Acid.

The same quantity of soda, dissolved in eight ounces of water very highly impregnated with carbonic acid, was taken under the same circumstances as in the former experiment, and the urine was voided at nearly similar intervals.

The separation of the phosphates was less distinct, and less rapid. In two hours after the urine had been voided,
there was a small dep posed principally of phos-
phate of lime; there distinct pellicle on the sur-
face, consisting of t phosphate of ammonia and
magnesia. This app roduced by the escape of
the carbonic acid, wh fore retained the ammonia-
co-magnesian phosph ution, and which now oc-
casions its deposition urface, is by no means un-
common, even in the ealthy persons: in the pre-
sent instance, it appe e, that carbonic acid passes
off from the stomach, dneys; for, after taking the
alkalies, in water very pregnated with it, the pel-
licle is uniformly proc d is also much more abun-
dant and distinct than other circumstances.

In similar experime potash, the results were in
all cases as similar e expected in researches of
this nature.

Experimen n Magnesia.

Magnesia was taken under circumstances similar to those of the soda in the former experiment: in the quantity of half a drachm, it produced no sensible effect upon the urine during the whole day. When taken in the dose of a drachm at nine o'clock in the morning, the urine voided at twelve o'clock became slightly turbid: at three o'clock the effect of the magnesia was at its maximum, and a distinct separation of the phosphates took place, partly in the form of a film, which when examined was found to be the triple phosphate of ammonia and magnesia, and partly in the state of a white powder, consisting almost entirely of the triple phosphate and phosphate of lime.

The effect of large doses of magnesia, in producing a white sediment in the urine, is very commonly known, and has been erroneously attributed to the magnesia passing off by the kidneys.

These experiments show that magnesia, even in very large doses, neither produces so rapid an effect upon the urine, nor so copious a separation of the phosphates, as the alkalies; on this its value as a remedy in calculous disorders seems materially to depend.

Experiment 4. *On Lime.*

Two ounces of lime water, taken in the morning upon an empty stomach, with a cup of milk and water, produced no effect whatever.

A pint of lime water, taken at four intervals of an hour each, produced a slight deposition of the phosphates at the end of the fifth hour. The urine voided at the third hour was not at all affected; at the fifth hour, the effect appeared at its height, but was not nearly so distinct as from small doses of soda, notwithstanding the insoluble compounds which lime might be expected to form with the acids in the urine.

The unpleasant taste of lime water, the quantity in which it requires to be taken, on account of the small proportion of the earth which is held in solution, and the uncertainty of its effect, are circumstances which render it of little use, excepting in some very rare cases, where it has been found to agree particularly well with the stomach.

The effect of carbonate of lime upon the urine was much less distinct than that of lime water: at times it produced no effect, but when taken in very large doses, a slight deposition of the phosphates was produced.

These experiments were repeated upon three different individuals, and there was always an uniformity in the results.

When the medicines were taken some hours after food being received into the stomach, their effects upon the urine were retarded, but not prevented.

The effects of many other substances upon the urine were examined into during this investigation; but they varied so much according to circumstances, that no satisfactory results were produced.

As it is found in the foregoing experiments, that the effects of soda on the urine are modified by the presence of carbonic acid, the following experiment was made, to ascertain whether any sensible effects are produced by that acid on healthy urine.

Twelve ounces of water very highly impregnated with carbonic acid, were taken upon an empty stomach at nine o'clock in the morning. At ten o'clock about eight ounces of urine were voided, which had a natural appearance,

ance, but, when compared with urine voided under common circumstances, was found to contain a superabundant quantity of carbonic acid: this gas was copiously given off when the urine was gently heated, or when it was exposed under the exhausted receiver of an air-pump.

In a patient who had a calculus of large dimensions extracted from the bladder, composed entirely of the phosphates, and whose stomach did not admit of the use of stronger acids, carbonic acid was given in water; it was found peculiarly grateful to the stomach, and upon examining the urine during its use, the phosphates were only voided in solution; but when at any time it was left off, they were voided in white sand.

III. Remarks on Mr. Ric alker's proposed Alterations in the Scale mometers, in our last Number.

To

Sir, The reading of ker's paper on thermometers in your last num me to trouble you, for the purpose of pointing at gentleman, what I conceive to have been why various improvements and suggested re he weights, measures, and modes of estimati s in this country and others, have been neglec of them forgotten, viz. their authors' having neg gn *new and appropriate names and characters* to the new denominations or things, which it was their object to introduce; but transferring the *old names*, as foot, inch, ounce, pound, degree, &c. &c. to things almost as new and dissimilar, as these are from each other.

If the most precise and short compound words were fixed on, indicative of degrees of heat and degrees of cold, derived perhaps from the Greek or Latin, as being dead or standard languages universally understood, to be used as new prefixes or additions (with distinctive *characters*[*] which could be used as abbreviations of these) to the *number* of thermometric divisions proposed, instead of using either the word *degrees* or the character ° in present use; I do not despair of seeing Mr. W.'s scale or even scales (if each have *their own names and characters*) adopted by many, since

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[*] Perhaps the initial Greek letters of the names might answer these purposes.

negative signs would thus become unnecessary, and no confusion could arise from their use; while on the other hand, every true friend of science and accuracy, will naturally adhere to the divisions *in use*, as answering their intended ends, although not in the best or most perfect manner; and set their face against imperfectly contrived changes.

It would have been desirable, in the last note at bottom of page 420, that Mr. R. Walker had pointed out a material circumstance affecting the uses of mercurial and alcoholial thermometers, viz. the very different periods of time necessary, for each to act in, or acquire and indicate the temperature of any medium under experiment.—See the Monthly Magazine, vol. xvii. p. 213.

I am, sir,
Yours, &c.
July 3, 1810. LONDINENSIS.

IV. *The Bakerian Lecture for* 1809. *On some new Electrochemical Researches on various Objects, particularly the metallic Bodies, from the Alkalies, and Earths, and on some Combinations of Hydrogen.* By HUMPHRY DAVY, *Esq. Sec. R.S. F.R.S.E. M.R.I.A.*[*]

[Continued from vol. xxxv. p. 415.]

III. *Experiments on Nitrogen, Ammonia, and the Amalgam from Ammonia.*

ONE of the queries that I advanced, in attempting to reason upon the singular phænomena produced by the action of potassium upon ammonia, was, that nitrogen might possibly consist of oxygen and hydrogen, or that it might be composed from water.

I shall have to detail in this section a great number of laborious experiments, and minute and tedious processes, made with the hopes of solving this problem. My results have been for the most part negative; but I shall venture to state them fully, because I hope they will tend to elucidate some points of discussion, and may prevent other chemists from pursuing the same paths of inquiry, and which at first view do not appear unpromising.

The formation of nitrogen has been often asserted to take place in many processes, in which none of its known combinations were concerned. It is not necessary to enter into the discussion of the ideas entertained by the German chemists, on the origin of nitrogen, produced during the passage of water through red-hot tubes, or the speculations of Girtanner, founded on these and other erroneous data: the early discovery of Priestley on the passage of gases through red-hot tubes of earthen-ware, the accurate researches of Berthollet, and the experiments of Bouillon La Grange, have afforded a complete solution of this problem.

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[*] From Philosophical Transactions for 1810, Part I.

One of the most striking cases, in which nitrogen has been supposed to appear without the presence of any other matter but water, which can be conceived to supply its elements, is in the decomposition and recomposition of water by electricity*. To ascertain if nitrogen could be generated in this man ad an apparatus made, by which a quantity of wa be acted upon by Voltaic electricity, so as to pı xygen and hydrogen with great rapidity, and in wl e gases could be detonated, without the exposure oı ter to the atmosphere; so that this fluid was in vith platina, mercury, and glass only; and the w ompleting the Voltaic and common electrical circ hermetically inserted into the tube. 500 double the Voltaic combination were used, in such acti about the eighth of a cubical inch of the mixe pon an average, was produced from 20 to 30 ti ery day. The water used in this experiment was ıalf a cubic inch; it had been carefully purged he air-pump and by boiling, and had been intr ıto the tube, and secured from the influence of t phere whilst warm. After the first detonation of the oxygen and hydrogen, which together equalled about the eighth of a cubical inch, there was a residuum of about $\frac{1}{40}$ of the volume of the gases; after every detonation this residuum was found to increase, and when about 50 detonations had been made, it equalled rather more than $\frac{1}{4}$ of the volume of the water, *i.e.* $\frac{1}{8}$ of a cubical inch. It was examined by the test of nitrous gas; it contained no oxygen; six measures mixed with three measures of oxygen diminished to five; so that it consisted of 2·6 of hydrogen, and 3·4 of a gas having the characters of nitrogen.

This experiment seemed in favour of the idea of the production of nitrogen from pure water in these electrical processes; but though the platina wires were hermetically sealed into the tube, it occurred to me as possible that at the moment of the explosion by the electrical discharge, the sudden expansions and contractions might occasion some momentary communication with the external air through the aperture; and I resolved to make the experiments in a method by which the atmosphere was entirely excluded. This was easily done by plunging the whole of the apparatus, except the upper parts of the communicating wires, under oil, and carrying on the process as before. In this experiment the residuum did not seem to increase quite so fast as in the former one. It was carried on for nearly two months. After 340 explosions, the permanent gas equalled $\frac{24}{100}$ of a cubical inch. It was carefully examined: six measures of it, detonated with three measures of oxygen, diminished to rather less than one measure;—a result which seems to show, that nitrogen is not formed during the electrical decomposition and recomposition of water, and that the residual gas is hydrogen. That the hydrogen is in excess, may be easily referred to a slight oxidation of the platina.

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* See Dr. Pearson's elaborate experiments, on the decomposition of water by electrical explosions. Nicholson's Journal, 4to, vol. i. page 301.

The refined experiments of Mr. Cavendish on the deflagration of mixtures of oxygen, hydrogen, and nitrogen, lead directly to the conclusion, that the nitrous acid sometimes generated in experiments on the production of water, owes its origin to nitrogen, mixed with the oxygen and hydrogen, and is never produced from those two gases alone. In the Bakerian lecture for 1806, I have stated several facts, which seem to show that the nitrous acid, which appears in many processes of the Voltaic electrization of water, cannot be formed, unless *nitrogen* be present.

Though in these experiments I endeavoured to guard with great care against all causes of mistake, and though I do not well see how I could fall into an error, yet I find that the assertion, that both acids and alkalies may be produced from pure water, has again been repeated*. The energy with which the large Voltaic apparatus, recently constructed in the Royal Institution, acts upon water, enabled me to put this question to a more decided test than was before in my power. I had formerly found in an experiment, in which pure water was electrified in two gold cones in hydrogen gas, that no nitrous acid nor alkali was formed. It might be said, that in this case the presence of hydrogen dissolved in the water, would prevent nitrous acid from appearing; I therefore made two series of experiments, one in a jar filled with oxygen gas, and the other in an apparatus in which glass, water, mercury, and wires of platina only, were present.

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* Nicholson's Journal, August 1809, p. 258.

In the first series 1000 double plates were used, the two cones were of platina, and contained about $\frac{1}{12}$ of a cubical inch each, and filaments of asbestus were employed, to connect them together. In these trials, when the batteries were in full action, the heat was so great, and the gases were disengaged with so much rapidity, that more than half the water was lost in the course of a few minutes. By using a weaker charge, the process was carried on for some hours, and in some cases for from two to three days. In no instance, in which slowly distilled water was employed, and in which the receiver w ith pure oxygen from oxymuriate of potash, w d or alkali exhibited in the cones; even when ni present, the indications of the production of ac aline matter were very feeble; though, if the as ouched with unwashed hands, or the smallest eutro-saline matter introduced, there was an separation of acid and alkali, at the points of the asbestus with the platina, which could be it by the usual tests.

In the second series ts, the oxygen and hydrogen produced from t re collected under mercury, and the two portio communicated directly with each other. In sev nade in this way, with a combination of 500 p ntinued for some days, it was always found tha separated in the glass negatively electrified; a quantity of acid, which could barely be made evident by litmus, in the glass positively electrified. This acid rendered cloudy nitrate of silver. Whether its presence was owing to impurities which might rise in distillation with the mercury, or to muriatic acid existing in the glass, I cannot say; but as common salt perfectly dry is not decomposed by silex, it seems very likely that muriatic acid in its arid state may exist in combination in glass.

I tried several experiments on the ignition and fusion of platina by Voltaic electricity, in mixtures of the vapour of water and oxygen gas. I thought it possible, if water could be combined with *more oxygen*, that this heat, the most intense we are acquainted with, might produce the effect. When the oxygen was mixed with nitrogen, nitrous acid was formed; but when it consisted of the last portions from oxymuriate of potash, there was not the slightest indication of such a result.

Water in vapour was passed through oxide of manganese,

ganese, made red hot in a glazed porcelain tube, the bore of which was nearly an inch in diameter; in this case a solution of nitrous acid, sufficiently strong to be disagreeably sour to the taste, and which readily dissolved copper, was formed.

This experiment was repeated several times, and, when the diameter of the tube was large, with precisely the same results. When red oxide of lead was used instead of oxide of manganese, no acid was however generated; but upon this substance a single trial only was made, and that in a small tube, so that no conclusion can with propriety be drawn from this failure.

I stated in the last Bakerian lecture, that in attempting to produce ammonia from a mixture of charcoal and pearl-ash, that had been ignited by the action of water, in the manner stated by Dr. Woodhouse, I failed in the trial in which the mixture was cooled in contact with hydrogen. I have since made a number of similar experiments. In general, when the mixture had not been exposed to air, there was little or no indication of the production of the volatile alkali; but the result was not so constant as to be entirely satisfactory; and the same circumstances could not be uniformly obtained in this simple form of the experiment. I had an apparatus made, in which the phænomena of the process could be more rigorously examined. Pure potash and charcoal, in the proportion of one to four in weight, were ignited in the middle of a tube of iron, furnished with a system of stopcocks, and connected with a pneumatic apparatus, in such a manner that the mixture could be cooled in contact with the gas produced during the operation; and that water exhausted of air could be made to act upon the cooled mixture, and afterwards distilled from it: figures of this apparatus, and an account of the manner in which it was used, are annexed to this paper. In this place I shall state merely the general results of the operations, which were carried on for nearly two months, a variety of precautions being used to prevent the interference of nitrogen from the atmosphere.

In all cases in which the water was brought in contact with the mixture of charcoal and potash, when it was perfectly cool, and afterwards distilled from it by a low heat, it was found to hold in solution small quantities of ammonia; when the operation was repeated upon the same mixture, ignited a second time, the proportion diminished; in a third operation it was sensible, but in the fourth barely perceptible. The same mixture, however, by the addition of of a new quantity of potash, again gained the power of producing ammonia in two or three successive operations; and when any mixture had ceased to give ammonia, the power was not restored by cooling it in contact with air.

Ammonia was produced in a case in which more than 200 cubical inches of gas had passed over from the action of water upon a mixture, and when the last portions only were preserved in contact with it during the cooling. In a comparative trial it was however found, that considerably more ammonia was produced, when a mixture was cooled in contact with the atmosphere, than when it was cooled in contact with the gas developed in the operation.

I shall not attempt to draw any conclusions from these processes. It would appear from some experiments of M. Berthollet, that nitrogen adheres very strongly to charcoal*. The circumstances that the ammonia ceases to be produced after a certain number of operations, and that the quantity is much greater when free nitrogen is present, are perhaps against the idea that nitrogen is composed in the process. But till the weights of the substances concerned and produced in these operations are compared, no correct decision on the question can be made.

The experiments of Dr. Priestley upon the production of nitrogen, during the freezing of water, induced that philosopher to conceive, either that water was capable of being converted into nitrogen, or that it contained much more nitrogen than is usually suspected.

I have made some repetitions of his processes. A quantity of water, (about a cubical inch and a quarter,) that had been produced from snow, boiled and inverted over mercury whilst hot, was converted into ice, and thawed in 16 successive operations; gas was produced, but after the first three or four times of freezing there was no notable increase of the volume. At the end of the experiment, about $\frac{1}{30}$ of a cubical inch was obtained, which proved to be common air.

About four cubical inches of water from melted snow were converted into ice and thawed, four successive times, in a conical vessel of wrought iron. At the end of the fourth process, the volume of gas equalled about $\frac{1}{10}$ of the volume of the water. It proved to contain about $\frac{1}{10}$ oxygen, $\frac{3}{10}$ hydrogen, and $\frac{6}{10}$ nitrogen.

Mr. Kirwan observed the fact, that when nitrous gas and sulphuretted hydrogen are kept in contact for some time, there is a great diminution of volume, and that the nitrous gas becomes converted into nitrous oxide, and that sulphur is deposited which has an ammoniacal smell. I repeated this experiment several times in 1800 with similar results, and I found, that the diminution of the volume of the gases when they were mixed in equal proportions, was to rather less than $\frac{1}{4}$, which seemed to be nitrous oxide.

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* *Mem. d'Arcueil*, tom. ii. page 485.

In reasoning upon this phænomenon, I saw grounds for a minute investigation of it. Sulphuretted hydrogen, as appears from experiments which I have stated on a former occasion, and from some that I shall detail towards the conclusion of this lecture, contains a volume of hydrogen equal to its own. But one of hydrogen demands half its volume of oxygen to convert it into water, and nitrous gas consists of about half a part in volume of oxygen; so that, supposing the whole of the hydrogen employed in absorbing oxygen from nitrous gas, nitrogen alone ought to be formed, and not nitrous oxide. Or, if the whole of the gas is nitrous oxide, this should contain all the nitrogen of the nitrous gas, leaving none to be supplied to the ammonia. I mixed together five cubical inches of nitrous gas, and five of sulphuretted hydrogen over mercury, the barometer being at $29\cdot5^{\text{in.}}$, thermometer at 51° Fahrenheit; twelve hours had elapsed before any change was perceived; there was then a whitish precipitate formed, and a deep yellow liquid began to appear in drops, on the inside of the jar, and the volume of the gases quickly diminished; after two days the diminution ceased, and the volume became stationary; the barometer was at $30\cdot45^{\text{in.}}$, and thermometer 52° Fahrenheit; when it equalled 2·3. The gas proved to be about $\frac{3}{4}$ nitrous oxide, and the remaining fourth was inflammable. An experiment was made expressly to determine the nature of the deep yellow liquid in the jar. It proved to be of the same kind as Boyle's fuming liquor, the hydrosulphuret of ammonia, but with sulphur in great excess.

In this experiment there was evidently no formation of nitrogen, and these complicated changes ended in the production of two new compounds; nitrogen, hydrogen; oxygen and sulphur combining to form one; and a part of the nitrogen and oxygen, becoming more condensed, to form another.

Having stated the results of the investigation on the production of nitrous acid and of ammonia, in various processes of chemistry, I shall notice some attempts that I made to decompound nitrogen, by agents which I con-

ceived might act at the same time on oxyge
basis of nitrogen. Potassium, as I have b
sublimes in nitrogen, without altering it, or
changed: but I thought it possible, that the c
different, if this powerful agent were made to
trogen, assisted by the intense heat and decon
gy of Voltaic electricity.

I had an apparatus made, by which the V
could be completed in nitrogen gas, confined
by means of potassium and platina. The pota
quantity of about two or three grains, was pl
of platina, and by contact with a wire of pla
be fused and sublimed in the gas. The quan
gen was usually about a cubical inch. Th
ployed was always in full action for these exp
consisted of one thousand double plates. The
were very brilliant: as soon as the contact wi
sium was made there was always a bright lig
as to be painful to the eye; the platina becan
the potassium rose in vapour; and by incre
stance of the cup from the wire, the elec
through the vapour of the potassium, prod
brilliant flame, of from half an inch to a
quarter in length; and the vapour seemed to
the platina, which was thrown off in small
state of fusion, producing an appearance si
produced by the combustion of iron in oxygen

In all trials of this kind, hydrogen was p
in some of them there was a loss of nitrogen.
seemed to lead to the inference that nitrog
pounded in the process; but I found that, i
as the potassium was introduced more free fro
potash, which would furnish water and con
drogen in the experiment, so in proportion
of this gas evolved; and in a case in which
precautions were taken, the quantity did no
the volume of gas, and there was no sensib
nitrogen lost.

The largest proportion of nitrogen which d
any experiment, was $\frac{1}{11}$ of the quantity used
case the crust of potash was considerable, an
hydrogen, nearly equal to $\frac{1}{4}$ of the nitrogen,
It cannot be said that the nitrogen is *not* de
this operation; but it seems much more li
slight loss is owing to its combination with
drogen, and its being separated with the pota

form of the gray pyrophoric sublimate, which I have found is always produced when potassium is electrized and converted into vapour in ammonia.

The phosphuret of lime in its common state is a conductor of electricity; and when it was made the medium of communication between the wires of the great battery, it burnt with a most intense light. It was ignited to whiteness in nitrogen gas; a little phosphuretted hydrogen was given off from it, but the nitrogen was not altered; the apparatus was similar to that used for the potassium.

As almost all compounds known to contain hydrogen are readily decomposed by oxymuriatic acid gas, a mixture of nitrogen and oxymuriatic acid gas was passed through a porcelain tube heated to whiteness; the products were received in a pneumatic apparatus over water, there was a small loss of nitrogen; but the greatest part came over densely clouded; and as nitromuriatic acid was found dissolved in the water, no conclusions concerning the decomposition of nitrogen can be drawn from the process.

The general tenour of these inquiries cannot be considered as strengthening in any considerable degree, the suspicion which I formed of the decomposition of nitrogen, by the distillation of the olive-coloured substance from potassium and ammonia, in tubes of iron.

In reasoning closely upon the phænomena in this operation, it appears to me indeed possible to account for the loss of nitrogen, without assuming that it has been converted into new matter Though the iron tubes which I used were carefully cleaned; yet still it was not unlikely that a small quantity of oxide might adhere to the welded parts; the oxygen of which, in the beginning of the process of distillation, might form water with hydrogen, given off from the fusible substance; which being condensed in the upper part of the tube, would be again brought into action towards the close of the operation, occasioning the formation, and possibly the absorption of some ammonia, and consequently a loss of nitrogen, and the production of an increased proportion of hydrogen. I have made one experiment, with the hopes of deciding this question, in an iron tube used immediately after the whole internal surface had been cleaned by the borer; six grains of potassium were used in a tray of iron, nearly thirteen cubical inches of ammonia were absorbed, and about six of hydrogen produced. Thirteen cubical inches of gas were evolved in the first operation; which consisted of nearly one cubical inch of ammonia, four of nitrogen, and eight of hydrogen. The portion of gas given off in the second operation equalled 3·6 cubical inches; which consisted of 2·5 hydrogen, and 1·1 nitrogen. The potassium produced in the operation was sufficient to generate 3·1 cubical inches of hydrogen.

As the iron in these experiments had been heated to intense whiteness, and must have been very soft; it was not impossible, considering the recent experiments of M. Hassenfratz *, that the loss of so large a portion of potassium might depend upon an intimate union of that body with iron, and its penetration into the substance of the tube. This idea is countenanced by another experiment of the same kind, in which the heat was raised to whiteness, and the barrel cut into pieces when cool: on examining the lower part of it, I found in it a very thin film of potash; but which, I conceive, could scarcely equal a grain in weight. The pieces of the barrel were introduced under a jar inverted in water; at the end of two days nearly 2·3 cubical inches of hydrogen were found to be generated.

In the experiments detailed in page 53 of the last volume of the Transactions †, a loss of nitrogen, and a production of hydrogen, was perceived in a case in which the residuum from a portion of fusible substance, which had been exposed to a low red heat, was distilled in a tube of platina; but in this case the residuum had been covered by *naphtha*, and it is possible that ammonia might have been regenerated by hydrogen from the naphtha, and absorbed by that fluid; and a part of the hydrogen might likewise proceed from the decomposition of the naphtha: and in several experiments in which I have burnt the entire fusible substance, I have found no loss of nitrogen.

Even the considerable excess of hydrogen, and deficiency of nitrogen, in the processes in which the fusible substance is distilled with a new quantity of potassium, page 451 ‡, it is possible to refer to the larger quantity of moisture, which must be absorbed by the fusible substance from the air, during the time occupied in attaching the potassium to the tray, and likewise from the moisture adhering to the crust of potash, which always forms upon the potassium, during its exposure to air.

These objections are the strongest that occur to me, against

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[*] *Journal des Mines, Avril* 1808, p. 275.

[†] Phil. Mag. vol. xxxiii. page 8.

[‡] Ibid. vol. xxxiv. page 339.

against the mode of explaining the phænomena by supposing nitrogen decomposed in the operation; but they cannot be considered as decisive on this complicated and obscure question, and the opposite view may be easily defended.

Though I have already laid before the Society a number of experiments upon the decomposition of ammonia, yet I shall not hesitate to detail some further operations which have been conducted according to new views of the subject.

I concluded from the loss of weight taking place in the electrical analysis of ammonia, that water or oxygen was probably separated in this operation; but I was aware that objections might be made to this mode of accounting for the phænomenon.

The experiment of producing an amalgam from ammonia, which regenerated volatile alkali, apparently by oxidation, confirmed the notion of the existence of oxygen in this substance; at the same time it led to the suspicion, that of the two gases separated by electricity, one, or perhaps both, might contain metallic matter united to oxygen: and the results of the distillation of the fusible substance, from potassium and ammonia, notwithstanding the objections I have made, can perhaps be explained on such a supposition.

I have made a number of experiments upon the decomposition of considerable quantities of ammonia, both by Voltaic and common electricity; and I have used an apparatus (of which a figure is attached to this paper) in which nothing was present but the gas, the metals for conveying the electricity, and glass. The ammonia was introduced by a stopcock which was cleared of common air, into a globe that was exhausted, after being filled two or three times with ammonia: the gas that was used was absolutely pure, the decomposition was performed without any possibility of change in the volume of the elastic matter, and the apparatus was such, that the gas could be exposed to a *freezing mixture*, and the whole weighed before and after the experiment.

The object in keeping the volume the same during the decomposition, was to produce the condensation of any aqueous vapour, which if formed in small quantity in the operation, (on the theory of the mechanical diffusion of vapour in gases,) might in the common case of decomposition, under the usual pressure, be in quantity nearly twice as much in the hydrogen and nitrogen, as in the ammonia.

In all instances it was found, that there was no loss of weight of the apparatus, nor was there any deposition of

moisture, during or after the electrization;
were uniformly tarnished; and in an experi
surfaces of brass were used, a small quantit
loured matter formed on the metal; but thou
nearly eight cubical inches of ammonia were
the weight of the oxidated matter was so m
scarcely sensible. By the use of a freezi
muriate of lime and ice, which diminished th
to −15°, there was a very feeble indication
addition of hydrometrical moisture.

In these experiments the increase of the
formly (within a range of five parts) fron
and the hydrogen was to the nitrogen in the
portions of from 73·74 to 27·26; the prop
being made, and the precautions before ref
taken *.

Assuming the common estimations of the
vity of ammonia, of hydrogen, and nitrog
clusions which I have advanced in the Baker
1807 would be supported by these new exp
as the moisture and oxygen visibly separat
conceived to be as much as $\frac{1}{11}$ or $\frac{1}{12}$ of the
ammonia, I resolved to investigate, more pr
had reason to think had been hitherto done
gravities of the gases concerned in their c
the very delicate balance belonging to the Ro
placed the means of doing this in my power.

Nitrogen, hydrogen, and ammonia, were d

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[*] Philosophical Transactions 1809, page 459. M. Bert
second volume of the Memoirs of Arcueil, has given a pap
position of ammonia, and he enters into an examination
oxygen separated in the electrical decomposition of am
supposes I rate at 20 per cent. and at the same time he c
periments which he is pleased to attribute to me, of th
charcoal and iron in ammonia. His arguments and his
points appear to me perfectly conclusive; but as I neve
opinion, as that 20 of oxygen were separated in the expe
imagined such results as the combustion of iron and char
and never published any thing which could receive such
I shall not enter into any criticism on this part of his pap
ments of this ingenious chemist on the direct decomposi
seem to have been conducted with much care, except as to
of his not boiling the quicksilver; which I conceive has c
overrate the increase of volume. At all events a loss c
to be expected than an increase of weight, in all very refine
this kind. It is possible that the volume may be exactly c
the nitrogen may be to the hydrogen as one to three;
neither the experiments of Dr. Henry, nor those that I hav
[illegible] hypothetical inferences that may be
[illegible] as an absolute fact.

continued exposure to potash, and were very carefully weighed. Their relative specific gravities proved to be at 30·5$^{\text{in.}}$ barometer, 51° Fahrenheit's thermometer.

For nitrogen, the 100 cubical inches	29·8 grains.
For hydrogen, ditto	2·27
For ammonia	18·4

Now, if these data be calculated upon, it will be found, that in the decomposition of 100 of ammonia, taking even the largest proportions of gases evolved, there is a loss of $\frac{1}{18}$*; and if the smallest proportion be taken, the loss will be nearly $\frac{1}{13}$.

These results and calculations agree with those that I have before given, and with those of Dr. Henry.

The lately discovered facts in chemistry, concerning the important modifications which bodies may undergo by very slight additions or subtractions of new matter, ought to render us cautious in deciding upon the nature of the process of the electrical decomposition of ammonia.

It is *possible*, that the minute quantity of oxygen which appears to be separated is not accidental, but a result of the decomposition; and if hydrogen and nitrogen be both oxides of the same basis, the possibility of the production of different proportions of water, in different operations, might account for the variations observed in some cases in their relative proportions; but on the whole, the idea that ammonia is decomposed into hydrogen and nitrogen alone, by electricity, and that the loss of weight is no more than is to be expected in processes of so delicate a kind, is, in my opinion, the most defensible view of the subject.

But if *ammonia* be capable of decomposition into nitrogen and hydrogen, what, it will be asked, is the nature of the matter existing in the amalgam of ammonia? what is the metallic basis of the volatile alkali? These are questions intimately connected with the whole of the arrangements of chemistry; and they are questions, which, as our instruments of experiment now exist, it will not, I fear, be easy to solve.

I have stated in my former communication on the amalgam from ammonia, that, under all the common circumstances of its production, it seems to preserve a quantity of water adhering to it, which may be conceived to be sufficient to oxidate the metal, and to reproduce the ammonia.

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* 100 of ammonia at the rate of 185, will give 136·9 of hydrogen, weighing 3·1 grains, and 48·1 of nitrogen, weighing 14·33 grains; but 18·4—17·4 =1, and at the rate of 180, 133 of hydrogen weighing 3·01 and 47 of nitrogen weighing 14, and 18·4—17=1·4.

I have tried various devices with the hopes of being able to form it from ammonia in a dry state, but without success. Neither the amalgams of potassium, sodium, or barium, produce it in ammoniacal gas; and when they are heated with muriate of ammonia, unless the salt is moist, there is no metallization of the alkali.

I have acted upon ammonia by different metallic amalgams negatively electrified, such as the amalgams of gold and silver, the amalgam of zinc, and the liquid amalgam of bismuth and lead; but in all these cases the effect was less distinct than when pure mercury was used.

By exposing the mercury to a cold of −20° Fahrenheit,
in a close tube, I have l in obtaining an amalgam
in a much more solid s this decomposed nearly as
rapidly as the common a , but it gave off much more
gaseous matter; and in ance I obtained a quantity
which was nearly equal nes its volume.

The amalgam which l ason to believe can be made
most free from *adher tre*, is that of potassium,
mercury, and ammoni olid state. This, as I have
mentioned in my form nication, decomposes very
slowly, even in contact ter, and, when it has been
carefully wiped with b aper, bears a considerable
heat without alteratio e lately made several new
attempts to distil the an from it, but without suc-
cess. When it is stron in a green glass tube filled
with hydrogen gas, the ys a partial regeneration of
ammonia; but with thi ia there is from $\frac{5}{10}$ to $\frac{6}{10}$
of hydrogen produced.

As it does not seem possible to obtain an amalgam in an uniform state, as to adhering moisture, it is not easy to say what would be the exact ratio between the hydrogen and ammonia produced, if no more water was present than would be decomposed in oxidating the basis. But in the most refined experiments which I have been able to make, this ratio is that of one to two; and in no instance in which proper precautions are taken, is it less; but under common circumstances often more. If this result is taken as accurate, then it would follow, that ammonia (supposing it to be an oxide) must contain about 48 per cent. of oxygen, which, as will be hereafter seen, will agree with the relations of the attractions of this alkali for acids, to those of other salifiable bases *.

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* Even in common air, the amalgam evolves hydrogen and ammonia, nearly in these proportions, and in one experiment which I lately tried, there

If hydrogen be supposed to be a simple body, and nitrogen an oxide, then, on the hypothesis above stated, nitrogen would consist of nearly 48 of oxygen, and 34 of basis; but if the opinion be adopted, that hydrogen and nitrogen are both oxides of the same metal, then the quantity of oxygen in nitrogen must be supposed less.

These views are the most obvious that can be formed, on the antiphlogistic hypothesis, of the nature of metallic substances; but if the facts concerning ammonia were to be reasoned upon, independently of the other general phænomena of chemical science, they perhaps might be more easily explained on the notion of nitrogen being a basis, which became alkaline by combining with one portion of hydrogen, and metallic, by combining with a greater proportion.

The solution of the question concerning the quantity of matter added to the mercury in the formation of the amalgam, depends upon this discussion; for, if the phlogistic view of the subject be adopted, the amalgam must be supposed to contain nearly twice as much matter as it is conceived to contain on the hypothesis of deoxygenation. In the last Bakerian lecture, I have rated the proportion at $\frac{1}{12000}$; but this is the least quantity that can be assumed, the mercury being supposed to give off only one and a half its volume of ammonia. If the proportion stated in page 56 [page 30 preceding] be taken as the basis of calculation, which is the maximum that I have obtained, the amalgam would contain about $\frac{1}{1800}$ of new matter, on the antiphlogistic view, and about $\frac{1}{900}$ on the phlogistic view.

I shall have occasion to recur to, and to discuss more fully, these ideas, and I shall conclude this section by stating, that though the researches on the decomposition and composition of nitrogen, which have occupied so large a space in the foregoing pages, have been negative, as to the primary object, yet they may not possibly be devoid of useful applications. It does not seem improbable, that the passage of steam over hot manganese may be applied

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there seemed to be no absorption of oxygen from the atmosphere. This circumstance appears to me in favour of the antiphlogistic view of the metallization of the volatile alkali; for if the hydrogen be supposed to be given off from the mercury, and not to arise from the decomposition of water adhering to the amalgam, it might be conceived, that being in the nascent state, it would rapidly absorb oxygen. In my first experiments upon the amalgam, finding that common air, to which it had been exposed, gave less diminution with nitrous gas than before, I concluded naturally, that oxygen had been absorbed; but this difference might have arisen, partly at least, from the mixture of hydrogen. Whether in any case the amalgam absorbs oxygen gas, is a question for further investigation.

to the manufacture of nitrous acid. And there is reason to believe that the ignition of charcoal and potash, and their exposure to water, may be advantageously applied to the production of volatile alkali, in countries where fuel is cheap.

[To be continued.]

V. *Description and Analysis of the Meteoric Stone which fell at Weston, in North America, the 4th December* 1807. *By* DAVID BAILIE WARDEN, *Esq. Consul-general of the United States at Paris*[*].

DESCRIPTION.

THIS aerolite presents, [illegible] ·al, the same characters as those hitherto examineu [illegible] s enveloped with a thin, black, and uneven crus [illegible] mass is principally composed of a granular sub [illegible] which breaks easily; it has an earthy appearance a [illegible] y cinereous colour, which, in certain parts, passes [illegible] tish gray.

Those portions which [illegible] this last tint, and which are as if glued in the mass, [illegible] round form, so that they are distinguished by cir [illegible] oval spots which interrupt the general colour. I [illegible] gravity is about 3·3: the sharp parts cut glass.

In observing the fra [illegible] rts of the stone, we there perceive: 1°. Particular [illegible] which are easily detached; little cells in which the [illegible] ced, and of which the substance is like that of th [illegible] self, except that its grain is more compact, and its [illegible] e smoother.

In exposing it to a strong light, we see traces of a lamellar tissue: 2°. grains of metallic iron, which, by polish, assume a whiteness, yield to the hammer, and attract very sensibly the magnetic needle: 3°. grains of oxided iron of the colour of rust: 4°. metallic particles extremely small, of a silver white colour, which seem to be of iron; and this opinion is strengthened, when we recollect that the native iron of Kamerdorf, and that of pseudo-volcanic origin, present, in certain parts, a silver white colour.

I have not seen any mark of sulphurated iron, although I found it by the analysis, as will hereafter appear.

All the fragments of this stone have a magnetic property, but without polarity; and the iron, which is very visible in certain parts, is so disseminated in all others where it escapes the eye, that the property of which there is question, manifests itself even in the smallest particles isolated by trituration.

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[*] From *Annales de Chimie* of March 1810.

I found it even in the globulous bodies which are first mentioned.

Pieces of this stone weighed from six to even 100 pounds.

ANALYSIS.

Having ascertained, by preliminary essays, that this stone contained chrome, nickel, iron, manganese, lime, magnesia, silex, alumine, and sulphur; I employed the following method of separating each of these substances.

1°. 100 grains of this stone, from which the metallic iron was isolated, by means of the magnetic needle, after being pulverised, were treated with a considerable quantity of water, through which was passed a current of oxygenated muriatic gas: by this means, the sulphur being converted into sulphuric acid, by the oxygen of the oxygenated muriatic acid, sulphates and muriates were obtained.

2°. The whole was evaporated to siccity, and treated with two parts of alcoholic potash: after fusion the mass presented a fallow colour, and its dissolution in water was of a fine yellow.

3°. The portions of the mass, which remained undissolved in water, were dissolved in an excess of muriatic acid; and being evaporated to siccity, I separated the silex, which after calcination weighed 41 grains.

4°. Into the muriatic acid was poured carbonate of potash in excess, which formed an abundant precipitate after an hour of ebullition.

5°. The liquor contained sulphate and chromate of potash: after being made acid, it was precipitated by muriate of barytes in excess, and there was obtained sulphate of barytes, corresponding to 2⅓ of sulphur: and saturating afterwards the excess of acid by an alkali, I obtained chromate of barytes, corresponding to 2⅓ of chromic acid.

6°. The precipitate, No. 4, was submitted (still in a humid state) to the action of alcoholic potash, and after filtration, the liquor gave, by means of the muriate of ammonia, a grain of alumine.

7°. Ammonia was poured into the remains of the precipitate, after having dissolved it in an excess of muriatic acid. The oxides of iron and manganese were precipitated, and the lime and magnesia remained in dissolution.

8°. The precipitate was isolated, and the lime separated from the magnesia by the oxalate of ammonia, which, after calcination, weighed three grains.

The magnesia was precipitated by caustic potash: it weighed, after desiccation, 16 grains.

9°. The oxides of iron and manganese were dissolved in an excess of muriatic acid, and pouring, by little and little, saturated carbonate of potash into the dissolution until red floccules were visible, and then leaving it to repose 24 hours, all the carbonate of iron precipitated, whilst that of the manganese remained in the liquor.

The carbonate of iron, after calcination, gave 24 grains of oxide: and that of manganese, deposited by ebullition, by the same operation, only 1⅓.—Which makes in all:—

Silex	41
Sulphur	2⅓
Chromic	2⅓
Alumine	1
Magnesia..	16
Lime	3
Oxide of i	30
Oxide of r se....	1⅓
Loss	3
.....	100

Analysis of the met isolated by the magnetic needle. 1°. 100 parts of e gave 28 of metallic iron, which is very brittle, o he nickel it contains. 2°. 40 grains of this iron w ved in nitro-muriatic acid, and by means of amm excess the oxide of iron was separated, which weighed 45 grains. The dissolution of nickel in this alkali was evaporated to siccity to expell all the ammonia. The oxide of nickel was redissolved by muriatic acid, and precipitated by the prussiate of potash, which gave one grain of the prussiate of nickel. We may infer, from these physical characters, and results of chemical analysis, that this stone is like all other meteoric stones hitherto known.

V. *Proposal for constructing, and putting in its Place, an Iron Tunnel under the River Thames.* By Colonel LENNOX.

To Mr. Tilloch.

SIR, I HEREWITH send you a plan for a tunnel under the Thames, which I hope you will not deem unworthy of a place in your valuable Magazine.

Being informed that no particular plan has yet been determined on by the Thames Tunnel Company*, they are extremely welcome to adopt this if they think fit. I cannot avoid encouraging a hope, that it will be found practicable: but, should my partiality render me too sanguine in favour of it, as the idea, I believe, is new, the publication of it may lead to some other of more ingenuity, and which may be easier and safer in the execution.

Explanation of a plan for constructing a tunnel of cast-iron under the river Thames:—

Fig. 1. A A, (Plate II.) section across the river. The waving line shows the present depth of the river; *a a*, the additional depth required by the plan.

Fig. 2. B B B, three of the frames of which it is proposed that the whole tunnel shall be formed: they are to be of cast-iron, each of one piece, and to be joined together by the flanches *d d d*, which are all one foot broad and four inches thick, with the screws *e e e*, in figs. 2 and 4, of four inches diameter, with half-inch sheet-lead between:—or the joints may be secured with the cement employed by steam-engine builders.

Fig. 3. C C, section of the tunnel, showing the above three frames, in figs. 2, in perspective; each frame to be ten feet in length, eighteen feet wide inside, and twelve feet high at the sides; the top to be convex, rising two feet in the middle; to be four inches thick at the bottom and sides, and three at top. Each frame will weigh upwards of forty tons†.

Fig. 4. D D D, elevation of the same frames, which shows the screws that unite the exterior flanches, and also the iron cramps, *f f*, which embrace the two adjoining flanches at bottom; these cramps to be each twelve inches broad, six inches thick, and two feet high.

Fig. 3. *g g*, tubes of eight inches bore, with openings to receive leakage water, and to convey it to one of the ends to be pumped out.

i i i, screw-holes. The dotted line *k k* expresses the paving when the whole is completed.

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* I believe the Thames Tunnel Company have settled the plan they mean to follow; but as the ideas suggested by Col. Lennox may prove useful on some future occasion, I have given it a place in this number.—Edit.

† Should the carriage of pieces of this weight from the foundry be found impracticable, the sides, and top and bottom, may be cast in separate pieces, with flanches to join them at the corners. In this case the joinings of the different parts may be so disposed that no two of the transverse joinings shall coincide, which will give additional strength to the whole, as every joint may thus be supported with three solid pieces at that place in which it falls.

By the section across the river it appears that the depth of the bed at low water (being only about 30 feet) is not sufficient to admit of laying down a tunnel such as I propose upon the bottom, without obstructing the course of the stream, or interfering with the ease of navigation: therefore, the first thing necessary would be, to excavate the bed of the river entirely across to about 16 feet deeper, and from 60 to 80 feet wide; and to render it as even as possible throughout; which I conceive may be effected without extraordinary labour or difficulty. This being done as far as from *b* to *c*, fig. 1, about 800 feet or something more, I will next suppose eighty of those frames, previously formed agreea [illegible] he plan and section, figs. 2 and 3, to be joined by [illegible] g them strongly together, as represented in figs. 2 [illegible] with half-inch sheet-lead between the flanches; w [illegible] operation should be performed on the bank of th [illegible] r rather below the level of low water, in a situation [illegible] the tide may have free access to it.

If then the ends of the [illegible] tunnel-frames so joined be (when empty) close stopp [illegible] ith strong oak plank, and well secured so as to render [illegible] m perfectly water-tight, *a machine is formed which [illegible] e admission of the tide will float;* as may be proved [illegible] the subjoined calculation, which for greater securit [illegible] not include the convexity of the top. At spring- [illegible] erefore, the whole may be floated to the required s [illegible], and *by additional weights applied* sunk in its prope [illegible]. But in case of any irregularity in its descent, or unevenness of the bottom prepared to receive it, *by removing those additional weights it will again become buoyant*, when the necessary remedies may be applied and obstacles removed. When once properly placed, by turning cocks fixed in each end it will soon fill with water and be permanently bedded.

Calculation of the weight of this tunnel in round numbers:—

Cast-iron..	20,020	cubic feet.... about	4,270	tons.
Lead......	566	....	178	
Oak	200	....	5	
		Tons..	4,453	
Water displaced	1,850,000	cubic feet ...	5,162	
This tunnel will require to sink it more than			709	
Exclusive of the convexity at top estimated at			60	
		Total..	769	

With respect to the manner of sinking this machine, I propose that two short ropes of sufficient strength, with loops at each end, should be passed over each frame, and slightly secured to keep them in their places; that when the machine is floated to its destined situation, (which should be about an hour before low water at the lowest tide,) anchors and cables shall be in readiness to secure it in its place; and that then a number of boats (suppose 160) shall attend, half on one side and half on the other, each with five tons of ballast conveniently disposed so as immediately to hook on to the ends of the short ropes before mentioned, in such a manner that one end of the tunnel shall not sink before the other, but both exactly together. These weights may be so regulated as occasion may require, should there appear any irregularity in its descent; and when it is placed as desired, and water admitted to fill it, they may be removed altogether*. The whole of this operation may, I am persuaded, be effected in two hours, that preceding and that following ebb-tide, if every previous arrangement be properly made.

This part of the tunnel is then supposed to occupy the space from *b* to *c*, fig. 1, and to be placed so as that the upper surface of it shall be nearly equal to, or rather below, the present bottom represented by the waving line. After which, by piling off the tide from low-water mark, the ends may be finished, as on dry land; which may be done either by a continuation of the same frames, or by arches of masonry or of brickwork, as may be judged best. It will then only remain to open the communication with the middle part, by removing the oak planking at each end, and pumping out the water; when, by laying a sufficient quantity of ballast so as to form a road-way clear above the lower flanches, and restoring the banks to their former state, the tunnel will be immediately ready for use.

In the execution of this project a situation should be selected close to low-water mark, of nearly 300 yards in length, where it would be necessary to lay down blocks of sufficient strength to support the great weight, and upon which the whole 80 frames may be screwed together, the level of which should be at least 15 feet below that of spring tides to ensure its floating when completed.

It may be objected to this great machine, that from its

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* Or perhaps the sinking may be conveniently effected by merely admitting from 800 to 1000 tons of water into the tunnel, a pump of sufficient power being properly secured in each end frame to pump out 200 or 300 tons, should it be found necessary to float the tunnel again.

vast weight and great length, the power o
each joining would be inadequate to hold tl
fectly together; and that in case of accide
must be infallibly lost, as it would then be
remove it from the spot on which it wou
sink, or even to separate the different parts
the tunnel formed in the manner propose
jected to no particular force whatsoever at
but be altogether equally borne up by the
the weights necessary to sink it may be al
applied as to ensure its regular descent,
form of the whole when joined as above,
the top and rising at the ends, together wi
thickness of the metal at the bottom, are
particularly favourable; and as from the
bottom it is sure to rest on a soft and unifor
on which it cannot meet with any object to
partial bearing,—I conceive the danger of a
remote, and the strength of the entire suffici
besides, trials may be made in a safe situatio

The chief difficulty appears to me to be
of the bed of the river to the depth requi
mode of effecting this, or whether it would
to choose another situation in which the
might be found sufficient, I leave to mor
perienced engineers to determine; stating
the materials of which the tunnel is to be
be procured for about £44,000; allowing
additional for all other charges incurred in
I do not conceive the expense would excee
£66,000.

I beg leave further to add, that if it shoul
enlarge this tunnel so as to afford a foot-p
to the space allowed for two carriages to pa
it may safely be done by giving it six more
making altogether 24 feet between the inte
and in order to afford it still greater streng
this case omit the interior lateral flanches, ar
thereof, apply plates of cast-iron of three c
thick, the full height of the sides, to extend
dle of one frame to that of the next, to be
number of the same kind of screws to the si
adjoining frames, with sheet-lead between ar
covering the joint inside. This would gi
great additional strength without much i
weight, besides that it would leave nearly a

free space inside; the increase of expense on this account would not, I suppose, much exceed twelve or fifteen thousand pounds, in addition to that before stated.

I am, sir,
Your obedient humble servant,
Bath, June 1810. W. CAULFIELD LENNOX.

VI. *Six Theorems, containing the chief Properties of all* Regular Douzeave Systems of Music; *with Twelve Corollaries thence deduced, showing others of their Relations; and Thirteen Scholia, containing the Temperaments of as many Systems, calculated thereby. With Remarks. By Mr.* JOHN FAREY.

To Mr. Tilloch.

SIR, I AM much pleased to observe, that at length a beginning has been made, at publishing tables of the Beats in 15″; made by the 72 concords in different systems of Musical Temperament, by your new correspondent Mr. C. J. Smyth, of Norwich, in your last Number, who will, I hope, persevere, and give us tables of many other systems; accompanied by such critical remarks, on their comparative merits and defects in practice, as he appears well qualified to make, either in your Magazine, or in the separate work which he has announced on the subject.

Some time ago, I had thoughts of preparing a work on Harmonics, perhaps as a kind of supplement to Dr. Robert Smith's justly celebrated work; but the prospect being now rather distant, of my being able to find leisure to complete this design, I am induced by the above paper of Mr. Smyth's, and the publication of a small work on Harmonics, by Mr. J. Marsh of Chichester, to transcribe from my papers some *Theorems*, showing the properties of *regular douzeaves*, or of such systems of twelve notes in the octave, as have all their fifths alike tempered, except, that between ♯G and ♭E, when there is a bearing fifth or quint wolf: but first I beg to make a few remarks.

In *douzeaves*, or systems of twelve notes, there are generally 16 wolves or tempered concords, differing* from the temperaments proper to the six several concords respectively.

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* Wolves, taken in their general sense, are not always larger than the temperaments, but may be equal to them, as happens throughout the isotonic or equal temperament scale, and may even be less than their respective temperaments, in some cases, as in scholia 1 and 7: they are, in fact, the places in the douzeave or other defective scales, where the resulting intervals or unavoidable inequalities fall, and, as such, are very important to be known and attended to by the composers of music, to be performed in such scales.

In *regular douzeaves*, none of these wolves occur, in any of the six concords, upon any of the four middlemost key-notes, viz. G, D, A or E respectively.

C, F, ♭B and ♭E have no wolves in their major concords (*i. e.* the III, V or VI upon them, respectively.)

B, ♯F, ♯C and ♯G have no wolves in their minor concords (*i. e.* 3d, 4th, or 6th.)

Whence it follows (se[illegible]. Smith's Harmonics, Plate XIX, p. 162, 2d edit.) th[illegible] *major Keys*, modulation can be made from C by ♯s, [illegible]gh the keys G, D, A and E without false concords or w[illegible]s; but if we proceed further by ♯s, B has a false III, ♯F [illegible] its III and VI false, ♯C has its III and VI false, ar[illegible] has its V, III and VI false, which last chord Mr. [illegible] calls *the wolf*, by way of eminence, p. 450 of last [illegible]e.

And in major keys, [illegible]on can also be made from C by ♭s, through F, ♭B, [illegible], and no major wolves occur, (yet ♭E has a false 4th); [illegible] proceeding further to ♭A (or ♯G) its V, III and VI [illegible]alse, as above.

In *minor Keys*, mod[illegible] can be made from A by ♯s through E, B, ♯F, ♯C, [illegible] without any minor wolf (yet ♯G has a false Vth), but [illegible]eeding further to ♯D (or ♭E), its 4th, 3d and 6th are [illegible]

And in minor keys, [illegible]tion can also be made from A by ♭s through D and G, without any false concords or wolves: but on continuing thus to modulate, C has a false 6th, F has its 3d and 6th false, ♭B its 3d and 6th false, and ♭E (or ♯D) has its 4th, 3d, and 6th false, as above.

The six following *Theorems*, express in terms, of the fractions $\frac{r}{s}$ and $\frac{t}{u}$ (either proper or improper) of the small intervals *Schisma* and *most Minute** or Σ and m, all the temperaments and wolves of the 72 concords, in any regular douzeave; and whence, such temperaments can readily be calculated for any proposed system, or the various properties and relations of its intervals can be discovered and computed: and by means of other theorems, the beats can be calculated from such temperaments, in terms of Σ and m. In the article BEATS in the "Edinburgh Encyclopædia," such a set of Theorems will shortly be given, with examples of the use of each, as will perhaps supersede the necessity of publishing them in your Magazine.

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* See vol. xxxii. p. 142, and engraved table, plate V.

Theorem 1. In any *regular douzeave*, or system of 12 notes, where

11 major *fifths* are each tempered (flat) by .... $\frac{-r}{s}\Sigma + \frac{-t}{u}$m;

throwing the bearing, resulting fifth or wolf on ♯G (that is between ♭A and ♭E);

then, the value of this resulting Vth or wolf will be .... $\frac{11r-12s}{s}\Sigma + \frac{11t-u}{u}$m.

Theorem 2. And, 11 *minor fourths* will each be (sharp) .... $\frac{r}{s}\Sigma + \frac{t}{u}$m;

and, the resulting 4th or wolf on ♭E will be .... $\frac{12s-11r}{s}\Sigma + \frac{u-11t}{u}$m.

Theorem 3. And, 8 *major thirds* will each be (sharp) .... $\frac{11s-4r}{s}\Sigma + \frac{u-4t}{u}$m;

the 4 resulting IIIds or wolves on ♯G, B, ♯F and ♯C being each ...... $\frac{8r-s}{s}\Sigma + \frac{8t}{u}$m.

Theorem 4. And, 8 *minor sixths* will each be (flat) .... $\frac{4r-11s}{s}\Sigma + \frac{4t-u}{u}$m;

the 4 resulting 6ths or wolves on C, ♭E, ♭B and F being each .... $\frac{s-8r}{s}\Sigma + \frac{-8t}{u}$m.

Theorem 5. And, 9 *minor thirds* will each be (flat) .... $\frac{3r-11s}{s}\Sigma + \frac{3t-u}{u}\text{m}$;

the 3 resulting 3ds or wolves on ♭E, ♭B and F being each .... $\frac{s-9r}{s}\Sigma + \frac{-9t}{u}\text{m}$.

Theorem 6. And, 9 *major sixths* will each be (sharp) .... $\frac{11s-3r}{s}\Sigma + \frac{u-3t}{u}\text{m}$;

the 3 resulting VIths on *F, *C and *G being each .... $\frac{9r-s}{s}\Sigma + \frac{9t}{u}\text{m}$.

Whence we may readily derive many useful Corollaries, viz.

Corollary 1. The sum of the 11 temperaments of the *fifth*, and its wolf, is a *constant quantity*, viz. $12\Sigma + \text{m}$, or the *Diaschisma*.

For $\frac{-11r}{s}\Sigma$ is the first part of the sum of the temperaments in theorem 6, and $\frac{11r-11r-12s}{s}\Sigma$ or $\frac{-12s}{s}\Sigma$ $= -12\Sigma$ is the aggregate of this sum and the wolf: likewise $\frac{-11t}{u}\text{m} + \frac{11t-u}{u}\text{m}$ is the aggregate of the remaining part, $= \frac{-u}{u}\text{m} = -\text{m}$, and therefore $12\Sigma + \text{m}$ with negative signs, indicating a *flat* temperament, is that which belongs to the fifths collectively, as I observed in volume xxix. p. 348: yet, Mr. J. Marsh in his recent "Theory of Harmonics," p. 14, says, that the same is "a *Diesis* or quarter tone," "the same degree as the 3 thirds mentioned in § 27," that is, $21\Sigma + 2\text{m}$!! See cor. 3.

Corollary 2. The sum of the 11 temperaments of the minor *fourth*, and its wolf, is constantly $12\Sigma + \text{m}$, as observed above.

Corollary 3. The sum of the 8 temperaments and 4 wolves of the *major thirds*, is $84\Sigma + 8\text{m}$, or four enharmonic Diesises.

Corollary 4. The sum of the 8 temperaments and 4 wolves of the *minor sixths*, is $-84\Sigma - 8\text{m}$, as above.

Corollary 5. The sum of the 9 temperaments and 3 wolves of the *minor thirds*, is $-96\Sigma - 9\text{m}$, or three Semitones minimum.

Corollary 6. The sum of the 9 temperaments and 3 wolves of the *major sixths*, is $96\Sigma + 9\text{m}$, as above.

Corollary 7. The sum of the temperaments of the minor third and of the major third, is equal to the temperament of the fifth.

For $\frac{3r-11s}{s} + \frac{11s-4r}{s} = \frac{-r}{s}$, the first part of the temperament of the fifth, and so of the latter part.—(See Dr. Smith's Harmonics, cor. 6, p. 42.)

Corollary 8. The sum of the temperaments of the fifth and of the major sixth, is equal to the temperament of the major third.

For $\frac{-r}{s} + \frac{11s-3r}{s} = \frac{11s-4r}{s}\Sigma$; also $\frac{-t}{u} + \frac{u-3t}{u} = \frac{u-4t}{u}\text{m}$, or $\frac{11s-4r}{s}\Sigma + \frac{u-4t}{u}\text{m}$, as in theorem 3. (See Dr. Smith's Harmonics, cor. 8, p. 43.)

Corollary 9. The difference between the *wolf* and the *temperament* of each of the six concords respectively, is the same, viz. $\frac{12r-12s}{s}\Sigma + \frac{12t-u}{u}\text{m}$, and is what Dr. Smith, at pages 160, 223, &c. calls the *Diesis*; it is the difference between adjacent flat and sharp notes, as between *D and ♭E, *A and ♭B, &c. p. 163.

For $\frac{11r-12s}{s} - \frac{-r}{s} = \frac{12r-12s}{s}\Sigma$, and $\frac{11t-u}{u} - \frac{-t}{u} = \frac{12t-u}{u}\text{m}$, in the fifths. Also, $\frac{8r-s}{s} - \frac{11s-4r}{s} = \frac{12r-12s}{s}\Sigma$, and $\frac{8t}{u} - \frac{u-4t}{u} = \frac{12t-u}{u}\text{m}$, in the major third, as before, and so of all the others.

Corollary 10. The *minor Limma* of Dr. Smith, p. 223, or the value of a *sharp* or a *flat*, is, $\frac{58s-7r}{s}\Sigma + \mathrm{f} + \frac{5u-7t}{u}\mathrm{m}$; or, without f*, we have $\frac{58{\cdot}1496s-7r}{s} + \frac{5{\cdot}0136u-7t}{u}\mathrm{m}$.

As is easily deduced from my theorems above, by the process in page 158, of the Harmonics. As is also the following,

Corollary 11. The *major Limma* of Dr. Smith is $\frac{46s+5r}{s}\Sigma + \mathrm{f} + \frac{4u+5t}{u}\mathrm{m}$; or t f, we have $\frac{46{\cdot}1496s+5r}{s}\Sigma + \frac{4{\cdot}0136u+5t}{u}\mathrm{m}$.

Corollary 12. The *mean* of regularly tempered systems, is $\frac{104s-2r}{s}\Sigma$ - $+ \frac{9u-2t}{u}\mathrm{m}$; or, without f we have $\frac{104{\cdot}2992s}{s}$ $+ \frac{9{\cdot}0272u-2t}{u}\mathrm{m}$.

By multiplying these Corollaries, I should, perhaps, exceed your limit st therefore content myself for the present with the f , as particular applications of the theorems and coro bove, viz.

Scholium 1. If a douze equired, in which *the fifths should be perfect*, we have by theorem 1, $\frac{-r}{s} + \frac{-t}{u} = 0$; which condition will be answered, if r and t each = 0 and s and u each $= \pm 1$; which values substituted in the fifth wolf $\frac{11r-12s}{s}\Sigma + \frac{11t-u}{u}\mathrm{m}$, gives $-12\Sigma - \mathrm{m}$ as it ought to do, by cor. 1.

Also, in theorem 3, $\frac{11s-4r}{s}\Sigma + \frac{u-4t}{u}\mathrm{m}$ gives $11\Sigma + \mathrm{m}$ or c, a major comma, as the sharp temperament of each major third in this system; also in theorem 6. $\frac{11s-3r}{s}\Sigma + \frac{u-3t}{u}\mathrm{m}$ gives $11\Sigma + \mathrm{m}$ or c, as the sharp temperament of the major sixths; which last temperaments prove the same as they must do, to fulfil the conditions of cor. 8.

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* By an examination of plate V in your 28th volume, and of a more extensive table of Intervals, it appears, that the number of Σs, always exceeds the number of m's in ratios between those of 12·5 : 1 and 10· : 1 (those of S and Є). But the major *comma* and its aliquot parts, most frequently occurring in temperaments, I have adopted its ratio of 11 : 1, and thus find $\mathrm{f} \doteq {\cdot}1496\Sigma + {\cdot}0136\mathrm{m}$; from which equation, the latter ones in cor. 10, 11 and 12 are obtained.

Though the fifth wolf is here so large, the IIIds and VIths wolves are each Σ only.

This system approaches very near to that of Mercator, wherein the octave is divided into 53 equal parts, and where $-\cdot 035094\Sigma$ is the flat temperament of the fifth. See M. Sauveur's general table of tempered systems, Mem. de l'Acad. 1711, 16mo. p. 416.

Scholium 2. If a douzeave be required, in which *the major thirds should be perfect,* we have in theorem 3 $\frac{11s-4r}{s}=0$, or $11s=4r$, whence $\frac{r}{s}=\frac{11}{4}$; also $\frac{u-4t}{u}=0$, or $u=4t$ and $\frac{t}{u}=\frac{1}{4}$; whence it appears, that the fifth is to be flattened $\frac{11}{4}\Sigma+\frac{1}{4}\mathrm{m}$, or $\frac{1}{4}\mathrm{c}$; and, by substituting the above values in the wolf, the same appears to be $21\Sigma+2\mathrm{m}$, as in cor. 3; and by the same in theorem 6, we get $\frac{11}{4}\Sigma+\frac{1}{4}\mathrm{m}$, or $\frac{1}{4}\mathrm{c}$, the sharp temperament of the major sixth; the same with that of the fifth; see cor. 8.

This is the *Mean Tone* system of Salinas, Zarlino, Aretinus, &c. Dr. Smith's Harmonics, p. 36, 41, &c. wherein the adjacent flat and sharp notes are distant $21\Sigma+2\mathrm{m}$, or an enharmonic Diesis, as appears by substituting the above values of $\frac{r}{s}$ and $\frac{t}{u}$ in cor. 9.

It is also nearly the same with a division of the octave into 112 equal parts, (see M. Sauveur's table above quoted), wherein $-2\cdot 8290\Sigma$ is the flat temperament of the fifth; $\frac{1}{4}\mathrm{c}$ above, being $-2\cdot 751966\Sigma$.

Scholium 3. If a douzeave be required, in which *the major sixths should be perfect,* we have in theorem 6, $\frac{11s-3r}{s}=0$, or $11s=3r$, whence $\frac{r}{s}=\frac{11}{3}$; also $\frac{u-3t}{u}=0$, or, $u=3t$; whence $\frac{t}{u}=\frac{1}{3}$, and $\frac{11}{3}\Sigma+\frac{1}{3}\mathrm{m}$, or, $\frac{1}{3}\mathrm{c}$, is here the flat temperament of the fifth: and by

substituting the same in the wolf we g
as in corollary 6; likewise in theore
$\frac{11}{3}\Sigma + \frac{1}{3}$m, or $\frac{1}{3}$c, the sharp tempe
major third also, as is consistent with
IIId and VIth wolves are here each $28\frac{1}{3}\Sigma$
Harmonics, p. 42.)

This system approaches very near to
the octave into 19 equal parts, where
the flat temperament of the fifth, (Dr
monics, p. 158, makes it $-\frac{1}{4}$c $-\frac{3}{35}$
or $-3{\cdot}6955\Sigma$); $-\frac{1}{3}$c above, is $-3{\cdot}6$
M. Sauveur's table.

The cases of equality of temperament
III and VI, and between the V and VI
red, in schol. 1 and 3, I proceed to

Scholium 4. If a douzeave be required,
major fifth shall be as much tempere
major sixth is sharp; from theorems 1
$\frac{-r}{s} = \frac{11s-3r}{s}$, or $11s = 2r$; whence
$\frac{-t}{u} = \frac{u-3t}{u}$, or $u = 2t$; whence $\frac{t}{u} =$
$+ \frac{1}{2}$m, or $\frac{1}{2}$c, is here the temper
fifth: and by substituting this value in
have $\frac{11}{2}\Sigma + \frac{1}{2}$m, or $\frac{1}{2}$c, for the te
the major sixth also. The V and VI w
each $48\frac{1}{2}\Sigma + 4\frac{1}{2}$m.

Scholium 5. If a douzeave be required, in w
fifth shall be as much tempered flat as t
is tempered flat; from theorems 1 an
$\frac{-r}{s} = \frac{4r-11s}{s}$, or $5r = 11s$; whence $\frac{r}{s}$
$\frac{-t}{u} = \frac{4t-u}{u}$ or $5t = u$, whence $\frac{t}{u} =$
$+ \frac{1}{5}$m, or $\frac{1}{5}$c, is here the temperamen
and by substituting this value in theore
$\frac{11}{5}\Sigma + \frac{1}{5}$m, or $\frac{1}{5}$c, for the flat temp

major third also: the temperaments of the major sixths being $\frac{22}{5}\Sigma + \frac{2}{5}m$, or $\frac{2}{5}c$.

This is the system of Mr. John Holden, since metamorphosed into an *irregular douzeave* in the article *Monochord*, Enc. Brit. 3d edit. vol. xii. p. 240, and into a still different one, by the Rev. Mr. Hawkes. (See your xxvith volume, p. 171, and xxxth volume, p. 5.)

It also approaches near to M. Sauveur's system of 43 equal parts in the octave, (see his general table above referred to) wherein $-2{\cdot}1177\Sigma$ is the flat temperament of the fifth; $\frac{1}{5}c$ above, being $-2{\cdot}201573\Sigma$.

One other case of this kind, viz. where the major thirds are tempered flat as much as the major sixths are tempered sharp, will be found to arise from different considerations in scholium 11.

Scholium 6. If a douzeave be required, in which *the temperament and wolf of the fifth shall be equal,* we have from theorem 1, $-r = 11r - 12s$, or $12r = 12s$, whence $\frac{r}{s} = \frac{1}{1}$; also $-t = 11t - u$, or $12t = u$, whence $\frac{t}{u} = \frac{1}{12}$, and $\Sigma + \frac{1}{12}m$, or $\frac{1}{11}c$, is the flat temperament of the fifth, in this case: and which substituted in theorem 3, either for wolf or temperament, gives $7\Sigma + \frac{2}{3}m$, or $\frac{7}{11}c$, the sharp temperament of the major third; also, in theorem 6, gives $8\Sigma + \frac{3}{4}m$, or $\frac{8}{11}c$, the sharp temperament of the major sixth.

This is the *Isotonic* or Equal Temperament System of Mersennus, &c. called by Mr. Marsh and others, though improperly, the Equal Harmony System (see scholium 10). See vol. xxix. p. 347: see also Dr. Smith's Harmonics, p. 158 and 167. In the latter page, however, the temperaments of the Vth, VIth and IIId are mistakenly said to be $\frac{1}{10}$, $\frac{7}{10}$ and $\frac{6}{10}$, instead of $\frac{1}{11}$, $\frac{8}{11}$ and $\frac{7}{11}$ of a comma, as they are above, very nearly.

Scholium 7. If a douzeave be required, *in which the several wolves shall differ from their respective temperaments, by the least known Interval or most Minute* (m): we have from corollary 9, $\frac{12r-12s}{s}=0$, or $12r=12s$; whence $\frac{r}{s}=\frac{1}{1}$; also, $\frac{12t-u}{u}=-1$, whence $\frac{t}{u}=\frac{0}{1}$. By which we obtain in theorem 1, $-\Sigma$ as the flat temperament of the fifths, and $-\Sigma-$m its flat wolf: also, in theorem 3, we get $7\Sigma+$m the sharp temperaments of the major thirds, and 7Σ their wolves; and by theorem 6, we obtain $8\Sigma+$m the sharp temperaments of the major sixths and 8Σ their wolves.

This is my *Equal Temperament System*, whose tempered fifth, and consequently all its other intervals,
can be *tuned* on an *y means of perfect intervals*
only, viz. 5 4ths — $-\text{III}=\text{V}-\Sigma$. (See vol.
xxviii. p. 65); such g to be upwards from C as
far as ♭A and down om *c* as far as ♭E, between
which notes, the w m, will result. The *beats*
calculated by Mr. at page 452 of your last
volume, belong in his system, and not to the
strict *Isotonic* abo the difference in practice
would be impercep veen these two systems.

Scholium 8. If a douz equired, in which *the ratio*
of the temperament e major thirds shall be to
their wolves as $1\frac{1}{4}$ to that is, or as 5 to 14, we
have from theorem $5:14::\frac{11s-4r}{s}:\frac{8r-s}{u}$;
whence $154s-56r=$ $5s$, or $159s=96r$, and $\frac{r}{s}$
$=\frac{53}{32}$; also, as $5:14::\frac{u-4t}{u}:\frac{8t}{u}$, whence $14u-56t=40t$, or $14u=96t$, and $\frac{t}{u}=\frac{7}{48}$: and $-\frac{53}{32}\Sigma-\frac{7}{48}$m is the flat temperament of the fifth, which substituted in the first theorem gives $\frac{199}{32}\Sigma+\frac{29}{48}$m,

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[*] Mr. Marsh, assuming the true major third to be 48 degrees or parts, states, the tempered III to be $=49\frac{1}{4}$, and the wolf or "extended third," (as he elsewhere calls it) to be $51\frac{1}{2}$ parts: in the system which he most approves; I therefore take the excess of these above the IIId, as giving the ratio of his temperament and wolf, in order to obtain the values of his notes in my theorems. It is not however clear, that such is exactly his meaning; since, 150 being assumed as the measure of the *octave*, the values of the *major third* and of the *diesis* can be no other than 48·28921, &c. and 5·132378, &c. or very nearly in the ratios of 612Σ, 197Σ and 21Σ: and it is not possible for 48 and 6 truly to represent the major third and diesis in such octave, or in any other.

the sharp wolf. In theorem 3, we get the sharp temperament of the major third $= 4\frac{3}{8}\Sigma + \frac{5}{12}m$, and its wolf $= 12\frac{1}{4}\Sigma + \frac{7}{6}m$. And in theorem 6, the sharp temperament of the major sixth $= 6\frac{1}{32}\Sigma + \frac{9}{16}m$, and its wolf $13\frac{29}{32}\Sigma + \frac{21}{16}m$.

This is Mr. J. Marsh's approved method of tuning a douzeave. Theory of Harmonics, page 13.

The system nearest to this of any which I remember to have met with, is that of a division of the octave into 67 equal parts, (see M. Sauveur's table above referred to), where $-1{\cdot}76455\Sigma$ is the flat temperament of the fifth, which here is $-1{\cdot}6574\Sigma$; and the same differs considerably from the other system recommended by Mr. Marsh page 18, which, perhaps after the example of Dr. Smith, he has borrowed from M. Henfling without acknowledgement. See my 10th scholium.

By Dr. Smith's Harmonics, 2d edit. p. 84, prop. XI, latter part of cor. 3, "when the bases and beats (of two tempered consonances) are the same, the temperaments have *ultimately** the inverse ratio of the major terms" of the perfect ratios of these consonances. Whence

Scholium 9. If a douzeave be required, wherein *the fifth* $(\frac{2}{3})$ *and the major third* $(\frac{4}{5})$ *to the same base shall beat equally quick*, the former flat and the latter sharp; we have from theorems 1 and 3, as $5 : 3 :: \frac{-r}{s} : \frac{11s-4r}{s}$, whence $55s-20r = -3r$, or $55s = 23r$, and $\frac{r}{s} = \frac{55}{23}$; also, $5 : 3 :: \frac{-t}{u} : \frac{u-4t}{u}$, whence $5u-20t = -3t$, or $5u = 23t$, and $\frac{t}{u} = \frac{5}{23}$; and $\frac{55}{23}\Sigma + \frac{5}{23}\mathrm{m}$ or $\frac{5}{23}\mathrm{c}$ is the flat temperament of the fifth. In theorem 3, we get $18\frac{3}{23}\Sigma + \frac{17}{23}\mathrm{m}$ the sharp temperament of the major third; and in theorem 6, $20\frac{12}{23}\Sigma + 1\frac{22}{23}\mathrm{m}$ the sharp temperament of the major sixth.

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* The ultimate ratios are in these cases, very near to the exact ratios: thus in scholium 9, $\frac{5}{23}c$, or $2{\cdot}393013\Sigma$, results from the ultimate ratios; the true temperament being $2{\cdot}393693\Sigma$, as derived from the length of string l of the Vth, in the equation $4l^4 - l^3 = \frac{1}{2}$; the difference being less than $\frac{7}{10000}$th of a Σ, an interval altogether imperceptible in practice.

This is the system *for defective scales* which Dr. Smith describes, and recommends, pages 219, 215, 189, 211, and 212: and of which Mr. Atwood has (but without acknowledgement) given the lengths of strings in his "Rectilinear Motion, &c."

A system, wherein the octave is divided into 74 equal parts, to be found in M. Sauveur's table, and where the temperament is $\mathrm{V} - 2{\cdot}3838\Sigma$, differs but very little from $\mathrm{V} - 2{\cdot}393012\Sigma$ in this system.

Scholium 10. If a douzeave be required, wherein *the fifth* ($\frac{2}{3}$) *and the major sixth* ($\frac{3}{5}$) *shall beat equally quick*, the former flat and the latter sharp: we have from theorems 1 and 6, as $5 : 3 :: \frac{r}{s} : \frac{11s - 3r}{s}$, whence $55s - 15r = 3r$, or $55s = 18r$ and $\frac{r}{s} = \frac{55}{18}$; also, as $5 : 3 :: \frac{t}{u} : \frac{u - 3t}{u}$, whence $5u - 15t = 3t$ or $5u = 18t$ and $\frac{t}{u} = \frac{5}{18}$; and $3\frac{1}{18}\Sigma + \frac{5}{18}\mathrm{m}$ or $\frac{5}{18}\mathrm{c}$ is the flat temperament of the fifth: which in theorem 3, gives $-\frac{11}{9}\Sigma - \frac{1}{9}\mathrm{m}$ or $\frac{1}{9}\mathrm{c}$, for the flat temperament of the major third: and in theorem 6, gives $\frac{11}{6}\Sigma + \frac{1}{6}\mathrm{m}$ or $\frac{1}{6}\mathrm{c}$, the sharp temperament of the sixth.

This is the famous *System of Equal Harmony* in 3 octaves, invented by Dr. Robert Smith. See his Harmonics, pages 216, 191, 206, 212, 214, &c. And differs but little from M. Henfling's system, (Mem. de l'Acad. 1711, 16mo, p. 408), wherein the octave is divided into 50 equal parts, as Dr. Smith shows in his Harmonics, p. 157, and states its fifth to be flattened $-\frac{1}{4}c - \frac{1}{37}c$ or $-\frac{41}{148}c$, which is $-3{\cdot}04966\Sigma$; or more correctly it is $-3{\cdot}04811\Sigma$; $\frac{5}{18}c$ being $-3{\cdot}05774\Sigma$ in the system of this scholium.

Scholium 11. If a douzeave be required, wherein *the major third* $(\frac{4}{5})$ *and the major sixth* $(\frac{3}{5})$ *shall beat equally quick*, the former sharp and the latter flat; we have from theorems 3 and 6, as 5 : 5 : : $\frac{11s-4r}{s}$: $\frac{3r-11s}{s}$, whence $11s-4r=3r-11s$ or $22s=7r$, and $\frac{r}{s}=\frac{22}{7}$; also, as 5 : 5 : : $\frac{u-4t}{u}$: $\frac{3t-u}{u}$, whence $u-4t=3t-u$ or $2u=7t$ and $\frac{t}{u}=\frac{2}{7}$; and $\frac{22}{7}\Sigma + \frac{2}{7}m$ or $\frac{2}{7}c$ is the temperament of the fifth. Which in theorem 3, gives $-\frac{11}{7}\Sigma - \frac{1}{7}m$, or $\frac{1}{7}c$, the flat temperament of the major third; and in theorem 6, gives $\frac{11}{7}\Sigma + \frac{1}{7}m$, or $\frac{1}{7}c$, the sharp temperament of the major sixth*.

This is the system which Dr. Smith barely mentions at page 220, on account of its differing so little from equal harmony, in my last scholium; wherein $\frac{5}{18}$ or $\frac{35}{126}c$ is the temperament of the fifth, which here is $\frac{2}{7}$, or $\frac{36}{126}$; the difference being only the $\frac{1}{126}$-th part of a major comma or $\cdot087364\Sigma$: also $\frac{1}{9}$ or $\frac{7}{63}$, and $\frac{1}{7}$ or $\frac{9}{63}$, have a difference of $\frac{2}{63}c$, or $\cdot34946\Sigma$, but little more considerable, in the major thirds; and $\frac{1}{6}$ or $\frac{7}{42}$, and $\frac{1}{7}$ or $\frac{6}{42}$ have a difference of $\frac{1}{42}c$, or $\cdot26209\Sigma$ in the major sixths.

This system differs more from that of M. Henfling;

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* Mentioned by Dr. Robison, Sup. Enc. Brit. 3d edit. ii. 662.

(see Sauveur's table above referred to) than the last, since $-\frac{2}{7}c = -3\cdot1451\Sigma$, and in his system $-3\cdot04811\Sigma$ is the temperament of the fifth.

Scholium 12. If a douzeave be required, wherein *the mean Tone thereof is to its major Limma as* 5 *to* 3, we have from corollaries 11 and 12, as $5 : 3 :: \frac{104\cdot2992s-2r}{s} : \frac{46\cdot1496s+5r}{s}$, whence $230\cdot7480s+25r = 312\cdot8976s -$

$6r$, or $82\cdot1496s =$ 3 1 $\frac{r}{s} = \frac{82\cdot1496}{31}$; also as 5 :

$3 :: \frac{9\cdot0272u-2t}{u}$: $\frac{u+5t}{}$, whence $20\cdot0680u+$

$25t = 27\cdot0816u - 6$ $\cdot0136u = -31t$ and $\frac{t}{u} =$

$\frac{-7\cdot0136}{31}$; and $-$ $\Sigma - \frac{7\cdot0136}{31}$m is the flat

temperament of th

This is the sy M. *Huygens.* (See Dr.

Smith's Harmoni es 158, 168, 121, 208,

224, &c.) whose ient of the fifth, as calcu-

lated by Dr. Smith, c $+ \frac{1}{110}$c or $- \frac{53}{220}$c, $=$

$2\cdot6519\Sigma$; mine ab g about $2\cdot6518\Sigma$.

The octave here osed to be divided into 31

equal parts. See 1 eur's table; Mr. Ambrose

Warren, in 1725, pretended to the discovery of this system. See Monthly Magazine, vol. xxix. page 413.

Scholium 13. If a douzeave be required, wherein *the mean Tone thereof is to its major Limma as* 9 to 5, we have from corollaries 11 and 12, as $9 : 5 :: \frac{104\cdot2992s-2r}{s} : \frac{46\cdot1496s+5r}{s}$, whence $415\cdot3464s+45r = 521\cdot4960s - 10r$, or $55r = 106\cdot1496s$, and $\frac{r}{s} = \frac{106\cdot1496}{55}$; also as $9 : 5 :: \frac{9\cdot0272u-2t}{u} : \frac{4\cdot0136u+5t}{u}$, whence $36\cdot1224u+45t = 45\cdot1360u-10t$, or $9\cdot0136u = 55t$ and $\frac{t}{u} = \frac{9\cdot0136}{55}$; and $-\frac{106\cdot1496}{55}\Sigma - \frac{9\cdot0136}{55}$m is the flat temperament of the fifth, $= -1\cdot9313\Sigma$.

This system answers to a division of the octave into 55 equal parts, and according to the papers of M. Sauveur in the Memoirs of the Parisian Academy, for 1707 and 1711, it was the system used by the Musicians of Paris at or previous to that time. See his general table of tempered systems above referred to.

I am, sir,
Your obedient humble servant,
Westminster, July 11, 1810. J. FAREY.

VII. *Report on the Memoirs presented to the Society of Pharmacy at Paris, in consequence of the Prizes offered in the Year* 1809. *Extracted by M.* BOUILLON LAGRANGE *from the full Report drawn up by Messrs.* NACHET, DEROSNE, *and* VALLEE.

OF nine memoirs sent to the Society, two have particularly fixed the attention of the committee. They were written in answer to the following question:

"To prepare the acetate of potash, so as to obtain it white and saturated, without employing radical vinegar, and without having recourse to fusion;—to point out which of the two, the acid or the alkali, gives it the colouring principle."

The first memoir, with the motto *Ex cognitis incognita*, is written with great precision.

The author, after having ascertained the advantage which would result from obtaining this salt in all its purity by a simple and œconomical process, begins by inquiring from whence the colouring principle arises: It cannot, he says, be owing to the alkali, when it is considered that the fusion of the acetate of potash renders it insoluble, and that the heat requisite for this fusion is not so strong as that which is necessary for the preparation of any given potash; and on the other hand, it cannot be essential to the acetic acid, when radical vinegar is capable of instantly furnishing a colourless salt. Consequently, this colouring principle must be a foreign substance contained in common vinegar, and which may be introduced into it in distillation. But this same principle is less volatile than the acetic acid, since distilled vinegar leaves a residue of it if we evaporate it a second time: it is not very soluble by itself, and it cannot be dissolved except by the addition of acetic acid, since it is precipitated, at least in part, when we saturate the latter by potash: and finally, it has been ascertained that

it is of a vegeto-animal nature, either from t
it exhales when placed on hot coals, or b
of ammonia which upon distillation furnis
of potash prepared with distilled vine
which does not give the same salt prepar
vinegar: whence the author concludes that t
ciple which colours the acetate of potash i
part of the ferment of common vinegar, c
distillation and more or less altered by this

Independently of this colouring princip
the constitution of common vinegar, the
memoir mentions another still more capa
the acetate of potash look brown: this is th
oil with which the vinegar is charged when
is carried too far. He further says, that th
be coloured by the oxides of iron and of n
tained in the alkali, or by the metallic uten
preparation: but this colour being merely
may avoid it entirely by using a pure potas
tin or porcelain. We must therefore adh
ment and the empyreumatic oil. The follo
are given for avoiding these two colouring
ferment may be separated from the acetate
more easily the less of it there is in the d
and the latter will contain so much the less
in common vinegar, the proportion of the f
smaller with respect to that of the acid, on
quantity of ferment brought over in distill
ways more or less in proportion with that
common vinegar. It follows therefore that
above all, to employ common vinegar, whic
most acid and the least charged with fermen
quisite may be attained by choosing a cle
sides being very strong and completely fer
the choice of the vinegar, the process of d
also have some influence on the quantity
contained in distilled vinegar: for, since this
volatile than the acetic acid, the less of it w
distillation, the more slowly this process is
in this respect we may admit a slight ebul
the fittest degree of heat.

If the preceding rules have been well at
distilled vinegar will contain so small a q
ment that it will be capable of furnishing
acetate of potash almost entirely colourless
withstanding every precaution, the whiten

still leaves something to be desired, there remains a final method of remedying it, which consists in filtering through charcoal in powder. The action of this substance, although little known as to its theory, is nevertheless certain in its effects; since it is sufficient to boil slightly with it the solution of the acetate as above prepared, in order to obtain it perfectly white after filtration and evaporation carefully managed. As to the empyreumatic oil, there is only one way of avoiding it, which is to stop the distillation of the vinegar at the moment when this principle begins to come over, and the product gives out an empyreumatic smell: for, beyond this term, the vinegar, if still white in appearance, would not undergo any change of colour during the evaporation of the acetate; and this colour, when once produced, could not be removed, either by charcoal powder or by any other means whatever.

The second memoir presents fuller details. Its motto is taken from Boileau:

> "L'artifice agréable
> Du plus affreux objet fait un objet aimable."

The author describes in the first instance the various processes hitherto adopted in preparing the acetate of potash. He mentions as the most exact the process of M. Bouillon Lagrange, which consists in crystallizing this salt; but he regrets not having been able to put it in practice, from the difficulty of separating the crystallized acetate from the mother waters, which are very thick. In order to obtain as advantageous a result by a more practicable process, he tried double decompositions; he treated acetate of lime with the carbonate or sulphate of potash, but he did not obtain an acetate of potash less coloured than if he had directly saturated the carbonate of potash with distilled vinegar.

It would be necessary, as he observes, to employ a crystallized acetate of lime, but in this case the process would become too tedious and expensive. The decomposition of the common acetate of lead by the carbonate of soda, furnished him with a tolerably white acetate of potash: although this method unites with the facility of using it the advantage of being cheaper, the author of the memoir does not think it right to resort to it, because the smallest negligence in the operation may change a wholesome medicine into a deadly poison. Recurring to the combination described of distilled vinegar and potash, he first inquires whence arises the colour assumed by this salt during its evaporation: he is also aware that it is owing to a foreign principle contained in the distilled vinegar; but he after-

wards saw that this substance was very slightly of a colouring nature by itself: he observed that the acetate of potash well saturated, is found as a consequence of its evaporation with an excess of alkali; and it is this excess of alkali which reacts on the foreign principle contained in the distilled vinegar, and colours it. In order to show more clearly this reaction of the potash, he divided into two equal portions a solution of acetate of potash: he evaporated both at the same degree of heat, maintaining constantly in the one an excess of acid, and in the other an excess of alkali: the salt produced by the liquor with excess of acid was much less coloured than that furnished by
the liquor with an exces i[*]. After having ascer-
tained the origin of the principle and the cause
which develops it, the a st endeavoured to destroy
it; and charcoal in his is the fittest agent: with
this view he filters the vinegar through charcoal,
he then saturates it wit e of potash, leaving in it
an excess of acid, whi es care to keep constantly
in the liquor during its n. The result is an ace-
tate equally white with ned by means of fusion.

This process, althou imple, did not appear to
him to be practicable, the acetate of potash is
mixed with a certain qu acetate of lime, to which
the lime contained in th l has given rise; and this
salt, by altering the pu acetate of potash, retards
its desiccation. It wo l, be very easy to separate
it by adding a slight exc bonate of potash, in order
to precipitate the lime; and we should afterwards put in an excess of acid: but it is easier to saturate the acid first. The following is the process as described in the memoir:

Pour into distilled vinegar a solution of carbonate of potash, until no more carbonic acid is extricated: afterwards evaporate the liquor, taking care always to keep an excess of acid in it: when it is reduced to three-fourths, allow it to cool, in order to separate from it the sulphate of potash and some impurities; decant it in order to heat it, and pour it when hot on a charcoal filter[†].

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[*] We have reason to believe, from our own experiments, that the potash still reacts, but much less on the colouring principle, even when the liquor contains an excess of acid; since by operating in this manner we always obtain an acetate of potash which is more or less coloured, whilst the same vinegar is capable of furnishing acetate of lime, magnesia, and alumine, which are very white. Soda did not appear to us to act so strongly as potash on this principle.—*Note by the Committee.*

[†] The acetate of lime when dried is less deliquescent than the acetate of potash, and yet it is much more difficult to produce the desiccation of it.—*Note by the Author.*

If the filtered liquor does not contain more free acid, add a little distilled vinegar*; then evaporate to dryness: and if we wish to obtain the acetate of potash well cleaned, we must, at the end of the evaporation, manage the fire properly, and not stir it; but in this case it is not so white as when we separate it with a silver spatula, and throw it on the edges of the basin as fast as it is formed at the surface of the liquid: this salt will also be whiter if dried by small portions.

On exposing for about 20 days to the solar rays the liquor filtered over charcoal, the author obtained a salt much whiter: hence he thinks that the same result might be obtained, by exposing to the light an acetate of potash made from distilled vinegar, without being filtered through charcoal.

He regrets that he has been unable to collect some important facts relative to the colouring matter: he remarked that it was partly precipitated after saturation; that it is a little soluble in water, and that a portion remains in solution in the liquid acetate of potash; that after having filtered distilled vinegar through very pure charcoal, like that which is produced from crystallized sugar, we no longer obtain, on saturating it with crystallized carbonate of potash, the same precipitate as before filtration.

The author of the memoir concludes, therefore,—

1. That the colouring matter of the acetate of potash belongs to a vegetable substance contained in distilled vinegar.
2. That this colouring matter is destroyed by charcoal.
3. That an excess of alkali, when we evaporate the result of the saturation of distilled vinegar by potash, may influence the whiteness of the acetate of potash.
4. That in order to obtain the earth pure white and saturated, it is sufficient to filter a concentrated solution of it over a small quantity of charcoal in powder; to keep in it afterwards to the end of the evaporation an excess of acid, by adding from time to time distilled vinegar, and to expose it for some days to the solar light.

In a note which terminates this memoir, the author says, that according to the valuable observation of Messrs. Vauquelin, Pontier and Derosne, he obtained two hectogrammes of excellent acetic ether, by rectifying over potash the first products of the distillation of 70 litres of distilled vinegar.

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* A little acetic acid (radical vinegar) would be preferable; very little is necessary when care has been taken to filter the liquor in the neutral state: we must also take care that it is not acid, in consequence of the lime which is in the charcoal.

The committee carefully repeated the e
tailed in the above memoirs. If we except
effects on exposing the acetate to the sun,
succeed with them, they pronounced them
rect; indeed, the principal agent in the whi
was already known. Lowitz has recommen
charcoal, in order to obtain an acetate of potas
than by the ordinary process; but whether h
ciently described the method of using it, or
gar of a bad quality, ill distilled or ill saturat
the depurating qualities of the charcoal hac
several chemists have been unsuccessful.

From these considerations, and particularl
tisfactory results obtained by the committ
themselves warranted in concluding, that the
two memoirs would have done better by mak
principle and the causes of the colouring of t
same time that they indicated the means of
removing them.

By following carefully the rules which t
and by taking all the precautions which t
we shall easily obtain, without having reco
an acetate of potash very white and perf
The society has therefore decreed a gold
value of 100 francs to each of the authors of

The author of the memoir first mention
noully of Bale; and of the second, M. Frem

VIII. *Of the Influence of Solar and Lunar Clouds and Vapours.* By Salem Har

To Mr. Tilloch.

Sir, In perusing the theory of the tides as
down by Kepler, and subsequently improv
Isaac Newton, I was forcibly struck with a
the attraction of the sun and moon (more p
latter) is capable (as the ebb and flow of the
have proved beyond dispute) of acting
power upon that immense and ponderous m
to raise its waters on those parts which
of the heavenly bodies alternately place in
their attraction; its effects upon the clouds,
exhaling particles, or comparatively speaking
those waters, must be still greater; and suf
duce, in conjunction with or opposition to th
frequent and apparently uncertain chang

hourly experience in the atmosphere. Impressed with this idea, and not without some degree of wonder to find it, as far as I could learn, unnoticed by the philosophical world, I began when at school to form a journal of the weather; noting at every observation the quarter of the wind, as well as the moon's altitude and azimuth; and had the satisfaction of finding my infantile speculation so well grounded, that I observed the weather almost invariably thick or rainy, when the wind and moon, being at or near the same quarter, were acting in conjunction; the latter drawing the clouds, as I imagine, to her nearest point of the horizon, from whence the former drives them over its surface; and that it became proportionally clearer as their relative change of situation enabled the wind to counterpoise the moon's attraction, and prevent those vapours from collecting.

In the year 1809, a voyage across the Atlantic, and a residence of some months at Havannah, enabled me to extend my observations to the northern extremity of the trade winds, as well as the climate of the torrid zone, both on sea and land. I shall therefore extract the journal of a few days in each of the situations wherein I have noticed the weather; with a slight comment on the nature of the country and the prevailing winds, or periodical change of seasons, leaving your philosophical readers to compare my statement with the idea that gave it birth,

Journal of the Weather at Wandsworth, near London.

Day of the Month.	*Time of Day.*	*Wind.*	*Moon's Azimuth.*	*Moon's Altitude or Depression.*	*Observations.*
Oct. 27, 1800.	Morn	S.S.W.	N.	50° Depr.	Very fine weather.
	Even	S.	S.E. by S.	28° Alt.	Cloudy and rainy.
28	Morn	S.S.W.	N.N.W.	44° Depr.	Fine weather.
	Even	S.S.E.	S.E. by S.	23° Alt.	Cloudy and rainy.
29	Morn	Variab. fr. S.E. to S.W.	N.W. by N.	35° Depr.	Fine weather.
	Even	S.W. by W.	S.E.	24° Alt.	Fair weather, but cloudy.
30	Morn	S.E. by E.	N.W.	26° Depr.	Very fair, but rather cloudy
	Even	Ditto.	S.E. by E.	23° Alt.	Very cloudy, but no rain.
31	Morn	S. by W.	N.W.	15° Depr.	Raining a little, but appears to be clearing off.
	Even	S.W.	E S. E.	24° Alt.	Very fine weather; no clds.
Nov. 1	Morn	S. by E.	N.W. by W	4° Depr.	Fine weather; a few light [clouds.
	Even	S.W.	E.	21° Alt.	Ditto ditto.
2	Morn	S.W.	W.N.W.	5° Alt.	Fine weather, a few clouds in the N.E. horizon.
	Even	S.E. by E. strong.	E. by N.	17° Alt.	Cloudy, with a little rain.

ournal of the Weather at Sea, between Madei Verd Islands.

ay of the onth.	Time of Day.	Wind.	Moon's Azimuth.	Moon's Altitude or Depression.	
arch 16, 309.	Morn	E.S.E. nearly calm.	E. by S.	25° Alt.	Very clou ward.
	Even	N.W. very light.	W. by N.	8° Depr.	Cloudy, b
17	Morn	N. rath. strong.	E. by S.	14° Alt.	Very fine
	Even	N. moderate.	W. by S.	19° Alt.	Ditto
18	Morn	N.W. moderate.	E. by N.	8° Alt.	Very fine light cl
	Even	N. very light.	W. by N.	6° Alt.	Fair; but
19	Morn	N.N.W. nearly calm.	E. by N.	0°	Very fine
	Even	N. nearly calm.	W.	24° Alt.	Ditto
20	Morn	N. nearly calm.	E.N.E.	8° Depr.	Very fine windw
	Even	S.W. nearly calm.	W. by N.	37° Alt.	Ditto
21	Morn	N.W. strong.	N.E by E.	15° Depr.	Cloudy a
	Even	N.W. fresh.	W. by S.	53° Alt.	Fair; but of the to winc ing sho
22	Morn	N.W. by W. strong.	N.E. by E.	20° Depr.	Very fine the hor
	Even	N.N.W. rath. strong.	W. by S.	58° Alt.	Very fine
23	Morn	N.N.E. moderate.	N.E.	34° Depr.	Very clou
	Even	N.E. moderate.	W.S.W.	81° Alt.	Very fine light cl
4	Morn	N. by W. moderate.	N.N.E.	42° Depr.	Very fai clouds, ward a
	Even	N.N.E. moderate.	W. by S.	78° Alt.	Very fine parts o
5	Morn	N.N.E. moderate.	N.E. by N.	48° Depr.	Very clou appears
	Even	N.E. moderate.	In the	Zenith.	Very fine several

Between the above two observations (about half past tl
ier a heavy squall of rain with a strong breeze from the N.
he zenith, and most parts of the horizon cloudy.

On the 16th, I was in sight of Madeira, and crossed the tropic of Cancer on the 24th, in longitude 24° 30′ west of Greenwich; the ship's course being S.S.W. The former part is consequently on the verge of the trades; as the latter is of the torrid zone. At this season of the year the winds are variable, but generally strong.

Journal of the Weather at Havannah.

Day of the Month.	*Time of Day.*	*Wind.*	*Moon's Azimuth.*	*Moon's Altitude or Depression.*	*Observations.*
Sept. 10, 1809.	Morn	N. by W.	E. by S.	21° Alt.	Very fine: a few light clouds to leeward.
	Even	N.N.E.	W.	10° Depr.	Fine: a few clouds to windwd.
11	Morn	S.E.	E.	7° Alt.	Fair; but very cloudy.
	Even	E.N.E.	W. by S.	0°	Very fine and serene.
12	Morn	S.W. very light.	E. by S.	5° Depr.	Very fine: a few clouds rising to windward.
	Even	E.S.E.	W. by S.	8° Alt.	Very fair: some clouds to do.
13	Morn	S.E. nearly calm.	E.	21° Depr.	Very fine: some clouds rising in the eastern horizon.
	Even	N.N.E.	W.S.W.	10° Alt.	Very fine: a few clouds near the moon. [At noon this day the thermometer stood at 90°.]
14	Morn	E.S.E. nearly calm.	E.	32° Depr.	Fine: some clouds rising to windward.
	Even	N.	S.W. by W.	31° Alt.	Very fine and serene.
15	Morn	S. by E. nearly calm.	E. by N.	42° Depr.	Very fine: light clouds rising to windward.
	Even	N.	S.W.	35° Alt.	Very fine and serene.
16	Morn	S.S.E.	E. by N.	64° Depr.	Very fine: some clouds to leeward.
	Even	N. strong.	S.S.W.	42° Alt.	Very fine: some clouds to windward.

Havannah is situated but a few miles south of the tropic of Cancer, and the land about it moderately high. It is sheltered by the hills of the Cavannah from all winds between N.E. and E.S.E., and these are by far the most prevalent. In the evening, however, it often shifts to the north, or even a point or two to the westward. At this season of the year the weather is intensely hot, with frequent and violent storms of rain and thunder, which usually take place between the hours of two and six in the afternoon.

nal of the Weather between Bermuda and the Western Isles.

Time of Day.	Wind.	Moon's Azimuth.	Moon's Altitude or Depression	Observations.	Height of Thermometer.
Morn	E.S.E. very strong.	W. by N.	17° Alt.	Fine: but rather cloudy in many parts of the horizon.	67°
Even	E.S.E. light.	E.N.E.	8° Dep.	Cloudy, with a lit. rain.	68°
Morn	E. by S. strong.	W.	25° Alt.	Very fine and serene.	67°
Even	N.E. strong.	N.E. by E.	18° Dep.	Cloudy, with small rain.	69°
Morn	N.E. blows hard.	W. by	lt.	Hazy weather.	66°
Even	N.N.E. a heavy gale.	N.E.	ep.	Very thick and cloudy, with small rain.	66° 30
Morn	E.N.E. strong.	W.S.W	lt.	Very fine: a few clouds in the horizon.	67°
Even	E.N.E. strong.	N.E.	ep.	Cloudy, with occasional showers.	69°
Morn	N.E. by E. very strong.	S.W. by	lt.	Thick hazy weather, with some small rain at intervals; appears now to be clearing off.	67°
Even	E. light.	N.N.E.	ep.	Thick and cloudy, with small rain. [Between the above observations, hazy weather with occasional showers; the wind light and variable.]	68°
Morn	E. moderate.	S.W.	45° Alt.	Fine: a few clouds, which are clearing off.	67° 30′
Even	N.E. by E. moderate.	N.	51° Dep.	Very fine: a few clouds to windward. [Much rain between 11 P.M. of the 29th and 1 A.M. of this day: the wind and moon being nearly in the same point (E. by N.) and the latter on the horizon.]	68°
Morn	N.N.E. moderate.	S.S.W.	53° Alt.	Very fair, but rather cloudy, particularly near the moon.	67°
Even	N. by E. moderate.	N.	55° Dep.	Fair; but cloudy in most parts of the horizon, particularly from N. to W.	67° 30′

In this part of the globe, more particularly during the tumnal and winter months, the wind is usually strong;

often increasing to a gale; the heaviest usually blows from the north and north-west. These observations were made between the 35th and 37th degree of north latitude, and 44° and 39° longitude west of Greenwich: the ship's course being E. by N.

There are certainly many places, in which a particular wind almost invariably produces rain; from the intervention of a chain of hills, or even a single mountain that impedes the regular course of the clouds, when moving in a certain direction; and breaks them over the valleys below. Or, the wind may be either sufficiently strong to overpower the moon's attraction, or so light as to afford no assistance in spreading the clouds which have been collected on or below the horizon, and thus produce an effect upon the weather contrary to what might have been expected from the relative situation of the impelling powers; a circumstance which, though very material, did not strike me when I began my observations; and the velocity of the wind is consequently unnoticed in the first part of my journal. Our atmosphere may contain at times so little vapour, as to be incapable of producing rain, although the moon and wind were acting ever so much in unison; but this can always be ascertained by the state of the barometer. When also the moon's altitude or depression is so great as to place her nearly in the *zenith*, or the *nadir*, her attraction can of course avail but little, either in assisting or counteracting the effect of the wind from whatever point it may happen to blow: its power, in short, must diminish in proportion as her distance from the horizon increases.

I do not pretend to improve, much less to controvert, the theories of those many learned and scientific characters who have written upon the nature and variation of the atmosphere; for my knowledge in every branch of philosophy is very slight; but I cannot help thinking, that a little attention to the subject which I have noticed, would frequently assist an observer of the weather, in foreseeing with additional certainty an approaching change; and I offer these remarks to the public, with no other view than the possibility of their being investigated, by those who possess the knowledge and leisure requisite in philosophical studies, to the advancement of science, as well as the benefit of those professions, in which a dependence is placed upon the atmosphere. I remain, sir,

Your respectful humble servant,

Richmond Green, July 10, 1810. SALEM HARRIS.

IX. *On*

IX. *On Crystallography. By M.* HA *from the last Paris Edition of his* Traité

[Continued from vol. xxxv. p. 460.

TABLE OF THE CRYSTALLINE

I. *Substances which have a common prim the same dimensions.*

Names of the Substances.	Form of the int
1. CUBE.	
BORATED magnesia	Cube
Muriated soda	Ditto
Amphigene	Irregular tetr
Analcime	Cube
Sulphurated lead	Ditto
Sulphurated iron	Ditto
Oxidated tin	Ditto
Gray cobalt	Ditto
Calcareous scheelin	Regular tetra
2. REGULAR OCTAHEDRON	
Fluated lime	Regular tetra
Muriated ammonia	Ditto
Sulphated alumine	Ditto
Spinelle	Ditto
Pleonaste	Ditto
Diamond	Ditto
Red oxidated copper	Ditto
Oxidulated iron	Ditto
Native bismuth	Ditto
Native antimony	Ditto
3. REGULAR TETRAHEDRO	
Pyritous copper	Regular tetra
Gray copper	Ditto.
4. RHOMBOIDAL DODECAHED	
Garnet	*Tetrahedron triangles e*
Sulphurated zinc	Ditto.

II. *Substances the primitive forms of wi the same kind, with dimensions respecti each.*

1. RHOMBOID.

* *With obtuse summits.*

Carbonated lime	Rhomboid

Names of the Substances.	Form of the integrant Molecule.
Tourmaline	Irregular tetrahedron
Chabasie	Rhomboidal
Dioptase	Ditto
Sulphurated antimoniated silver	Rhomboidal.

** *With acute summits.*

Corundum	Rhomboidal
Oligistous iron	Ditto
Sulphurated iron	Ditto.

2. OCTAHEDRON.

* *Pyramids with square bases.*

Alkaline fluated alumine..	Irregular tetrahedron
Zircon	Ditto
Harmotome	Ditto
Anatase	Ditto
Molybdated lead	Ditto
Mellite	Ditto.

** *Pyramids with rectangular bases.*

Nitrated potash	Irregular tetrahedron
Carbonated lead.........	Ditto
Sulphated lead	Ditto
Oxidated zinc...........	Ditto.

*** *Pyramids with rhombic bases.*

Sulphur................	Irregular tetrahedron
Red sulphurated arsenic...	Irregular tetrahedron.
Blue carbonated copper...	Ditto.

3. TETRAHEDRAL PRISM.

1. STRAIGHT PRISM.

* *With square bases,*

Sulphated magnesia.......	Isosceles-rectangle-triangular prism
Idocrase	Ditto
Meionite	Prism with square bases
Wernerite	Ditto
Mesotype	Isosceles-rectangle-triangular prism
Chromated lead	Ditto

Names of the Substances.	Form of the integrant Molecule.
Oxidated uranium	Prism with square bases
Oxidated titanium	Isosceles-rectangle-triangular prism.

** *With rectangular bases.*

Cymophane	Prism with rectangular bases.
Euclase	Ditto
Peridot	Ditto
Prehnite	Ditto
Stilbite	Ditto
Ferruginated scheelin	Ditto

*** *W* [illegible] *bic bases.*

Sulphated barytes	lene - rectangle - triangular prism
Sulphated strontian	to
Topaz	m with rhombic bases
Staurotide	sceles-rectangle-triangular prism
Macle	certain
Mica	m with rhombic bases
Talc	to
Arsenical iron	to
Sulphurated molybdenu	to
Siliceo-calcareous titaniu	to.

**** *With obliqu* [illegible] *parallelogram bases.*

Sulphated lime	m with oblique - angled parallelogram bases
[illegible]	Prism with oblique - angled parallelogram bases
[illegible]	Ditto.

2. OBLIQUE PRISM.

[illegible] *rectangled bases.*

[illegible]	Prism with rectangled bases.

[illegible] *rhombic bases.*

[illegible]	Prism with rhombic bases
[illegible]	Ditto
[illegible]	Oblique triangular prism
[illegible]	Prism with rhombic bases.

[illegible] *-angled parallelogram bases.*

[illegible]	Prism with oblique angled parallelogram bases

Disthene

Names of the Substances.	Form of the integrant Molecule.
Disthene	Prism with oblique angled parallelogram bases
Sulphated copper	Ditto.

4. REGULAR HEXAHEDRAL PRISM.

Phosphated lime	Equilateral triangular prism
Telesie	Ditto
Emerald	Ditto
Nepheline	Ditto
Pycnite	Ditto
Dipyre	Ditto
Sulphurated mercury	Ditto.

5. PYRAMIDAL DODECAHEDRON.

Quartz	Irregular tetrahedron
Phosphated lime	Ditto.

III. *Forms which are found to be secondary in different species.*

1. CUBE.

Names of the Substances,	Primitive Forms.
Fluated lime	Regular octahedron
Native bismuth	Ditto.

2. REGULAR OCTAHEDRON.

Muriated soda	Cube
Sulphurated lead	Ditto
Sulphurated iron	Ditto
Gray cobalt	Ditto.

3. REGULAR HEXAHEDRAL PRISM.

Carbonated lime	Obtuse rhomboid
Corundum	Acute rhomboid
Mica	Straight prism with rhombic bases
Sulphurated antimoniated silver	Obtuse rhomboid
Phosphated lead	Pyramidal dodecahedron
Sulphurated molybdenum.	Straight prism with rhombic bases.

4. RHOMBOIDAL DODECAHEDRON.

Fluated lime	Regular octahedron
Oxidulated iron	Ditto.

5. SOLID WITH 24 EQUAL AND SIMILAR TRAPEZOIDS.

Names of the Substances.	Primitive Forms.
Muriated ammonia	Regular octahedron
Garnet	Rhomboidal dodecahedron
Amphigene	Cube
Analcime	Ditto
Sulphurated iron	Ditto.

Explanation of the plan which has been adopted in the descriptions of the different species of minerals.

After having given the synonymy of the best known authors, we have successively presented the essential character of the substance, and the physical, geometrical, and chemical characters, the assemblage of which forms the specific character.

We have excluded from this character every thing connected with fugitive accidents, such as colours, when they are owing to a principle which is only interposed in the substance.

In the detail of the geometrical characters, care has been taken to indicate not only the direction of the natural joints, but also the greater or less facility of obtaining them, their difference of neatness in one and the same crystal, and those, in short, whose positions are only presumed. In addition to this, we have made known, in a note, the respective dimensions of the molecule, and all that may serve as data for applying the theoretical calculus to the laws of decrements upon which the secondary forms depend.

After the indication of the chemical characters, we have given the result of the analyses of the substance which seem to have merited most confidence.

The table of varieties, which follows the characters, is generally divided into two sections, one of which contains the descriptions of the forms, and the other refers to the accidents of lights. The forms are either determinable, *i. e.* susceptible of being described geometrically, from the number, the disposition and the mutual incidences of their faces, or indeterminable, *i. e.* produced by a confused or precipitated crystallization, so that geometry cannot describe them, and we can at most indicate the vague relations which exist between them and known objects; as when we say of a mineral that it is cylindrical, globular, granular, &c. and the last term of this kind of degradation of forms is expressed by the word *amorphous*, which designates a substance in masses of a certain volume completely irregular.

The description of every determinable variety presents successively the name which it bears, conformably to the principles of the method of nomenclature which has been above explained, the indication of its representative sign, that of its figure, its synonymy, according to Romé de l'Isle or other crystallographers; and lastly, the measurements of its principal angles. When the structure of the variety is complex, we add to its description explanations proper for better understanding the results of the laws upon which it depends.

The indications relative to colour and to transparency compose the second section, under the title of *Effects of light**. It is proper to remark on this subject, that any given form may offer successively all the varieties of colour and transparency, and that, in return, every colour and every degree of transparency may be met with in every kind of form. But it is unnecessary to overload the method with all these combinations. It is sufficient, if it presents a method of indicating that which exists in any given variety, to describe this variety completely. Thus the table of the characters of telesie contains implicitly all the following combinations: *primitive limpid telesie; unitary red transparent telesie; amorphous translucid telesie.*

When the name which we have adopted for one species of mineral has been applied to different species, from a delusive resemblance, such as colour, we indicate these double applications in a particular table placed at the end of that of the varieties; and I hope I shall be applauded for the tedious task which I have entered upon, in order to clear up the confusion which arose from these communications of one and the same name to substances so ill adapted to be associated with each other.

Each article is terminated by annotations relative to the situation of the substances in the ground, to the researches which have made us acquainted with them, to its physical properties, its uses in the arts, medicine, &c. I have even thought it right to present most of these objects more in detail than has been generally done, so as to avoid the dryness of too concise indications, without however giving myself up to a multiplicity of details which would appear to be misplaced in a treatise upon mineralogy.

[To be continued.]

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* We have placed the word *limpid* at the head of effects of colours, because it seemed natural to commence here by the privation of character, since it indicates that the substance is in the greatest possible state of purity.

X. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

June 28, The President in the chair. The conclusion of M. De l'Isle's paper on the poison of the *bohan upas* and *antea* was read. The emetic power of this, poison suggested to the author the propriety of making some experiments with other emetics, by injecting them into wounds and blood-vessels in the same manner as he did the upas. Ipecacuanha and tartar emetic were injected, and both produced very violent effects, particularly the latter; but they were not so destructive ial life as the upas. On dissecting the bodies of th als killed by injecting this poison, and comparing the h the effects of common emetics, he was led to conc hat the upas does not kill by any specific action on t ves, but that, by acting on the blood only, it is so i ieously destructive to animal life.

A paper from Mr. Goo ad, describing the nature of the horny concretions ppeared all over the skin of a heifer exhibited in Lo st year. The head, neck, and shoulders of this ai ere thickly covered with little horns of various le d thickness, some of them nearly three inches long. ears that these horns were chiefly composed of calca atter, and that one-fourth of them was of an animal

July 5, Dr. Wollaston paper on a peculiar species of urinary calculus, which he called cystic oxide, only two specimens of which he has been able to procure. The cystic oxide dissolves in solutions of all the alkalies, but not in saturated carbonate of ammonia. Dr. W. also took occasion to correct some essential errors in his paper on calculi, which appeared in the Philosophical Transactions for 1797; subsequent experience having convinced him that phosphate of lime and phosphate of magnesia rarely or never exist together in the same calculus.

A paper on muriatic acid, by Mr. Davy, was read. The object of Mr. Davy's paper was to detail some new facts respecting the muriatic acid. Finding that charcoal, though ignited to whiteness, will not burn or decompose oxymuriatic acid gas, he was led to institute experiments to determine whether oxygen could be procured from it by any means: and the results of his inquiries are, that there is no proof whatever of its containing that substance. Muriatic acid gas may be decomposed into oxymuriatic acid and hydrogen; and recomposed from these bodies. In all cases in which oxygen gas is procured from oxymuriatic acid gas, water is present: and the oxygen is furnished by the water; and hydrogen is always combined with the oxymuriatic acid gas; so that, as inflammable bodies decompose water by attracting oxygen, so oxymuriatic acid decomposes it by attracting hydrogen. Mr. Davy has detailed some experiments which render it probable that the body called hyperoxymuriatic acid is in fact the simple basis of the muriatic compounds, and that it forms oxymuriatic acid by uniting to hydrogen, and common muriatic acid gas by uniting to more hydrogen.

In attempting to decompose oxymuriatic acid gas by the combustion of phosphorus and the action of ammonia, Mr. Davy discovered a very singular compound; which, though composed of oxymuriatic acid and ammonia with a little phosphorus, is neither fusible, volatile, nor decomposable at a white heat; neither soluble in acid nor alkaline menstrua; and possessed of no taste or smell.

Mr. Davy has detailed nine modes of decomposing common salt, founded upon these new facts, and has formed nine deductions from them respecting the composition of chemical agents in general.

A paper on pus, by Dr. Pearson, was read. Previously to the author's observations and experiments, a brief historical account was given of what has been already done on the subject. The conclusions among many others are: That the pus consists essentially of three differentsubstances, viz. An opake animal oxide, seemingly already self-coagulated; matter analogous to the coagulable lymph of the blood, but in a different state of aggregation. 2. Innumerable spherical particles, seen with the microscope, separable by chemical agents from the other parts. 3. A limpid coagulable liquid, in many properties similar to the serum of blood. The saline impregnations are the same as those of serum of blood and expectorated matter, especially muriate of soda, neutralized potash, and the phosphates of lime. Various other substances are frequently found in pus, which are considered to be accidental, and depend upon different diseases.

The Society then adjourned till Thursday the 8th of November.

IMPERIAL SOCIETY OF NATURAL HISTORY OF MOSCOW.

M. Fischer, president of this society, has published the following short account of their labours for the last four years. This sketch is arranged under the following heads: I. Labours and Undertakings of the Society. II. Miscellanies. III. Promotions and Rewards. IV. Necrology. V. Literary Novelties. VI. Minutes of the Society, and Report of the Presents made to the Society and to the Museum of the Imperial University. The following are the contents of the first branch of their labours.

Journey to Siberia undertaken at the expense of the Society.—This expedition set out on the 9th of February 1809, and is to last three years. It is composed of Professor Tauber, who is known from his description of the valley of Plauen in Saxony; M. James M nown from his travels in Germany, France, Engla d Sweden; and M. Helm, botanist and chemist, kn his description of several new plants, and by several es: this is his second visit to Siberia. These gentl accompanied by two pupils, Messrs. Kotoroff an sky, and they are provided with every necessary, such oks, charts, instruments, and a chemical laboratory y were to be occupied the first year with the Ouxal of mountains; the second, with that of the Altai; th l, with the mountains of the Daourie; and, if circ ces will permit them, they will also visit Kamschat e profound erudition and zeal of the above gent fford reason to hope for some important discove hey are also accompanied by a draftsman, and by on who is acquainted with the art of stuffing and pre animals.

Description of the Government of Moscow.—His Imperial Majesty having given five thousand roubles to be expended in examining the immense district which goes by this name, the professors of Moscow have recently visited several parts of the country with this view. The following is an account of what has been already done: Some astronomical and trigonometrical observations have been repeated at Moscow, and in some districts of the government, such as Svenigorod berea, Moja'sk, Riousa, by professors Goldbach and Panthner, attached to the repository for charts at St. Petersburgh. The latter has also established, at the expense of the society, barometers and thermometers at the above places, in order to obtain some useful observations.

M. Fischer undertook the natural history department: he was accompanied in his excursion by M. Drouginine, secretary to the society; and by M. Gorke, one of the pupils at the university of Moscow. From the lateness of the season they procured but few plants or insects, but they were more fortunate in their mineralogical pursuits. Petrifactions of all kinds, several mineral springs rich in iron and carbonic acid, a good clay for earthenware, Labrador stone, garnets in granite and in gneus, granatite in gneus, and a new earthy substance, were procured by them. This new substance is of a very fine lavender blue, and is found in veins several lines thick between layers of cimolite, which in some places forms the transition to a true mountain cork. Sometimes it is found on round masses of flint, sometimes fossil shells are found in it, and pectinites which are wholly black and changed into flint. This substance contains, according to the analyses of Messrs. Helm and Muller, lime, alumine, and phosphoric acid. It forms, therefore, a new species adjoining the Apatite, and it has been designated by the name of Ratofkite, from the place where M. Fischer resides.

Mr. Davy's experiments.—M. Jacquin in a letter to M. Fischer informs him, that in concert with his friends the director Schreibers, colonel Tihursky, and M. Bremser, he repeated the recent experiments of Mr. Davy with success. They generally made use of a battery with vertical piles composed of 1300 pairs of disks, which were generally three inches in diameter, and formed together 70 square feet of surface in contact:—the experiment succeeded however with 300 pairs of disks, and it was even perceptible with 70 pairs. One of the processes adopted by the above gentlemen seems to be somewhat novel: they placed in a wine glass a small piece of alkali moistened in the air, on a small plate of platina which communicates with the hydrogen pole, and which was entirely covered with rectified petroleum. Finally, they placed on the alkali a thin plate of platina, and pressed it with a metallic rod communicating with the oxygen pole. The effects being remarked, bubbles of air were extricated as in the first experiment; sometimes there were trifling detonations; and some time afterwards they found the whole of the inferior surface of the alkali strewed with small scales having a metallic appearance like those which are seen floating in the petroleum. This preparation is very beautiful, particularly when placed in the microscope. It is not combined easily with mercury; for a globule adhering to the point of the brass wire, when plunged in mercury, was not detached, and afterwards detonated in water as before.

In the experiment last described, the place of the platina may be supplied by a flat piece of charcoal. The diamond and sulphur are not conductors of the electric fluid, and produce no effect. The experiment does not succeed better in vacuo than in the open air. "What is this substance (M. Fischer asks) which resembles a metal? Is it the alkali reduced, or one of its constituent parts, which being combined with oxygen represents it, as Mr. Davy seems to think? or, Is it hydruret of potash? But whence this metallic appearance?"

Miscellanies.—Their majesties the Emperor Alexander I. and the King of Prussia have examined with great interest the skeleton of the mammoth brought from the shores of the Lena by M. Adams *.

M. Tilesius, associate of [illegible] academy, well known for his talent at painting obje[illegible] natural history, has prepared 40 folio drawings [illegible] mammoth. His observations do not seem to [illegible] entirely with those of Cuvier.

The meteorological o[illegible]ns from Moscow prove that the cold was greate[illegible] night between the 11th and 12th of January. I[illegible]nan froze mercury in a saucer exposed to the ai[illegible]nt Bontourline observed that the mercury in three [illegible]hermometers was frozen, and sunk into the bowl. [illegible]n a thermometer which was not frozen, he found [illegible]m six in the morning to six in the evening, on the [illegible] January, the cold was at 35° of Reaumur. M. R[illegible] Troitsk, observed it at 34 degrees before the mercu[illegible]ozen.

The botanist Frederick Fischer, and M. Langsdorff associate of the academy, who accompanied Krusenstern in his voyage round the world, are occupied with a work on the Ferns. They have prepared drawings of several new species.

M. Fischer, the professor and director of the academy, is collecting materials for a comparative *craniognosy*. An accurate knowledge of the cranium, as one of the chief organs of animal organization, will fill up an important chasm in comparative anatomy. The craniology of Dr. Gall will only be made use of in order to demonstrate the influence of the brain on the form of the excavations of the skull. It will appear in Latin and French, accompanied with engravings.

M. Mohs has made a mineralogical excursion through Carinthia, Carniola, &c. He has been particularly occupied with the situation of the lead mines at Villach.

The Imperial Academy of Petersburgh proposed a prize of 100 ducats for the best memoir on the following subject: "Give an easy method for ascertaining, independent of all knowledge of botany, poisonous plants in an indubitable manner." Three memoirs were consequently given in; but the prize has not been awarded to either.

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* See Phil. Mag. vol xxix. p. 141.

A similar prize has been offered for the best "chronology of the Byzantine authors from the foundation of the city of Constantinople to its conquest by the Turks." The memoirs on the above subject must be transmitted to St. Petersburgh on or before the 1st of July 1811.

XI. *Notices respecting New Books.*

The Medical Society of London have in the press a volume of memoirs, containing several valuable communications, in medical and surgical science, from eminent resident and corresponding members of the society. The title of the volume will be "Transactions of the Medical Society of London, Vol. I. Part I." and it will be accompanied by engravings. Part II. will appear in a few months afterwards, the society having come to the determination of giving publicity to their transactions more frequently than heretofore.

XII. *Intelligence and Miscellaneous Articles.*

DE LUC'S ELECTRIC COLUMN.

To Mr. Tilloch.

July 23d, 1810.

Sir,—Notwithstanding the changes which have happened in the state of the atmosphere, the small bells, which are in communication with De Luc's electric column, have continued to ring without ceasing, as far as my observations have gone, from the 25th of March to this day. Although we have of late had heavy rain accompanied with thunder and lightning, we have not had any very damp weather, which I imagine is the most likely to stop the motion of the small clapper, by depositing moisture on the insulating parts of the apparatus.—If you, or any of your readers, are acquainted with a method of preparing varnish of a better insulating power than those varnishes mentioned in Cavallo's Treatise on Electricity, I shall be glad to have it communicated to me, and others who are interesting themselves in making experiments with this new column.

I wish here to correct a mistake which I made in the account of the electric column printed in your Magazine for March last. I there have miscalled the ends of the column: that which I have named the zinc end should have been named the silver end, and the contrary. So that the effects on the electrometer of the coated jar were, respecting the *plus* and *minus* states, just what might have been imagined they would be. The mistake arose owing to the silver and paper being connected together; for, had the two metals been united, and the paper separate, the instrument would then have resembled more the usual construction of a Galvanic trough; and I should not, I imagine, have been led into any error [illegible] pecting the names of the ends or poles of it. I r [illegible] in, &c.

B. M. Forster.

THE O [illegible] BLIND.

The plan to which we [illegible] in our last has been since published in a prospectus. [illegible] he purpose of this humane institution a convenient h [illegible] as been taken at No. 5, Prospect Place, Lambeth. [illegible] prospectus states: That under the patronage of hi [illegible] higness the Duke of Sussex, a seminary is to be o [illegible] or the tuition of blind subjects of the higher classe [illegible] ciety, where they may be taught reading, writing, [illegible] ns of corresponding with distant friends, music, g [illegible] y, the belles lettres, languages, the rudiments of [illegible] ences generally, and such a familiar acquaintance w [illegible] vailing accomplishments, as will enable the blind of both sexes to partake of the innocent amusements of society, including draughts, backgammon, chess, cards, dancing, &c. Among other additions to the plan of M. Haüy, who succeeded in a similar attempt at Paris before the revolution, and on whose model the institution professes to be formed, it adopts the idea of its pupils deriving, from a constant and consoling illustration of the Gospels, those dispositions to habitual cheerfulness and content which they are so eminently calculated to excite when contemplated properly.

ARTIFICIAL COLD.

Professor Leslie, of Edinburgh, in following out a series of experiments on the relations of air and moisture, has within these few weeks been led to a very singular and important discovery. Without any expenditure of materials, he can, by means of a simple apparatus, in which the action of certain chemical powers is combined, freeze a mass of water, and keep it for an indefinite length of time in the state of ice. In the space of an hour he has, on a small scale, formed a cake of ice 6 inches in diameter, and three quarters of an inch thick. With very little trouble he can produce a permanent cold of 90 degrees of Fahrenheit below the temperature of the air, and might easily push it to 100 or even 110. The professor is now engaged in prosecuting these fruitful researches, and will soon, we hope, favour the public with an account of his process, and of the chief results.

SUPPOSED NEW EARTH.

M. Vinterl, of Pest in Hungary, has lately sent to the French Institute several specimens of an earth which he conceived to be new, and to which he gave the name of Andronia. A committee of the Institute, consisting of Messrs. Fourcroy, Guyton Morveau, Berthollet, and Vauquelin, have analysed this substance, and have determined that it is merely a compound of silex, lime, alumine, potash, and iron.

CRANIOLOGY.

The following observations have been published in the foreign journals on the system of craniology by M. Gall.

1. The Italian poet Dolce, who died in 1568, in his dialogue on the means of preserving and strengthening the memory, alludes to a head which is represented at page 8 of the Venice editions of 1562 and 1586, the cranium of which is divided and figured according to M. Gall's system; and under this wood-cut we read the following inscription: " In questa tu vedi ove è il senso commune, ove la fantasia, la cogitativa, la imaginativa, la stimulativa, la memorativa: ed anco l'odorato e il gusto."

2. The grand chancellor of Denmark, Schumacher, count Griffenfield, who died in 1699, must have practised cranioscopy with success, if we may credit M. Wedel Simonson, the author of a dissertation read before the medical society of Copenhagen. The same gentleman (M. Schumacher) maintained a medical disputation in 1650, *De nervis*; Bartholin being then president of the above society.

3. Frenair (a French author) says in his biography of Laurence Sterne, who died in 1768, and which was prefixed to the French translation of Sterne's works, " that an eminent surgeon had dissected the brain of Laurence Sterne, under the persuasion that he would find something extraordinary in its configuration."

4. Swedenborg, who died in 1774, taught that good or

bad qualities had an influence on the form of the cranium.

5. The principal theorem of M. Gall, that the brain impresses on the cranium its different forms, is also to be found in the "*Fragmens Physiognomiques*" of Lavater, Leipsic, 1775—1778.

A German traveller has recently discovered in the neighbourhood of the Red Sea the ruins of the ancient city of Dscherrasch, probably the Gerusa of antiquity. He found the remains of several public edifices, two amphitheatres, several palaces, a temple, &c.

Geology and natural [illegible] have lately sustained a severe loss by the premat[illegible]h of Mr. William Martin of Macclesfield, Cheshir[illegible]mber of the Geological Society of London, and au[illegible] most useful work, "Outlines of an Attempt to e[illegible] Knowledge of extraneous Fossils on scientific Pri[illegible]" in octavo; and also of "Petrificata Derbiensia[illegible]es and Descriptions of Petrifactions collected in [illegible]re," in 4to. with coloured plates; of which 52 a[illegible]ed in the 1st volume published less than a year b[illegible] death. We are truly concerned to learn that M[illegible] has left a wife and young family without means [illegible]rt; the profession he followed, that of a drawi[illegible]r, as commonly happens in country places, not [illegible]roved very lucrative. We should rejoice to hear t[illegible] considerable progress has been made by Mr. M. towards a second volume of the above highly interesting and useful work, and that some means were devised by the friends of geological science to alleviate the situation of his widow and orphan children.

LIST OF PATENTS FOR NEW INVENTIONS.

To the Rev. Henry Liston, of Ecclesmachen, in Scotland, and Charles Broughton, of Edinburgh, writer to the signet, for improvements in the construction of organs.—July 3, 18[illegible]0.

To Samuel Hill, of Serle-street, London, Esq., for a method of joining stone pipes in a more effectual manner than has been before discovered.—July 3.

To James Hall, of Walthamstow, for a method of manufacturing a material from the twigs or branches of broom, mallows, and rushes, and other shrubs or plants of the like species, to be used instead of flax or hemp; and for the same purposes for which flax and hemp are now used.—July 3.

To John Kent, of Southampton, architect, for certain improvements in the method of making artificial stone.—July 3.

To Robert Howden, of Providence-row, Finsbury-square, baker, for an improved method of extracting foul air out of ships, whereby a constant succession of fresh air will be introduced; and at the same time moderating the degree of heat according to the climate. And also of extracting the foul air from mines and pits of every description, and of regulating the degree of heat, and of giving heat and a constant succession of fresh air to houses in general.—July 3.

To William Shakespear, of Birmingham, and Thomas Osler the younger, of the same place, for an improved method, or methods, of manufacturing glass or paste drops for chandeliers, lamps, and lustres.—July 5.

To Richard Varley, of Cheadle Mosley, in the county of Chester, for certain new additions to and improvements upon the machinery now in use for the roving, spinning, doubling, and twisting, of cotton, silk, flax, wool, mohair, and other materials used for the manufacture of twist, thread, or other kind of yarn.—July 7.

To George Hall, of the Strand, goldsmith, for certain improvements in the art of working and making spoons, forks, and such other articles of gold, silver, or other metals, as usually are or may be stamped or struck by means of seats and punches, or dies of any kind or description; and likewise in the tools or instruments to be used in carrying the said improvements into effect and practice.—July 18.

To Ralph Wedgwood, of Oxford-street, for his new character for language, numbers, and music, and the methods of applying the same.—July 18.

To George Stebbing, of Portsmouth, mathematical instrument maker, for certain improvements on the action and other parts of sea and land compasses.—July 18.

To Benjamin Agerday, of Handsworth, Staffordshire, for improvements in the construction of a toast-stand, (for the purpose of holding a plate before the fire,) a hearth brush or dust brush, and toasting fork, and occasionally in combining or uniting the said brush and toasting fork in one utensil or article.—July 18.

METEOROLOGICAL TABLE,

BY MR. CAREY, OF THE STRAND,

For July 1810.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Thermometer. Noon.	Thermometer. 11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
June 27	57	64°	59°	29·96	30	Fair
28	59	69	66	·99	50	Fair
29	66	68	56	[illegible]	10	Showery
30	60	72	63	[illegible]	60	Fair
July 1	64	74	66	[illegible]	65	Fair
2	60	74	64	[illegible]	70	Fair
3	60	66	55	[illegible]	0	Rain
4	61	56	54	[illegible]	0	Showery
5	60	67	64	[illegible]	51	Showery
6	64	70	62	[illegible]	62	Fair
7	65	74	64	[illegible]	65	Fair
8	67	60	55	[illegible]	10	Rain
9	60	70	58	[illegible]	52	Fair
10	66	69	64	[illegible]	35	Fair
11	65	75	62	[illegible]	66	Stormy
12	60	70	60	[illegible]	49	Showery
13	63	71	64	[illegible]	68	Showery
14	66	69	57	[illegible]	0	Showers with
15	61	69	56	·80	36	Ditto [thunder
16	66	68	55	·93	48	Fair
17	56	64	52	·55	33	Cloudy
18	52	65	55	·70	44	Fair
19	56	66	52	·80	62	Fair
20	56	65	54	·80	60	Fair
21	52	63	52	·94	45	Cloudy
22	53	64	57	30·11	46	Fair
23	57	70	54	·16	70	Fair
24	58	70	57	·14	53	Fair
25	59	73	64	29·96	40	Showers
26	60	63	60	·64	0	Rain

N. B. The Barometer's height is taken at one o'clock.

XIV. *On Pendulums.* *By* Ez. Walker, *Esq.*

The mechanism of pendulums is a subject which has of late years attracted much attention; but whether any real improvements have been made since the days of Harrison, is a question on which there are various opinions. Rods of zinc, pewter, lead, and other soft metals have been substituted for those of brass, to reduce the gridiron pendulum to a more simple form: but it has been found by experience that some of those soft metals, when under the pressure of the weight of the lens, do not long retain the same power of expansion and contraction.

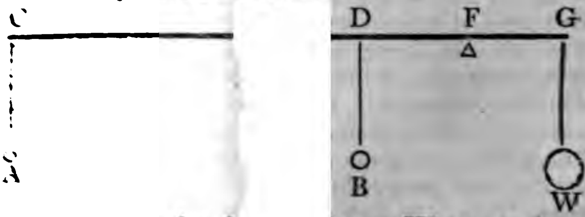
The late Mr. James Bullock, a very ingenious clock-maker, and a man of much experience, told me, that brass and steel were the only metals he could rely on, in the construction of compound pendulums.

The gridiron pendulum is constructed on the supposition that it is kept invariably of the same length by rods of different metals, which have their lengths duly proportioned to their expansions and contractions; but late writers have advanced several objections to this mode of compensation. The principal of these objections are: 1st, The length of the pendulum may be increased by its weight. 2dly, Where the rods pass through the connecting bars there is some friction, which causes them to move by starts, and not according to the increase and decrease of heat: and, 3dly, The difficulty of exactly adjusting the lengths of the rods. But there is another source of error in this pendulum, which has not, I believe, been attended to by writers on this subject.

Suppose that the distance between the centre of the lens and the point of suspension were not to suffer any change by the vicissitudes of heat and cold, still the length of the pendulum might vary. For as the ends of the compensation rods are connected by cross pieces, and as all these, except one, are put in motion by every variation in the temperature of the air, some moving in a direction contrary to the others; therefore it is evident, that the motions of these cross bars must alter the distance between the point of suspension and the centre of oscillation, unless their weights be adjusted according to their motions to and from the point of suspension.

Suppose the cross piece which connects the two extreme rods of the gridiron pendulum be fixed to the centre of the lens, and that the expansions of steel and brass be as three to five: then, if the expansion of one of the extreme steel rods raises the top bar three degrees nearer the point of suspension, the brass rod, which is joined to the steel rod at the top, will remove the cross bar fixed to its lower end, only two degrees from the same point.

To investigate the ratio of the weights of these cross pieces, that the centre of gravity of the pendulum may remain at the same distance from the point of suspension, in all degrees of heat;—Let CG represent an inflexible lever, considered as without weight, kept in equilibrio upon the fulcrum F, by three weights, A, B, and W. And let CF$=x$, DF$=y$, and FG$=z$.



Then per mechanics A [illegible] $\times y = W \times z$, or $Ax + By = Wz$. And supposi[illegible] y to flow in contrary directions, we have $\dot{x}+$ [illegible] $-\dot{y} \times B = W\dot{z} = A\dot{x} + B\dot{y}$. Therefore, $A\dot{x} = B\dot{y}$, a[illegible]quently, $A : B :: \dot{y} : \dot{x}$.

Hence, if the weights [illegible]oss pieces be inversely as their motions, they will [illegible] the distance of the centre of gravity from the poin[illegible]nsion, and "the distance of the centre of suspen[illegible]n the centre of percussion or oscillation, in the same body, will always remain the same; if the distance of its centre of gravity from the point of suspension, and the plane of its motion (in regard to the body) remain the same*."

This construction might improve the gridiron pendulum; but it would be very difficult to exhibit a theorem completely accurate for a mode of compensation which is liable to so many irregularities. A pendulum of a more simple construction, and that might be more easily adjusted for heat and cold, is still an object that merits the attention of the astronomer.

The mercurial pendulum is founded on principles more simple and correct than any other compound pendulum that has yet been invented; but the manner of constructing it with a glass vessel has prevented its being more generally used in the best clocks. This objection is now obviated by the application of a steel rod with a glass vessel attached to it containing quicksilver, so that when the steel rod expands downwards the quicksilver expands upwards, and *vice versâ*. The rate of the clock will show whether the pendulum be over or under corrected; consequently by taking a little quicksilver out of the bob, or adding a little, the compensation may be adjusted to the utmost degree of exactness, and with very little trouble.

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* Emerson's Fluxions, page 312.

It may be supposed that a pendulum cannot be adjusted for heat and cold by the going of the clock, but the rate of my clock shows that this supposition is not well founded; for when a wooden pendulum was attached to it, its rate of going was affected merely by dryness and moisture; but with a mercurial pendulum its rate was affected only by heat and cold, the pendulum being a small matter under corrected; but this variation in its rate during twelve months was very little more than one second per day, although the temperature of the outward air did not vary less, by Fahrenheit's scale, than 70 degrees.

The following register of the going of this clock was computed from the sun's transits over the meridian, observed with a 3½ feet transit telescope.

Lynn, July 16, 1810. E. WALKER.

P. S. For an account of the greatest annual variation in the daily rate of the transit clock at the Royal Observatory for six years, see Phil. Mag. vol. xxxiv. p. 4.

An Account of the Going of a Clock with a mercurial Pendulum made by Mr. Barraud.

1809.	Daily Rate of the Clock.	No. of Days.	1809.	Daily Rate of the Clock.	No. of Days.
June 27	″			″	
July 1	+0·14	4	Aug. 26	+0·36	3
4	0·30	3	30	0·35	4
11	0·55	7	Sept. 7	0·22	8
13	0·37	2	9	0·42	2
14	0·38	1	15	0·20	6
16	0·18	2	17	0·28	2
19	0·16	3	21	0·53	4
26	0·30	7	24	0·54	3
31	0·29	5	29	0·48	5
Aug. 8	0·44	8	Oct. 1	0·43	2
13	0· 0	5	7	0·40	6
17	0·28	4	8	0·41	1
23	0·20	6	11	0·10	3

TABLE (Continued).

1809.	Daily Rate of the Clock.	No. of Days.	1810.	Daily Rate of the Clock.	No. of Days.
26	″ +0·38	15	April 11	″ +1·18	13
Nov. 7	0·44	12	16	1·18	5
9	0·42	2	18	1·08	2
15	0·51	6	20	1·00	2
16	0·37		22	0·75	2
19	0·59		23	0·88	1
21	0·45		25	0·44	2
24	0·93		27	0·27	2
27	0·79		28	0·19	1
Dec. 2	0·82		29	0·23	1
4	0·68		ay 1	0·29	2
10	0·83		8	0·52	7
13	0·91		11	0·56	3
15	0·85		13	0·55	2
21	1·14		14	0·55	1
28	0·89		18	0·63	4
			20	0·62	2
1810.			21	0·39	1
			23	0·44	2
Jan. 1	+0·85		25	0·29	2
			26	0·39	1
5	0·75	4	27	0·31	1
10	0·62	5	30	0·23	3
18	[illegible]	8	June 1	0·24	2
[illegible] 4	1·11	17	8	0·08	7
[illegible]	[illegible]	1	14	0·32	6
[illegible]	[illegible]	5	18	0·33	4
[illegible]	1·22	4	21	0·31	3
[illegible]	[illegible]	5	25	0·15	4
[illegible]	[illegible]	2	29	0·13	4
[illegible]	[illegible]	5	July 1	0·14	2
[illegible] 11	[illegible]	13	7	0·14	6
18	[illegible]	7	9	0·14	2
[illegible]	[illegible]	4	12	0·16	3
[illegible]	[illegible]	1			
[illegible]	[illegible]	2			
[illegible]	[illegible]	4			

XV. *The*

XV. *The Bakerian Lecture for* 1809. *On some new Electrochemical Researches on various Objects, particularly the metallic Bodies, from the Alkalies, and Earths, and on some Combinations of Hydrogen.* By Humphry Davy, *Esq. Sec. R.S. F.R.S.E. M.R.I.A.*

[Concluded from p. 32.]

IV. *On the Metals of Earths.*

I have tried a number of experiments with the hopes of gaining the same distinct evidences of the decomposition of the common. earths, as those afforded by the electrochemical processes on the alkalies, and the alkaline earths.

I find that when iron wire ignited to whiteness, by the power of 1000 double plates, is negatively electrified and fused in contact with either silex, alumine or glucine, slightly moistened and placed in hydrogen gas; the iron becomes brittle and whiter, and affords by solution in acids, an earth of the same kind as that which has been employed in the experiment.

I have passed potassium in vapour through each of these earths, heated to whiteness in a platina tube: the results were remarkable, and perhaps not unworthy of being fully detailed.

When silex was employed, being in the proportion of about ten grains to four of potassium, no gas was evolved, except the common air of the tube mingled with a little inflammable gas, not more than might be referred to the moisture in the crust of alkali, formed upon the potassium. The potassium* was entirely destroyed; and glass with excess of alkali was formed in the lower part of the tube; when this glass was powdered, it exhibited dark specks, having a dull metallic character not unlike that of the protoxide of iron. When the mixture was thrown into water, there was only a very slight effervescence; but on the addition of muriatic acid to the water, globules of gas were slowly liberated, and the effect continued for nearly an

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* The results of this experiment are opposed to the idea that potassium is a compound of hydrogen and potash or its basis; for if so, it might be expected that the hydrogen would be disengaged by the attraction of the alkali for silex. In my first experiments on this combination, I operated in an apparatus connected with water, and I found that the potassium produced as much hydrogen as if it had been made to act upon water; in this case the metal had rapidly decomposed the vapour of the water, which must have been constantly supplied.

hour; so that there is great reason to believ
had been either entirely or partially deoxyge
slowly reproduced by the action of the wat
the slight attraction of the acid for the eart

When the potassium was in the quantity
and the silex of four grains, a part of the
spontaneously as it was taken out of the tu
tube was quite cool, and left, as the result of
alkali and silex. The part which did no
similar in character to the matter which h
scribed, it did not act upon water, but
muriatic acid.

Potassium in acting upon alumine and gl
more hydrogen than could be ascribed to th
sent in the crust of potash; from which it
that even after ignition, water adheres to th

The results of the action of the potas
phoric substances of a dark gray colour
throwing off brilliant sparks *, and leavin
and earth, and which hissed violently wh
water, decomposing it with great violence.
products in two experiments, one on alum
glucine, in which naphtha was introduced
tube, to prevent combustion; the masses w
and presented small metallic particles, wh
as potassium, but so small that they could
so as to be more minutely examined; they
ing naphtha. Either a part of the potas
mposing the earths
tered into combinati
contrary to analog
hich will be immed
s of the earths to
d, there was great
ved with the com
Mercury was th
n the tube of plati
was in excess, I
mercury, whilst the
gave the characters
se of glucine and
g the action of ve

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n, which I have suppose
potassium, sulphur, a
se.

acid upon the amalgam, yet I could not be entirely satisfied that there was any of the metals of these earths in triple combination.

Mixtures of the earths with potassium, intensely ignited in contact with iron filings, and covered with iron filings in a clay crucible, gave much more distinct results. Whether silex, alumine, or glucine was used, there was always a fused mass in the centre of the crucible; and this mass had perfectly metallic characters. It was in all cases much whiter and harder than iron. In the instance in which silex was used it broke under the hammer, and exhibited a crystalline texture. The alloys from alumine and glucine were imperfectly malleable. Each afforded, by solution in acids, evaporation, and treatment with re-agents, oxide of iron, alkali, and notable quantities of the earth employed in the experiment.

Though I could not procure decided evidences of the production of an amalgam, from the metals of the common earths, yet I succeeded perfectly by the same method of operating, in making amalgams of the alkaline earths.

By passing potassium through lime and magnesia, and then introducing mercury, I obtained solid amalgams, which consisted of potassium, the metal of the earth employed, and mercury.

The amalgam from magnesia was easily deprived of its potassium by the action of water. It then appeared as a solid white metallic mass, which by exposure to the air became covered with a dry white powder, and which when acted upon by weak muriatic acid, gave off hydrogen gas in considerable quantities, and produced a solution of magnesia.

By operations performed in this manner, there is good reason to believe, it will be possible to procure quantities of the metals of the alkaline earths, sufficient for determining their nature and agencies, and the quantities of oxygen which they absorb; and by the solution of the alloys containing the metals of the common earths, it seems probable, that the proportions of metallic matter in these bodies may likewise be ascertained.

On an hypothesis which I have before brought before the Society, namely, that the power of chemical attraction and electrical action may be different exhibitions of the same property of matter, and that oxygen and inflammable bodies are in relations of attraction which correspond to the function of being negative and positive respectively, it would follow that the attractions of acids for salifiable bases would be inversely as the quantity of oxygen that they contain; and supposing the power of attraction to be measured by the quantity of basis which an acid dissolves, it would be easy to infer the quantities of oxygen and metallic matter from the quantities of acid and of basis in a neutral salt. On this idea I had early in 1808 concluded that barytes must contain least oxygen of all the earths, and that the order as to the quantity of inflammable matter must be strontites, potash, soda, lime, and so on; and that silex must contain the largest quantity of oxygen of all.

If the most accurate analyses be taken, barytes may be conceived to contain about 90·5* of metal per cent. strontites 86†, lime 73·5*, ma[illegible] 66‡.

The same proportion wo[illegible]llow from an application of Mr. Dalton's ingenious su[illegible]ition§, that the proportion of oxygen is the same in all [illegible]oxides, and that the quantity of acid is the same in [illegible]utral salts, *i. e.* that every neutral salt is composed o[illegible] particle of metal, one of oxygen, and one of acid.

We are in possession [illegible]curate experiments on the quantity of acids required [illegible]issolve alumine, glucine, and silex; but according [illegible]chter's estimation of the composition|| of phosphat[illegible] alumine, alumine would appear to contain about 56 [illegible]nt. of metallic matter.

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* Mr. James Thompson, Nicho[illegible]urnal, 1809, p. 175, and Berthier.

† Mr. Clayfield. Thomson's [illegible], vol. ii. p. 626, 629.

‡ Murray's Chemistry, vol. iii. [illegible]

§ The principle that I have stat[illegible]e affinity of an acid for a salifiable [illegible] the quantity of oxygen contained by the basis, th[illegible]parison of the electrical relations of the earths, with their [illegible] in its numerical applications, must be considered merely as a consequence of Mr. Dalton's law of general proportions. Mr. Dalton had in the spring of 1808, communicated to me a series of proportions for the alkalies and alkaline earths; which, in the case of the alkalies, were not very remote from what I had ascertained by direct experiments. M. Gay Lussac's principle, that the quantity of acid in metallic salts is directly as the quantity of oxygen, might (as far as it is correct) be inferred from Mr Dalton's law, though this ingenious chemist states that he was led to it by different considerations. According to Mr. Dalton, there is a proportion of oxygen, the same in all protoxides, and there is a proportion of acid, the same in all neutral salts; and new proportions of oxygen and of acid are always multiples of these proportions. So that if a protoxide in becoming a deutoxide takes up more acid, it will be at least double the quantity, and in these cases the oxygen will be strictly as the acid. Mr. Dalton's law even provides for cases to which M. Gay Lussac's will not apply, a deutoxide may combine with a single quantity of acid, or protoxide with a double quantity. Thus in the insoluble oxysulphat of iron perfectly formed, (as some experiments which I have lately made seem to show) there is probably only a single proportion of acid; and in the super-tartrite of potash there is only a single quantity of oxygen, and a double quantity of acid. Whether Mr. Dalton's law will apply to *all cases*, is a question which I shall not in this place attempt to discuss.

|| Thomson's Chemistry, vol. ii. p. 531.

M. Berzelius*, in a letter which I received from him a few months ago, states, that in making an analysis of cast iron, he found that it contained the metal of silex, and that this metal in being oxidated took up nearly half its weight of oxygen.

If the composition of ammonia be calculated upon, according to the principle above stated, it ought to consist of 53 of metallic matter, and about 47† of oxygen, which agrees very nearly with the quantity of hydrogen and ammonia produced from the amalgam.

Though the early chemists considered the earths and the metallic oxides as belonging to the same class of bodies, and the earths as calces which they had not found the means of combining with phlogiston, and though Lavoisier insisted upon this analogy with his usual sagacity, yet still the alkalies, earths, and oxides have been generally considered as separate natural orders. The earths, it has been said, are not precipitated by the triple prussiates, or by the solutions of galls ‡; and the alkalies and alkaline earths are both distinguished by their solubility in water; but if such characters be admitted as grounds of distinct classification, the common metals must be arranged under many different divisions; and the more the subject is inquired into, the more distinct will the general relations of all metallic substances appear. The alkalies and alkaline earths combine with prussic acid, and form compounds of different degrees of solubility; and solutions of barytes (as has been shown by Dr. Henry and M. Guyton) precipitate the triple prussiate of potash; the power of combination is general, but the compounds formed are soluble in different degrees in water. The case is analogous with solutions of galls; these, as I have mentioned in a paper published in the Philosophical Transactions for 1805, are precipitated by almost all neutrosaline solutions; and they form compounds more or less soluble in water, more or less coloured, and differently coloured with all salifiable bases. It is needless to dwell upon the combinations of the alkalies and earths, with oils, to form soaps; and of the earthy soaps, some are equally insoluble with the metallic soaps. The oxide of tin, and other oxides abounding in oxygen, approach very near in their general characters to zircon, silex, and alumine; and in habits of amalgamation, and of alloy, how near do the metals of the alkalies approach to the lightest class of oxidable metals!

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* In the same communication this able chemist informed me, that he had succeeded in decomposing the earths, by igniting them strongly with iron and charcoal.

† I take the proportions of the volumes from the very curious paper of M. Gay Lussac, on the combinations of gaseous bodies, *Mem. d'Arcueil*, tom. ii. page 213, and the weights from my own estimation, according to which 100 cubic inches of muriatic acid gas weigh 39 grains, at the mean temperature and pressure, which is very nearly the same as the weight given by MM. Gay Lussac and Thenard.

‡ *Klaproth*, *Annales de Chimie*, tome x. p. 277.

It will be unnecessary, I trust, to pursue these analogies any further, and I shall conclude this section by a few remarks on the alloys of the metals of the common earths.

It is probable that these alloys may be formed in many metallurgical operations, and that small quantities of them may influence materially the properties of the compound in which they exist.

In the conversion of cast into malleable iron, by the process of blooming, a considerable quantity of glass separates, which, as far as I have been able to determine, from a coarse examination, is principally silex, alumine, and lime, vitrified with oxide of iron.

Cast iron from a particular spot will make only cold-short iron; whilst, from another spot, it will make hot-short; but by a combination of the two in due proportions, good iron is produced; may not this be owing to the circumstance of their containing different metals of the earths, which in compound alloy may be more oxidable than in simple alloys, and may be more easily separated by combustion?

Copper, M. Berzelius informs me, is hardened by silicium. In some experiments that I made on the action of potassium and iron on silex, the iron, as I have mentioned before, was rendered white, and very hard and brittle, but it did not seem to be more oxidable. Researches upon this subject do not appear unworthy of pursuit, and they may possibly tend to improve some of our most important manufactures, and give new instruments to the useful arts.

V. *Some Considerations of Theory illustrated by new Facts.*

Hydrogen is the body which combines with the largest proportion of oxygen, and yet it forms with it a neutral compound. This, on the hypothesis of electrical energy, would show that it must be much more highly positive than any other substance; and therefore, if it be an oxide, it is not likely that it should be deprived of oxygen by any simple chemical attractions. The fact of its forming a substance approaching to an acid in its nature, wh bined with a metallic substance, tellurium, is opposed to the idea of its being a gaseous metal, and perhaps to the idea that it is simple, or that it exists in its common form in the amalgam of ammonium. The phænomena presented by sulphuretted hydrogen are of the same kind, and lead to similar conclusions.

Muriatic acid gas, as I have shown, and as is further proved by the researches of MM. Gay Lussac and Thenard, is a compound of a body unknown in a separate state, and water. The water, I believe, cannot be decompounded, unless a new combination is formed: thus it is not changed by charcoal ignited in the gas by Voltaic electricity; but it is decompounded by all the metals; and in these cases hydrogen is elicited, in a manner similar to that in which one metal is precipitated by another; the oxygen being found in the new compound. This, at first view, might be supposed in favour of the idea that hydrogen is a simple substance; but the same reasoning may be applied to a protoxide as to a metal; and in the case of the nitromuriatic acid, when the nitrous acid is decomposed to assist in the formation of a metallic muriate, the body disengaged (nitrous gas) is known to be in a high state of oxygenation.

That nitrogen is not a metal in the form of gas, is almost demonstrated by the nature of the fusible substance from ammonia, and (even supposing no reference to be made to the experiments detailed in this paper) the general analogy of chemistry would lead to the notion of its being compounded.

Should it be established by future researches that hydrogen is a protoxide of ammonium, ammonia a deutoxide, and nitrogen a tritoxide of the same metal, the theory of chemistry would attain a happy simplicity, and the existing arrangements would harmonize with all the new facts. The class of pure inflammable bases would be *metals* capable of alloying with each other, and of combining with protoxides. Some of the bases would be known only in combination, those of sulphur, phosphorus *, and of the boracic, fluoric, and muriatic acids; but the relations of their compounds would lead to the suspicion of their being metallic. The salifiable bases might be considered either as protoxides, deutoxides, or tritoxides: and the general relations of salifiable matter, to acid matter, might be supposed capable of being ascertained by their relations to oxygen, or by the peculiar state of their electrical energy.

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* The electrization of sulphur and phosphorus goes far to prove that they contain combined hydrogen. From the phænomena of the action of potassium upon them in my first experiments, I conceived that they contained oxygen, though, as I have stated in the appendix to the last Bakerian lecture, the effects may be explained on a different supposition. The vividness of the ignition in the process appeared an evidence in favour of their containing oxygen, till I discovered that similar phænomena were produced by the combination of arsenic and tellurium with potassium. In some late experiments on the action of potassium on sulphur and phosphorus, and on sulphuretted

The whole tenour of the antiphlogistic doctrines necessarily points to such an order; but in considering the facts under other points of view, solutions may be found, which, if not so simple, account for the phænomena with at least equal facility.

If hydrogen, according t[illegible] ypothesis to which I have often referred, be consider[illegible] the principle which gives inflammability, and as the c[illegible] of metallization, then our list of simple substances w[illegible] clude oxygen, hydrogen, and unknown bases only; [illegible] s and inflammable solids will be compounds of th[illegible] ses, with hydrogen; the earths, the fixed alkalies, r[illegible] oxides, and the common acids, will be compounds [illegible] ame bases, with water.

The strongest argumen[illegible] avour of this notion, in addition to those I have [illegible] stated, which at present occur to me, are: First, [illegible] perties which seem to be inherent in certain bodies, [illegible] hich are either developed or concealed, according to [illegible] ure of their combinations. Thus sulphur, when it is [illegible] d in water either in combination with hydrogen [illegible] gen, uniformly manifests acid properties: and the [illegible] uantity of sulphur, whether in combination with hydrogen, whether in its simple form, or in combination with one proportion of oxygen, or a double proportion, from my experiments seems to combine with the same quantity of alkali. Tellurium, whether in the state of oxide or of hydruret, seems to have the same tendency of combination with alkali; and the alkaline metals, and the acidifiable bases, act with the greatest energy on each other.

[illegible] phosphuretted hydrogen, I find that the [illegible] cording to the circumstances of the experi[illegible] have obtained a larger volume of gas from [illegible] sed to the action of certain of these bodies, [illegible] These experiments are still in progress, [illegible] of them before the Society. The idea of the [illegible] and phosphorus is however still supported [illegible] being nonconductors of electricity is one ar[illegible] otassium and sodium I find when heated in [illegible] quantity of atmospheric air, absorb both [illegible] me nonconducting inflammable bodies ana[illegible] ances.

Second, The facility with which metallic substances are revived, in cases in which hydrogen is present. I placed two platina wires, positively and negatively electrified from 500 double plates of six inches, in fused litharge; there was an effervescence at the positive side, and a black matter separated at the negative side, but no lead was produced; though when litharge moistened with water was employed, or a solution of lead, the metal rapidly formed: the difference of conducting power may be supposed to produce some difference of effect, yet the experiment is favourable to the idea, that the presence of hydrogen is essential to the production of the metal.

Third, Oxygen and hydrogen are bodies that in all cases seem to neutralize each other, and therefore in the products of combustion it might be expected that the natural energies of the bases would be most distinctly displayed, which is the case; and in oxymuriatic acid, the acid energy seems to be blunted by oxygen, and is restored by the addition of hydrogen.

In the action of potassium and sodium upon ammonia, though the quantity of hydrogen evolved in my experiments is not exactly the same as that produced by their action upon water; yet it is probable that this is caused by the imperfection of the process*; and supposing potassium and sodium to produce the same quantity of hydrogen from ammonia and water, the circumstance, at first view, may be conceived favourable to the notion that they contain hydrogen, which under common circumstances of combination will be repellent to matter of the same kind: but this is a superficial consideration of the subject, and the conclusion cannot be admitted; for on the idea that in compounds containing gaseous matter, and perhaps compounds in general, the elements are combined in uniform proportions; then whenever bodies known to contain hydrogen are decomposed by a metal, the quantities of hydrogen ought to be the same, or multiples of each other. Thus, in the decomposition of ammonia by potassium and sodium, two of hydrogen and one of nitrogen remain in combination, and one of hydrogen is given off; and in the action of water on potassium to form potash, the same quantity of hydrogen ought to be expelled. From my analysis* of sulphuretted hydrogen, it would appear, that if potassium in forming a combination with this substance sets free hydrogen, it will be nearly the same quantity as it would cause to be evolved from water. And if the analysis of Mr. Proust and Mr. Hatchett, of the sulphuret of iron, be made a basis of calculation, iron, in attracting sulphur from sulphuretted hydrogen, will liberate the same proportion of hydrogen as during its solution in diluted sulphuric acid; and taking Mr. Dalton's law of proportion, the case will be similar with resp other metals: and if such reasoning were to be ado that metals are proved to be compounds of hydroge use, in acting upon different combinations cont hydrogen, they produce the evolution of equal prop of this gas, then it might be proved that almost any of matter is contained in any other. The same qu f potash, in acting upon either muriate, sulphate, te of magnesia, will precipitate equal quantities o esia; but it would be absurd to infer from this, th h contained magnesia, as one of its elements; the of repelling one kind of matter, and of attracting kind, must be equally definite, and governed by th circumstances.

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* There seems to be always the same proportion between the quantity of ammonia which disappears, and the quantity of hydrogen evolved; *i. e.* whenever the metals of the alkalies act upon ammonia, supposing this body to be composed of three hydrogen, and one of nitrogen, in volume, two of hydrogen and one of nitrogen remain in combination, and one of hydrogen is set free. And it may be adduced as a strong argument in favour of the theory of definite proportions, that the quantity of the metals of the alkalies and nitrogen, in the fusible results, are in the same proportions as those in which they exist in the alkaline nitrates.

Potassium, sodium, ir ury, and all metals that I have experimented upon, in acting upon muriatic acid gas, evolve the same quantity of hydrogen, and all form dry muriates; so that any theory of metallization, applicable to potash and soda, must likewise apply to the common metallic oxides. If we assume the existence of water in the potash, formed in muriatic acid gas, we must likewise infer its existence in the oxides of iron and mercury, produced in similar operations.

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* The composition may be d om the experiments in the last Bakerian lecture, which show th ins a volume of hydrogen equal to its own. If its specific gravity be taken as 35 grains, for 100 cubical inches, then it will consist of 2·27 of hydrogen, and 32·73 of sulphur. When sulphuretted hydrogen is decomposed by common electricity, in very refined experiments, there is a slight diminution of volume, and the precipitated sulphur has a whitish tint, and probably contains a minute quantity of hydrogen. When it is decomposed by Voltaic sparks, the sulphur is precipitated in its common form, and there is no change of volume; in the last case the sulphur is probably ignited at the moment of its production. In some experiments lately made in the laboratory of the Royal Institution, on arseniuretted and phosphuretted hydrogen, it was found that when these gases were decomposed by electricity, there was no change in their volumes; but neither the arsenic nor the phosphorus seemed to be thrown down in their common state; the phosphorus was dark-coloured, and the arsenic appeared as a brown powder, both were probably hydrurets: this is confirmed likewise by the action of potassium upon aseniuretted and phosphuretted hydrogen; when the metal is in smaller quantity than is sufficient to decompose the whole of the gases, there is always an expansion of volume; so that arseniuretted and phosphuretted hydrogen contain in equal volumes, more hydrogen than sulphuretted hydrogen, probably half as much more, or twice as much more. From some experiments made on the weights of phosphuretted and arseniuretted hydrogen, it would appear that 100 cubic inches of the first weigh about 10 grains, at the mean temperature and pressure, and 100 of the second about 15 grains.

The solution of the general question concerning the presence of hydrogen in all inflammable bodies, will undoubtedly be influenced by the decision upon the nature of the amalgam from ammonia, and a matter of so much importance ought not to be hastily decided upon. The difficulty of finding any multiple of the quantity of oxygen, which may be supposed to exist in hydrogen, that might be applied to explain the composition of nitrogen from the same basis, is undoubtedly against the simplest view of the subject. But still the phlogistic explanation, that the metal of ammonia is merely a compound of hydrogen and nitrogen; or that a substance which is metallic can be composed from substances not in their own nature metallic, is equally opposed to the general tenour of our chemical reasonings.

I shall not at present occupy the time of the Society by entering any further into these discussions; hypothesis can scarcely be considered as of any value, except as leading to new experiments; and the objects in the novel field of electrochemical research have not been sufficiently examined to enable to decide upon their nature, and their relations, or to form any general theory concerning them which is likely to be permanent.

Explanation of the Figures.

Fig. 1. The apparatus for electrizing potassium in gases. A the glass tube. B the wire negatively electrified. C and D the cup and wire positively electrified.

Fig. 2. The apparatus for decomposing water, out of the contact of air, page 20. AA the cones containing the water. BBB the tubes for conveying the gas. C and D the pneumatic apparatus.

Fig. 3. The apparatus for decomposing and recomposing water under oil. CC the wires for communicating the Voltaic electricity. DD the wires for producing the explosion. B the tube. A the vessel containing it. *a, d, c,* the level of the different fluids.

Fig. 4. The apparatus for exposing water to the action

of ignited potash and charcoal, out of the contact of air. A the tube for water. B the iron tube. C the receiver for the ammonia. D the pneumatic apparatus.

Fig. 5. The apparatus for the decomposition of ammonia.

Fig. 6. A Voltaic apparatus, being one of the 200 which compose the new Voltaic battery of the Royal Institution. For the construction of this battery, and of other instruments applicable to new researches, a fund of upwards of £1000 has been raised by subscription, from members of the Royal Institution. As yet, the whole combination has not been put into action; but reasoning from the effects of that part of it which en used, some important phænomena may be exp om so great an accumulation of electrical power.

XVI. *Report of the D o-Pock Institution, under the Patronage of His he Lord Lieutenant, for* 1809.

An Abstract from the R f Inoculations and Distribution of Matter.

	Patients Inoculated	ts issued ctioners neral.	Packets to Army Surgeons.
1804	578	776	236
1805	1,032	1,124	178
1806	1,356	1,340	220
1807	2,156	1,790	320
1808	3,002	2,285	333
1809	3,911	2,540	244
Totals.	12,065	9,855	1,531

The directors of the institution have great pleasure in bserving the progressive increase of vaccine inoculation, nd the influence of experience in satisfying the public of ts efficacy. Most of the above 12,065 patients being conined to a city where small-pox has been in general prevaent, must have been exposed in every possible way to its nfection, by living in the same house, or frequently sleepng in the same bed with the infected. The anxiety of arents, too, has often led them intentionally to expose their children to small-pox infection. As far, however, as the immediate observation of the Institution extends, cow-pock has been found to resist all such trials, with three exceptions only.

It now appears by increasing experience, that in a very few instances the vaccine infection will form fairly on the arm, and go through its regular stages, without being absorbed into the blood. The same thing has repeatedly happened in inoculating for the small-pox, where no eruptive fever or eruption succeeded the inoculation. In the three cases of small-pox which have succeeded vaccination, the disease has been mild and of short duration.

The efficacy of cow-pock, as far as Dublin is concerned, does not rest upon the proofs adduced in its favour by this Institution, for it has been extensively practised during the last five or six years. There are grounds for believing that the number vaccinated throughout the city, including the above 12,065, does not fall short of 35,000. The cases of small-pox following cow-pock which have been reported, upon any reasonable authority, to the Institution, do not exceed six. No one who is acquainted with the careless and inattentive manner in which many practitioners have hitherto conducted vaccination, can be surprised to hear of cases of failure. The neglect of parents also to have their children examined at the regular periods after inoculation, tends to bring the practice into disrepute. To obviate this inconvenience, it has been the practice for some time at this Institution, to oblige parents to deposit a small sum, to be returned after the child has gone through the disease, provided they have attended agreeably to instruction; otherwise the sum is forfeited. This regulation has had the desired effect.

It was reported at an early period of the practice, that vaccination afforded only a temporary security, which was at first limited to three years. Numerous experiments, tried in different quarters, satisfactorily proved the falsehood of this assertion. A similar opinion has been lately revived, but the period of security extended to five or six years. Neither analogy nor experience justifies such an idea, and the history of casual cow-pock fully refutes the allegation, as numerous cases are on record of persons, after having casual cow-pock, resisting during a long life the small-pox, under every circumstance of exposure, inoculation, &c. Besides, had the preventive powers of cow-pock not been permanent, it is but reasonable to suppose that many of the above 12,06[illegible] must, under the existing circumstances of exposure, have taken the small-pox. Above twenty children who were vaccinated five or six years ago, have [illegible] order of the directors, been submitted to variolous inoculation, but without the effect of producing small-pox. [illegible] experiments have been instituted, under the [illegible] other practitioners, with the like result. Nineteen [illegible] who had the cow-pock eight and nine years [illegible] inoculated with small-pox matter, at [illegible] Hospital, under the inspection of Mr. Stewart, [illegible] and Mr. Creighton, surgeon of the hos-[illegible] no other effect than local inflammation.—[illegible] received [illegible]. Bryce, of Edinburgh, he [illegible] have [illegible]ed an experiment of ino-[illegible] with the small-pox, who [illegible] from [illegible]s to five months.—The re-[illegible]hows clearly that a pustule [illegible] is as readily produced five [illegible] the end of eight years, con-[illegible] the secu[illegible] complete at the latter pe-[illegible]."

[illegible] extract [illegible]e Report of the Small-Pox [illegible] shows [illegible]recorded:—"Eleven thou-[illegible] upwards, have been vac-[illegible]y-five hundred were after-[illegible] the natural small-pox, by [illegible] with small-pox matter, [illegible]er amply sufficient to sa-[illegible]ecurity and success of the [illegible]."—December, 1802.—So great [illegible] of variolous inoculation, [illegible] small-pox infection with-[illegible] every reasonable mind [illegible]—[illegible] Mr. Charles Murray's [illegible]

[illegible] circulated, that Dr. Jenner [illegible] entertain some doubt of the [illegible] directors thought it expedient [illegible] to him, and to lay his an-[illegible] the public.

[illegible] letter of the 3d instant, in-[illegible] the Cow-Pock Institution, in [illegible]. The former letter you allude [illegible]. It is with the greatest plea-[illegible] increase of vaccination in your metropolis, and the uninterrupted success that has attended the practice, at once a proof of the zeal, industry and attention of the medical officers; for which I beg leave to make my most grateful acknowledgements.

"And now, sir, a few remarks on the very extraordinary communication you have make to me respecting Lady C——. It has been one of the usual devices of the enemies of vaccination, almost from the time of my first making it known, to represent me as having lost my confidence of its prophylactic powers, or, at least, that I was wavering on the subject. Can I, who, with the aid of my nephews, have vaccinated a number of persons little short of 30,000, without one single instance of accident or of failure, that ever reached my ears, for a moment entertain such an absurd idea? Or could I have ever thought of inoculating for the small-pox, while I hold that practice in abhorrence, and condemn it both publicly and privately? Believe me, the whole story you relate to me is an entire fiction, without the faintest shadow of foundation. Never from the commencement of my experiments to the present hour, have I used a particle of variolous matter, except for the purpose of putting some of those to a test on whom I made my first trials. For some years past, I have relied wholly on the vaccine lymph, for testing those on whom any material irregularity appeared in the progress of the pustule.

"Believe me, &c.

Berkeley, Feb. 19, 1809. "EDWARD JENNER."

While the directors, with such weight of evidence in its favour, feel themselves warranted in continuing to recommend vaccination as a preventive of small-pox, they cannot but regret that in a few cases it has been difficult to determine whether a patient has had the disease constitutionally or locally. They however confidently hope that by pursuing Mr. Bryce's test, and by increased attention to the progress of the disease, practitioners will be enabled to surmount the only objection to a practice which tends to preserve more than 30,000 lives annually, in the British Isles.

Mr. Bryce proposes that a second inoculation be performed about the sixth day after the first: the vesicle produced by this second inoculation is accelerated in its progress, so as to arrive at maturity, and again fade, at nearly the same time as the affection arising from the first inoculation. Mr. B. considers the acceleration of the second inoculation to be the effect of the constitutional affection produced by the first; and therefore, if it shall be found that no such acceleration takes place, but that the second inoculation proceeds by a slow progress through all the stages, it is to be concluded, that no constitutional action has taken place from the first insertion of the virus; and when this is the case, the second inoculation must be regarded as a primary affection, and a third puncture made according to the plan laid down for conducting the second inoculation; and thus (he says) we may go on until the proper test be obtained; or until we be satisfied that the constitution completely resists the action of cow-pock.

Although small-pox is b eans exterminated from
Dublin, among the poor, e general substitution of
vaccine for variolous inc n has considerably di-
minished the number of p brought to the hospitals
and dispensaries for advice. he upper ranks of society
death from small-pox is u of, and the most exten-
sive practitioners acknowl it a case of small-pox in
private practice is a very urrence. And although
the reintroduction of sn into society would add
greatly to the emolumer of physic and surgery,
there is no liberal man in rofession who would not
sincerely deplore such a c

Si order,

January 10, 1810. . B. LABATT, Secretary.

1 Hospital, Dublin, Jan. 4, 1810.

The following Report having been laid before the Governors of the Foundling Hospital, and appearing to be highly satisfactory:—Ordered, That three thousand copies thereof be printed, for the purpose of their being circulated as generally throughout the United Kingdom as possible.

By order,

A. BAILIE, Register.

AS some persons have lately attempted to prejudice the minds of the public by representing vaccine inoculation as a doubtful security against small-pox, limiting its influence to a certain period, and wishing us to believe that its preventive powers diminish in proportion to the distance of time from inoculation;—I have, therefore, at the request of the right honourable and honourable the governors of the Foundling Hospital, instituted such experiments as enable me (a second time) to congratulate the public on their successful event.

From my situation, as surgeon to the Foundling Hospital, I have had it fully in my power to select such cases as had been faithfully recorded by me to have undergone vaccination at the earliest period of cow-pock inoculation in this city, and such have been approved of by those gentlemen who have honoured me with their presence, to witness and subscribe their names to the progress and event of the following experiment on nineteen children chosen for the purpose, who were divided into two classes. The first nine comprehend those who in a state of infancy were vaccinated by me between the 30th of December 1800 and 3d of July 1801, now more than eight years. These were again inoculated with small-pox infection by George Stewart, esq. surgeon-general, on the 24th of July, 1804, (and witnessed by several gentlemen of the first respectability in their profession,) in like manner to disprove the assertions of Mr. Goldson, as may be seen in the twelfth volume of the Medical and Physical Journal, and with the most complete success—all having resisted the small-pox, although exposed to it in every way possible. These nine children, with ten others, who were also vaccinated by me in a state of infancy, from 15th of July 1801 to 30th of August 1802, upwards of seven years, were again submitted to small-pox inoculation, on Friday, 22d of December last; the infection taken from a child of Mr. Stafford's, No. 7, Hanbury-lane, in confluent small-pox, and the matter inserted in two places in the arm of each child, in a fluid state, and in the greatest quantity. In every instance, the punctures in the arm of each child from the third day inflamed, and continued until the seventh, when the inflammation gradually subsided, as certified by Mr. Stewart, and marked in a table, which, in another publication, will be more fully expressed;—which circumstance has proved the activity of the small-pox matter inserted, and which must have affected the constitution, were it in the least susceptible of the disease. Fourteen days have now elapsed, the inflammation of the punctures is entirely gone, and never was attended with the slightest fever, sickness, or eruption.

In corroboration of the above facts, conducted with every degree of accuracy, and which cannot admit of the smallest doubt on the minds of those gentlemen who have witnessed them, and hereunto subscribed their names; I can safely assert, that I have submitted upwards of five hundred infants and children, vaccinated by me at this Institution, and at the Dispensary for Infant Poor and Cowock Inoculation, as established in the year 1800, to a like xperiment, and with the same result in every instance.

Dublin, Merrion-square, West, January 4, 1810.
J. Creighton.

George Stewart,
Gustavus Hume,
S. Wilmot,
Ralph S. Obre,
A. Colles,
William Hartigan,
Philip Crampton,
Members of the Royal College of Surgeons in Ireland.

Edmund Connell,
Samuel Bell,
William Dillon,
James M'Creight,
Ap s.

XVII. *Information, that* *ter Publication of the late Mr. Smeaton's Engine* *esigns and Papers is in hand.—Copy of a List* *principal British Strata, by the late Rev. Joh* *l, (of whose posthumous Papers on Geological* *, further Information is requested;)—with som* *iments of Mr. Smeaton's on Limestones,—and* *respecting Mr. Tofield. Communicated by Mr.* AREY.

To *och.*

Sir, As my eldest son w w days ago employed, in xamining the miscellane lles of papers, which belonged to the late ingeni John Smeaton, the civil ngineer, now in Sir Jose nks's possession, with a view to the further publication by Messrs. Longman, Hurst, Rees, and Co. of his Drawings and Reports on civil engineery, which so long and impatiently have been expected by those nterested in this branch of the useful arts, he found a small scrap of paper (only four inches by three) in the handwriting of Mr. Smeaton, part of the cover of a letter, as appears by part of a seal and the London post-mark of November 21, 1788, on the back of it, which, having obtained Sir Joseph's permission, I think of sufficient importance, in a geological point of view, to request the favour of you to lay before your readers.

It relates to the order and thicknesses of the *strata* in England, as appears by Mr. Smeaton's title or endorsement on it, viz. "Mr. Michel's account of the south of England strata," which is as follows, viz.

	Yards
"Chalk	120
Gelt	50

Sand,

	Yards
Sand, of Bedfordshire	10 or 20
Northampton lime and Portland limes lying in several strata	100
Lyas strata	70 or 100
Sand, of Newark	about 30
Red clay, of Tuxford and several	100
Sherewood Forest, pebbles and gravel	50 unequal
Very fine white sand	uncertain
Roch Abbey and Brotherton limes	100
Coal strata, of Yorkshire	„

The Mr. Michel alluded to, was, it appears, the late Rev. John Michel, rector of Thornhill, near Wakefield, Yorkshire, who was an intimate friend of Mr. Smeaton, the late Mr. Cavendish, &c. &c., and whose name must be very familiar to most of your readers, from his many valuable papers in the Transactions of the Royal Society of London, of which he was a member.

This account of the strata, imperfect as it is, appears to me important, as showing, that Mr. Michel was acquainted with the principal features of the south of England strata, at an earlier period than any thing was published on the subject, especially if we suppose, as is most reasonable, that this communication was made verbally by Mr. Michel to his friend Mr. Smeaton, very soon after November 1788, who took it down on the cover of a recent letter, as being the only piece of paper then at hand; for Mr. Smeaton's decease in September 1792, shows that it must have been prior to that time.

It appears to me probable, that this account was principally made from the result of Mr. Michel's observations, in his journeyings by the great North road between the place of his residence and London; The "chalk" being that which appears from near Hatfield to Baldock; the "golt" being the chalk-marle (and perhaps some alluvial clays also) thence to near Sandy in Bedfordshire; where, doubtless, the "sand" is situate, to which he alludes. In crossing Northamptonshire from Wansford to Stamford, the "limes" are first noticed, which he rightly associates with, and considers the same as, those of Portland-Island, though distant 170 miles therefrom in a straight line! The next are the "lyas" strata, which appear between Grantham and Balderton; and here, the use of a term for these strata of limestone, which was not then known or in use, I believe, nearer than Gloucestershire or Somersetshire, shows again that

hat Mr. Michel had contemplated the identity of the Briish strata over wide spaces*. The "sand" of Newark is seen on its S.E. side near Balderton: the "red clay" of Tuxford is noticed as the produce of "several" other places, and is the gypseous earth, or red marle, which forms so conspicuous a figure across a large portion of the middle and western parts of England. Sherwood Forest "pebbles and gravel," over the northern skirt of which, this road passes between Tuxford and Doncaster, is noticed by Mr. Michel, as being "unequal" in thickness; and if his observations had been further extended, it would doubtless have appeared clear to him that the same ought not to have been taken into h of strata, any more than the numerous other patche *alluvium on the surface* which he must have pass this road, and has not noticed; and particularly s am right in conjecturing, that the "very fine white which he mentions, as of "uncertain" thickness, is ped, *as an accidental bed* in the "red clay," (whic ad before mentioned) similar to what we find at iton on the S. of Derby, and some few other places, occurrence is rather rare, I believe, and should the wish much to learn, the precise spot or pits to wl r. Michel here alludes; it being a part of the countr I have never visited.

The "lime" of Brothert ng associated with that of Roche-Abbey, 25 miles S of it, shows again, that Mr. Michel had discover at least of those geological principles, which the rs of Mr. *William Smith* very soon after tended to confirm, and to render them of the utmost practical use and importance.

Mr. Michel was also aware, that the coal-strata known in Yorkshire, are under-measures to the yellow lime, above mentioned: it must however be observed, that the *thicknesses* in the above list are most of them (except perhaps the chalk, the galt, and the Balderton sand) greatly underrated; while many very thick or important strata (of which I intend to give a short account in my Derbyshire Report) are omitted altogether; as the Bagshot-heath sand, the

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* The lyas or blue lias lime stone having been much the object of Mr. Smeaton's notice, on account of its important quality of making a durable morter which sets suddenly and very hard, even under sea-water, as he proved in the building of the Eddystone light-house, and others of his great works; and with whose appearance at Aberthaw and Watchet on the opposite shores of the Bristol Channel, and numerous other places, he was so well acquainted, was probably the reason, why so very laconic a mention of these strata is here made by Mr. Smeaton.

London clay, the Woolwich or Black-heath sand, the Aylesbury limestone, the Clunch clay*, the Bedford limestone and clays beneath it, the Barnack rag, and Colleyweston lime and slate, the Foston blue clay, and the Maidwell lime, all of which occur above the lias-clay; while the coal series above the yellow lime (under the Sherwood gravel as I suspect) and the important *blue beds* in the yellow limestone series, are unnoticed: enough however is contained in the above list, to show, that the late Rev. John Michel ought to be ranked among those, to whom geological science is indebted; and I take this method of addressing myself to those, who may be now in possession of his papers, to search for and communicate whatever details they may contain on the British strata, that will either further explain the above communication to Mr. Smeaton, or show the source, whence Mr. M. may have derived the above particulars of the South British strata†: which would be conferring a great obligation on

Your obedient humble servant,

JOHN FAREY, Sen.

12, Upper Crown Street, Westminster,
August 4, 1810.

XVIII. *An*

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* Between the Bedfordshire or Woburn sand, and the Northamptonshire limes or Bath freestone, which clay extends under almost all the Lincolnshire fens, and most of those in Cambridgeshire and in Yorkshire.

† P. S. Since writing the above I have been informed, that Mr. Michel, whose death happened April 21, 1793, was at an early part of his life keeper of the Woodwardian collection of fossils at Cambridge, which is thought by some to be the very best general geological collection in existence, though made near a century ago, owing to the great care and minuteness with which the *localities* and attendant circumstances of the fossils therein, are described: essential particulars, which yet have appeared beneath the attention of too many of our modern mineralogists and geologists, as it should seem. It is not improbable, that a comparison of the fossils and their localities, in this celebrated collection, first suggested the ideas of a determinate order in the British strata to Mr. Michel, and the examination of his papers is therefore a matter of the greater importance, from the probability, that some such arrangement of the facts in the Woodwardian catalogue, may be found among them. Perhaps also, the present keeper of the Woodwardian collection and papers will have the goodness to inform us, whether any such arrangement of the British strata in a series is to be found, or minutes of any such attempts, among the Woodwardian papers?

Another scrap of paper, found among Mr. Smeaton's loose memorandums, contains his experiments on twelve sorts of limestone, by dissolving 40 grains of each in aquafortis, and drying the clayey undissolved residuums in the sun, the weights of which are as follow, viz.

	Grains.
"Yellow lyas, of Axminster	5¼
Ditto with shining spangles (mica probably) ..	5½
Yellow snake-stone, of Glastonbury	5
Blue lyas, of Watchet	4½
Ditto of Aberthaw	4½
Ditto of Bath	4¼
Ditto of Axminster	3½
Yellow clump-stone, of Sherborne	3

White

XVIII. *An Analysis of several Varieties of British and Foreign Salt, (Muriate of Soda,) with a view to explain their Fitness for different œconomical Purposes. By* WILLIAM HENRY, *M.D. F.R.S. Vice-Pres. of the Literary and Philosophical Society, and Physician to the Infirmary at Manchester**.

SECT. I. *General Observations.*

IN undertaking the series of experiments described in the following pages, I had not so much in view the discovery of novelties in science, as the determination, by the careful
employment of known pr   and by the improvement
of methods of analysis, of   ber of facts, the establish-
ment of which (it appeare   probable) might have an
influence on an important   of national revenue and
industry.

An opinion has for sor   past existed, and I believe
has been pretty general b   his and other countries, to
the disadvantage of Brit   as a preserver of animal
food; and a decided prefe   has been given to the salt
procured from France, S   ortugal, and other warm
climates, where it is pre   the spontaneous evapora-
tion of sea water. In co   y with this opinion, large
sums of money are ann   d to foreign nations, for
the supply of an article, v   reat Britain possesses, be-
yond almost any other c   n Europe, the means of
drawing from her own   resources. It becomes, therefore, of much consequence to ascertain, whether this preference of foreign salt be founded on accurate experience, or be merely a matter of prejudice; and, in the

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				Grains.
White lyas, with shining spangles, of Wells	..		..	1¼
Brown limestone, of Plymouth	..	..	..	1
———— of Chudley (Chidgley?)	..		..	0¼

Forty grains of burnt lime in flower, dissolved in aquafortis, left of clayey matter when dried in the sun, as follows, viz.

Blue lyas, of Watchet	..	..	..	4¼
———— of Briddistow	..	..	..	3¼

The Watchet (residuum) made into a ball just stuck together, the Briddistow scarcely."

When I was at the house of Mr. Jessop the engineer (who was formerly a pupil and assistant of Mr. Smeaton) at Butterley in Derbyshire, he mentioned that a Mr. *Tofield*, a civil engineer of the southern part of Yorkshire, formed a design 30 years ago, of investigating the British strata. I shall be thankful to any of your readers who can communicate any particulars of this undertaking, and of its author, if they will do so.

* From Philosophical Transactions for 1810, Part I.

former case, whether any chemical difference can be discovered, that may explain the superiority of the one to the other.

The comparative fitness of these varieties of salt for the curing of provisions, which has been a subject of much controversy among the parties who are interested, can be decided, it is obvious, in no other way, than by a careful examination of the evidence on both sides. Where evidence, however, is doubtful, and where there exists, as in this case, much contrariety of testimony, it cannot be unfair to yield our belief to that which best accords with the chemical and physical qualities of the substances in question. Again, if salt of British production should be proved to be really inferior in chemical purity to foreign salt, it would be important to ascertain, as the basis of all attempts towards its improvement, in what, precisely, this inferiority consists. It seemed desirable, also, to examine whether any differences of chemical composition exist among the several varieties of home-made salt, which can explain their variable fitness for œconomical purposes.

Such were the considerations that induced me to undertake an inquiry, which has occupied, for several months past, a large share of my leisure and attention. I began the investigation, wholly uninfluenced by any preconceived opinions on the subject; and I had no motive to see the facts in any other than their true light, since I have no personal interest, either directly or remotely, in the decision of the question.

The principal sources of the salt, which is manufactured in this country, are rock salt, brine springs, and sea water. The first material is confined entirely, and the second chiefly, though not wholly, to a particular district of Cheshire. Of the extent and boundaries of this district, the process of manufacture, and other circumstances interesting to the mineralogist as well as to the chemist, an ample and excellent history has been given by Mr. Henry Holland, in the agricultural report of the county of Chester*. From his account, I shall extract, in order to render some parts of this memoir more intelligible, a very brief statement of the characteristic differences of the several varieties of salt, which are prepared in Northwich and its neighbourhood.

In making the *stoved* or *lump salt*, the brine is brought to a boiling heat, which, in brine fully saturated, is 226°

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* Published in 1808.

of Fahrenheit. This temperature is continued during the whole process; and as the evaporation proceeds, small flaky crystals continue to form themselves, and to fall to the bottom of the boiler. At the end of from eight to twelve hours, the greatest part of the water of solution is found to be evaporated; so much only being left, as barely to cover the salt and the bottom of the pan. The salt is then removed into conical wicker baskets, termed *barrows;* and, after being well drained, is dried in stoves, where it sustains a loss of about one-seventh of its weight.

On the first application of heat to the brine, a quantity of carbonate of lime, and sometimes a little oxide of iron, both of which had been h [illegible] solution by an excess of carbonic acid, are separa [illegible] d are either removed by skimming, or are allowe [illegible] side to the bottom of the pan, along with the salt [illegible] med, and with some sulphate of lime; and are [illegible] ls raked out. These two operations are called *clear* [illegible] an. Some brines scarcely require them at all, and [illegible] only occasionally. The whole of the impurities, h [illegible] are not thus removed; for a part, subsiding to the b [illegible] orms a solid incrustation, termed by the workmen *p* [illegible]. The portion of this, which is lowest, acquires [illegible] induration and adhesion to the pan, that it is ne [illegible] o remove it, once every three or four weeks, by [illegible] blows with a pick-axe. These sediments are form [illegible] in making the other varieties of salt.

In preparing *common s* [illegible] brine is first raised to a boiling heat, with the double view of bringing it as quickly as possible to the point of saturation, and of clearing it from its earthy contents. The fires are then slackened, and the evaporation is carried on for 24 hours, with the brine heated to 160° or 170° Fahrenheit. The salt, thus formed, is in quadrangular pyramids or hoppers, which are close and hard in their texture. The remainder of the process is similar to that of making stoved salt, except that after being drained it is carried immediately to the storehouse, and not afterwards exposed to heat, an operation confined to the stoved salt.

The *large-grained flaky salt* is made with an evaporation conducted at the heat of 130 or 140 degrees. The salt thus formed is somewhat harder than common salt, and approaches more nearly to the cubic shape of the crystals of muriate of soda.

Large-grained or *fishery salt* is prepared from brine heated only to 100° or 110° Fahrenheit. No perceptible agitation, therefore, is produced in the brine, and the slowness of the process, which lasts from seven or eight to ten days, allows the muriate of soda to form in large, and nearly cubical crystals, seldom however quite perfect in their shape *.

For ordinary domestic uses, stoved salt is perfectly sufficient. Common salt is adapted to the *striking* and salting of provisions, which are not intended for sea voyages or warm climates. For the latter purposes, the large-grained or fishery salt is peculiarly fitted.

On the eastern and western coasts of Scotland, and especially on the shores of the Firth of Forth, large quantities of salt are made by the evaporation of sea water. In consequence of the cheapness of fuel, the process is carried on, from first to last, by artificial heat, at a temperature, I believe, equal or nearly so to the boiling point, and varying, therefore, according to the concentration of the brine. The kind of salt, chiefly formed in Scotland, approaches most nearly to the character of stoved salt. In some places a salt is prepared, termed *Sunday salt;* so called, in consequence of the fires being slackened between Saturday and Monday, which increases considerably the size of the crystals.

I am indebted to Dr. Thomson of Edinburgh, (who gave me his assistance with great zeal and alacrity) for an opportunity of examining upwards of twenty specimens of Scotch salt, prepared by different manufacturers. That distinguished chemist, it appears from a letter which he addressed to me on the subject, was some time ago engaged in experiments on Cheshire salt. The particulars he has lost; and he retains only a general recollection of the facts, which confirms, I am happy to state, the accuracy of the results obtained by my own experiments.

At Lymington in Hampshire, advantage is taken of the greater heat of the climate, to concentrate the sea water by spontaneous evaporation to about one-sixth its bulk, before admitting it into the boilers. One kind of salt is chiefly prepared there, which most nearly resembles in grain the stoved salt of Cheshire. The process varies a little, in some respects, from that which has been already described. The salt is not fished (as it is termed) out of the boiler, and drained in baskets; but the water is entirely evaporated, and the whole mass of salt taken out at once, every eight hours, and removed into troughs with holes in the bottom.

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* Cheshire Reports, p. 53, &c.

Through these it drains into pits made under ground, which eceive the liquor called *bittern* or *bitter liquor*. Under he troughs, and in a line with the holes, are fixed upright takes, on which a portion of salt that would otherwise ave escaped, crystallizes and forms, in the course of ten r twelve days, on each stake, a mass of sixty or eighty ounds. These lumps are called *salt cats*. They bear the roportion to the common salt, made from the same brine, f one ton to 100.

From the mother brine or bitter liquor, which has drained nto the pits, the sulphate of magnesia is made during the vinter season, when the manufacture of salt is suspended,
n consequence of the w he temperature required
or the spontaneous evapo the sea water. The pro-
ess is a very simple one*. bitter liquor from the pits
s boiled for some hours in s, which are used in sum-
ner to prepare common s d the impurities, which
ise to the surface, are by skimming. During
he evaporation, a portion non salt separates; and
his, as it is too impure fo reserved for the purpose
f concentrating the brin mmer. The evaporated
itter liquor is then remov wooden coolers eight feet
ong, five feet wide, and ot deep. In these it re-
nains twenty-four hours, which time, if the wea-
her prove clear and cold lphate of magnesia, or
Epsom salt, crystallizes ottom of the coolers, in
uantity equal to about th of the boiled liquor.
The uncrystallizable fluid i et off through plug-holes
t the bottom of the coolers; and the Epsom salt, after eing drained in baskets, is deposited in the store-house. This is termed *single* Epsom salts, and after solution and a econd crystallization, it acquires the name of *double Epsom* salts. Four or five tons of sulphate of magnesia are roduced from a quantity of brine, which has yielded 100 ons of common, and one ton of cat salt.

On the banks of the Mersey, near its junction with the

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* I am indebted for an account of this process, as well as of the method making common salt at Lymington, to the liberal communication of harles St. Barbe, esq. of that place. Though not strictly connected with e subject, I give his description of the mode of making Epsom salt, beuse no correct statement of the process has, I believe, been hitherto pubhed. The analysis of sea water, indeed, by a justly distinguished chemist Bergman), excludes, erroneously, the sulphate of magnesia from its comsition, and his results have led to the opinion, that to manufacture this lt on the large scale, requires the addition either of sulphuric acid, or of me sulphate to the *bitter liquor*. (See Aikin's Chemical Dictionary, ii. 8.)

Irish Channel, the water of that river before evaporation is brought to the state of a saturated brine, by the addition of rock salt. The advantage of this method of proceeding will be obvious when it is stated, that 100 tons of this brine yield at least 23 tons of common salt, whereas from the same quantity of sea water, with an equal expenditure of fuel, only two tons 17 cwt. of salt can be produced *.

Within the few past years, an attempt has been made to apply rock salt itself to the packing of provisions. For this purpose it is crushed to the proper size between iron rollers. The trials which have been made, I am informed, are but few, and the results hitherto are not perfectly known.

The *bay salt* imported from foreign countries is well known to be prepared by the spontaneous evaporation of sea water, which, for this purpose, is confined in shallow pits, and exposed to the full influence of the sun and air. I have no addition to make to the accounts of its manufacture, which have already been given by various writers †.

As the results of the investigation, which forms the subject of this memoir, may be acceptable to many persons who can scarcely be expected to take an interest in a long detail of analytical processes, I shall present, in the following section, a general view of the experiments, and of the conclusions that may be deduced from them. In the last place, in order that other chemists may be enabled to repeat the analyses under similar circumstances, I shall describe minutely the methods that were adopted, some of which are new, and others reduced to greater precision. If, however, in the future progress of science, it should appear that any of these processes are imperfect, it may still be admitted that, for all useful purposes, they afford a fair *comparison* of the composition of the several varieties of culinary salt; since the sources of fallacy, that may hereafter be discovered, must have been the same in every case, and have produced in each an error of nearly the same amount.

SECT. II. *General Statement of the Results of the Experiments, and Conclusions that may be deduced from them.*

A comparison of the component parts of British and foreign ɔreign salts, and of different varieties of British salt with ıch other, will best be made by an examination of the ɔllowing table, which comprehends the results of the ıalysis of equal weights of each variety.

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* See the Earl of Dundonald's "Thoughts on the Manufacture and Trade of Salt." London, 1785.

† Encyclop. Method. art. *Salins*. (Des Marais Salans) Aikin's Dictionary of Chemistry, ii. 224. Watson's Chemistry, vol. ii. p. 52. It is necessary to remark, that a great proportion of what is sold in London as bay-salt is Cheshire large-grained fishery salt.

Kind of Salt.	Insol. matter.	Muriate of lime.	Muriate of Magnesia	Total earthy muriates.	Sulph. of lime.	Sulph. of magnesia.	Total sulphates.	Total im purities.	Pure muriate of soda.
	1000 parts by weight consist of								
St. Ube's	9	a trace.	3	* 3	23½	4½	28	40	960
St. Martin's	12	do.	3½	* 3½	19	6	25	40½	959½
Oleron	10	do.	2	* 2	19½	4¼	23¾	35¾	964¼
Scotch (common)	4	—	[illegible]	28 or *	15	17½	32½	64½	935½
Scotch (Sunday)	1	—	[illegible]	11½	12	4½	16½	29	971
Lymington (com.	2	—	[illegible]	11 or*	15	35	50	63	937
Do. (cat)	1	—	[illegible]	5	1	5	6	12	988
Crushed rock	10	0·1/15	[illegible]	0¼	6½	—	6½	16½	983½
Fishery	1	0·¼	[illegible]	1	11¼	—	11¼	13¼	986¾
Common	1	0·¼	[illegible]	1	14½	—	14½	16½	983½
Stoved	1	0·¼	[illegible]	1	15½	—	15½	17½	982½

I. The *total amount of* [illegible] *ties*, and the *quantity of* *al muriate of soda*, conta[illegible] each variety of common lt, may be learned by insp[illegible] the two last columns of e table. From these it a[illegible] that the foreign bay salt purer, generally speaking, [illegible] salt which is prepared by e rapid evaporation of se[illegible]; but that it is contami-ıted with about three ti[illegible]e amount of impurities scoverable in an equal [illegible] of the Cheshire *large-rained* salt, and with mo[illegible] twice that of those that e found in the *stoved* and *common* salt of the same district.

II. The *insoluble matter* in the foreign salt, after the tion of boiling water, appears to be chiefly argillaceous rth coloured by oxide of iron, and is probably derived in ırt from the pits in which the sea water is submitted to aporation. We may, perhaps, assign the same origin to e very minute portion of muriate of lime, which is not und in the salt prepared by evaporating sea water in me-llic vessels, nor even in the mother liquor, or uncrystal-zable residue. In sea salt prepared by rapid evaporation, e insoluble portion is a mixture of carbonate of lime ith carbonate of magnesia, and a fine siliceous sand; and the salt prepared from Cheshire brine, it is almost en-ely carbonate of lime. The insoluble part of the less ıre pieces of rock salt is chiefly a marly earth, with some lphate of lime. The quantity of this impurity, as it is ıted in the table, is considerably below the average, which my experiments has varied from 10 to 45 parts in 1000.

Some estimate of its general proportion, when ascertained on a larger scale, may be formed from the fact that Government, in levying the duties, allows 65lb. to the bushel of rock salt, instead of 56lb., the usual weight of a bushel of salt.

III. The *earthy muriates*, and especially that with base of magnesia, abound most in salt which is prepared by the rapid evaporation of sea water. Now since common salt, in all its forms, contains, as will afterwards appear, very little water of crystallization, it is probable that the muriate of magnesia, discovered by the analysis of sea salt, is derived entirely from that portion of the mother liquor which adheres to the salt after being drained, and which amounts to about one-seventh of its weight. The larger the size of the grain, the less is the quantity of this solution which the salt holds suspended; and hence the salt prepared at a lower degree of heat, being in larger crystals, is less debased by the magnesian muriate, than the salt formed at a boiling temperature. It is probable, also, that when the salt is drawn at intervals from the boiler, the proportion of the earthy muriate will vary with the period of the evaporation at which it is removed. For it may readily be conceived, that as the proportion of the earthy muriates in any brine is increased by the separation of muriate of soda, the greater will be the quantity of the muriates which the crystals of common salt, formed in the midst of the brine, will retain; thence it follows, that, so far as the earthy muriates only are concerned, salt must diminish in purity as the process of evaporation advances.

In the several varieties of Cheshire salt, the earthy muriates do not exceed one thousandth part of this weight, and they are precisely (or so nearly so that the difference is not ascertainable) the same in all. This will cease to be matter of surprise, when it is considered that the salt obtained by evaporating to dryness the whole of a portion of Cheshire brine, does not give more than five parts of earthy muriates in 1000. In the entire salt of sea water, according to Bergman, the earthy muriates form no less than 213 parts in the same quantity.

According to the proportion in which the earthy muriates are present in any kind of salt, will be its power of deliquescence, or of attracting moisture from the atmosphere. It is not entirely, however, from the salts with earthy base that common salt derives this quality; for the most transparent specimens of rock salt, which I find to consist of absolutely pure muriate of soda, attract much moisture from a humid atmosphere.

IV. The *sulphate of magnesia* and the *sulphate of lime* both enter into the composition of all the varieties of salt prepared from sea water; but the sulphate of lime alone is found in Cheshire salt. The proportion of sulphate of magnesia is greatest in that variety of sea salt which has been formed by rapid evaporation. In foreign bay salt its quantity is very insignificant.

From the table it may be seen, that the proportion of sulphate of lime is greater in foreign bay salt than in any variety of British salt, even than in those which are prepared from sea water [illegible] oiling heat. The only explanation of this fact, t [illegible] rs to me, is, that during the rapid evaporation of [illegible] ater a considerable part of the calcareous sulphate i [illegible] pitated at an early stage of the process, and is partly [illegible] d in *clearing* the boiler, a process which can scarc [illegible] erformed during the formation of bay salt, in [illegible] ose sides are composed of moist clay. The remai [illegible] the selenite, thus precipitated by the rapid evapo [illegible] sea water, enters into the composition of the pan- [illegible]

In the course of this i [illegible] was induced to repeat the same experiments severa [illegible] on various specimens of salt bearing the same [illegible] on; and was surprised to find that the results by [illegible] s corresponded. In one instance, for example, f [illegible] t was found in 1000 parts to contain no less then 1 [illegible] of sulphate of lime; while another specimen, nominally the same, contained only $11\frac{1}{4}$ parts of selenite in the same quantity; and a third only $5\frac{1}{4}$. At length it occurred to me that these differences were probably owing to the circumstance of the salt having been taken from the boiler at different periods of the evaporation. I requested, therefore, to be furnished with specimens of salt, drawn at different stages of the process from a given portion of brine, evaporated in the same boiler. These were submitted to analysis; and the results are shown in the following table.

		Sulphate of lime.
Common salt drawn from the boiler two hours after the first application of heat ..	contained in 1000 parts	16
Salt drawn four hours after do.		11
Salt drawn six hours after do.		$3\frac{1}{2}$

Hence it appears that there was a gradually increasing purity in the salt from sulphate of lime, as the process of evaporation advanced, the greatest part of this earthy compound being deposited at an early stage of the process. Different specimens of the same kind of salt may, therefore, differ in chemical purity as much from each other as from other varieties. But when the impurities contained in a solution of muriate of soda are of a different species from those of Cheshire brine, and consist chiefly of the earthy muriates, the order will be reversed, and the purest salt, as I have already suggested, will be that which is first deposited, the contamination with the muriate of lime or of magnesia continuing to increase as the process advances to a conclusion *.

At an early period of the inquiry, it appeared to me probable that the differences between the several varieties of culinary salt might depend, in some degree, on their containing variable proportions of water of crystallization. It was found, however, by experiment, that the proportion of water in any variety of common salt, after being dried at 212° Fahrenheit, is not much greater or less than that which is contained in any other variety. Pure transparent rock-salt, calcined for half an hour in a low red heat, (=4° or 5° of Wedgwood's pyrometer,) lost absolutely nothing of its weight. It is remarkable, also, that the pure native salt, if free from adventitious moisture, may be suddenly and strongly heated, with scarcely any of that sound called *decrepitation*†, which is produced by the similar treatment of all the varieties of artificial salt. Even these varieties, however, exposed during equal times to a low red heat, do not lose more than from half a grain to three grains in one hundred. This comparison cannot be extended to the salt prepared at a boiling temperature from sea water; because the muriate of magnesia which these varieties contain, is decomposed at a red heat, and deprived of its acid.

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* I cannot on any other principle explain the considerable differences, as to the proportion of muriate of magnesia, that were discovered in the several varieties of Scotch salt sent to me by Dr. Thomson. For this reason, in stating the analysis of Scotch salt, I have given, in the table, that result which was most frequently obtained; and have withheld the names of the manufacturers, because the differences were probably in a great measure accidental, and not the result of greater or less skill in the preparation. One specimen of Lymington salt which I examined, contained fully as much muriate of magnesia as any of the Scotch samples. The *cat salt* of that place, however, contrary to my expectation, proved to possess a very extraordinary degree of purity; a fact of which I satisfied myself by repeated experiments.

† Decrepitation is occasioned by the sudden conversion into vapour of the water contained in salts, when its quantity is insufficient to effect the watery fusion. It is a property peculiar to salts which hold only a very small proportion of water in combination; as muriate of soda, nitrate of lead, and sulphate of potash.

The

The following table shows the quantity of water con-
ined in several kinds of salt, inferred from the loss which
ey sustain by ignition during equal times, after being
rst dried at 212°.

100 parts of large-grained fishery salt contain of water	....	3
100	.... foreign bay salt (St. Martin's)	3
100	.... ditto ....(Oleron)	2½
100	.... ditto, Cheshire common salt	1½
100	.... ditto ......stoved salt	0½

The loudness and violence of the decrepitation was, as
early as could be judged, in the same order, and was most
emarkable in the large-gra rieties.

To determine the propo f real muriate of soda in
hose varieties of artificial ich are nearly free from
arthy muriates, I employe he process of decompo-
ition by nitrate of silver. llowing are the quantities
f fused *luna cornea* obtai m 100 grains of each of
hree varieties dried, prev o solution, at the tem-
erature of 212° Fahrenhe

100 gr. pure transparent salt gave of luna cornea	....	242
100....stoved salt, ren pure	....	239
100....fishery salt.	....	237*

The proportion of ingr in the several kinds of
nuriate of soda (setting the impurities) appears,
herefore, to be nearly th in all. And as the very
ninute quantity of water vered by analysis is not
onstant in the several varieties, it may be inferred to be
ather an accidental than a necessary ingredient; for in the
atter case an invariable proportion might be expected,
onformably to the important law, establishing an uni-
ormity in the proportions of chemical compounds, which
ias been explained by Mr. Dalton, and confirmed by Drs.
Thomson and Wollaston.

What then, it may be inquired, is the cause of those

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[*] From 100 grains of pure artificial muriate of soda, previously heated o redness, Dr. Marcet has since informed me that he obtained 24·16 grains f fused luna cornea. The weights of the precipitates thrown down in my xperiments by nitrate of silver are not, I am aware, exactly those which night have been expected from the table of the comparative proportions f water given in the text. Each experiment, however, was twice repeated vith every precaution I could adopt, and with the same results. That dif-erent kinds of salt give different proportions of luna cornea, is proved also y comparing the experiment of Dr. Marcet with the results of Dr. Black nd Klaproth, both of whom found the fused muriate of silver from 100 arts of common salt to weigh 235 grains.

differences which are acknowledged, on all hands, to exist among the several species of muriate of soda, so far as respects their fitness for œconomical purposes? If I were to hazard an opinion, on a subject about which there must still be some uncertainty, it would be that the differences of *chemical composition*, discovered by the preceding train of experiments, in the several varieties of culinary salt, are scarcely sufficient to account for those properties which are imputed to them on the ground of experience. The *stoved* and *fishery* salt, for example, though differing in a very trivial degree as to the kind or proportion of their ingredients, are adapted to widely different uses. Thus the large-grained salt is peculiarly fitted for the packing of fish and other provisions, a purpose to which the small-grained salts are much less suitable. Their different powers, then, of preserving food must depend on some mechanical property; and the only obvious one is the magnitude of the crystals, and their degree of compactness and hardness. Quickness of solution, it is well known, is pretty nearly proportional, all other circumstances being equal to the quantity of surface exposed. And since the surfaces of cubes are as the squares of their sides, it should follow that a salt whose crystals are of a given magnitude will dissolve four times more slowly than one whose cubes have only half the size.

That kind of salt, then, which possesses most eminently the combined properties of hardness, compactness, and perfection of crystals, will be best adapted to the purpose of packing fish and other provisions. because it will remain permanently between the different layers, or will be very gradually dissolved by the fluids that exude from the provisions; thus furnishing a slow but constant supply of saturated brine. On the other hand, for the purpose of preparing the pickle, or of *striking* the meat, which is done by immersion in a saturated solution of salt, the smaller-grained varieties answer equally well; or, on account of their greater solubility, even better.

With the hardness or strong aggregation of the several varieties of salt, it seemed to me not improbable that their specific gravity might in some degree be connected. The exact determination of this property in saline substances is, however, a problem of considerable difficulty, as will sufficiently appear from the various results which have been given, with respect to the same salts, by different experimentalists. Thus Muschenbroek makes the specific gravity of artificial muriate of soda to vary from 1918 to 2148, the mean of which is 2033. Sir Isaac Newton states it at 2143, and Hassenfratz at 2200[*]. All that was necessary for my purpose was an approximation to the truth; and the introduction of a small error could be of no importance, provided it were the same in every case, since the comparison would still hold good.

The specific gravity of rock salt, there can be little difficulty in determining with precision. A piece of this salt[†], of such perfect transparency that I had reserved it as a cabinet specimen, weighed in the air 513 grains, and lost, when weighed in alcohol, 194 grains. The alcohol, at the temperature of 56° Fahrenheit, had the specific gravity of
820, and hence that of tl ay be estimated at 2170.
Another specimen cons less pure, and more ap-
proaching to a fibrous f had the specific gravity of
2125 only.

For ascertaining the sp eights of artificial varieties
of salts, I used a very sir trivance. It consisted of
a glass globe about 3⅓ di having a stem or neck 10
inches long. Sixteen c hes of water (each 252½
grains at 60° Fahrenheit, he whole of the globe, and
about half an inch of the art of the neck; and from
the line where the water n the instrument, it was
accurately graduated up to hundredth parts of a
cubical inch. Into this I poured exactly sixteen
cubic inches of a perfec ated solution of common
salt; and then added 40(of the salt under examina-
tion, washing down the that adhered to the neck
by a portion of the liquid, which had been previously taken out of the globe for the purpose. As much as possible of the air which adhered to the salt was dislodged by agitation, and the increase of bulk was then observed.

Care was taken that the salts were all of equal temperature and dryness, and that no change of temperature happened during the experiment.

	Hundredths of a cub. in.	Hence its specific grav. was[‡]
400 grains of the less pure kind of rock salt, broken down into small fragments, filled the space of........	75	2112
400 grains of stoved salt...........	75	2112
400 do (another sample)	76	2084
400 do. common salt	76	2084
400 large grained fishery salt	83	1909
400 do. (another sample)..........	83	1909
400 St. Ube's	82	1932

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[*] *Annales de Chimie*, vol. xxviii. p. 13.

[†] Foliated rock salt of Jameson. See his Mineralogy, vol. ii. p. 10.

[‡] Distilled water at 1000 being taken as the standard.

If the above mode of determination at all approach to correctness, it would appear that the specific gravity of rock salt is diminished, by being broken into small fragments, from 2125 to 2112, probably in consequence of the quantity of air which the fragments envelop, and which cannot be entirely separated by agitation. From the numbers given in the last column, it is evident that the smaller-grained salts are specifically heavier than those which are composed of larger and more perfect crystals. A difference of only one or two hundredth parts of a cubic inch is perhaps entitled, in a process of this kind, to little reliance; and I do not therefore regard it as indicating any material difference in the specific gravity of the first four or last three salts submitted to experiment.' But when the difference amounts to eight hundredths, as between the small- and large-grained salt, it may safely be imputed to an inferior specific gravity in that species, which occupies so much greater a proportional bulk *.

The last series of experiments proves decisively, that in an important quality, (viz. that of specific gravity,) which is probably connected with the mechanical property of hardness and compactness of crystals, little or no difference is discoverable between the large-grained salt of British, and that of foreign manufacture. If no superiority, then, be claimed for British salt as applicable to œconomical purposes, on account of the greater degree of chemical purity which unquestionably belongs to it, it may safely, I believe, be asserted that the larger-grained varieties are, as to their mechanical properties, fully equal to the foreign bay salt. And the period, it may be hoped, is not far distant, when a prejudice (for such, from the result of this investigation, it appears to be,) will be done away, which has long proved injurious to the interests and prosperity of an important branch of British manufacture.

[To be continued.]

XIX. *Description of a Metallic Thermometer for indicating the higher Degrees of Temperature.*

To Mr. Tilloch.

Sir, I beg leave, through the medium of your Magazine, briefly to mention the principle of a *new thermometer,* conontrived by me, for the purpose of exhibiting the dif- erence in temperature, or degrees of heat, which takes place etween the mercurial thermometer, the scale of which erminates upwards, at 600°, and that of baked clay, or Wedgwood's thermometer, the scale of which commences t 1077° of Fahrenheit, or red-heat, thus forming an inter- nediate or *connecting thermometer* between the two above nentioned.

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* M. Hassenfratz seems to have suspected that a difference in the specific gravity of the same salt may be occasioned by a variation in its state of crystallization. *De la Pesanteur spécifique des Sels, Ann. de Chim.* xxviii. p. 17.

A metallic composition is formed, not liable to alteration n its quality or quantity by repeated exposure to heat, the nelting point of which is at a little below 600° of Fahren- neit, and its boiling point at 1200°. A case resembling n form the glass case fo dinary thermometer, but omewhat larger, contain etallic composition, and he scale consists in a s *graduated rod*, equal in neight at the commencem the scale, that is when he metallic composition is liquid to the top of the ube; the graduated rod ting at the bottom in a hin, circular, flat plate, rests or floats as it were ipon the liquid metal; an oportion as the latter ex- pands and rises in the tub at, the graduated rod is ouoyed up, or raised abo top of the tube, passing hrough a perforated cov he *maximum*, or boiling oint*.

The same principle, I bserve, admits of being xtended, for the purpos ertaining the variation in emperature up to the mo e heat, perhaps, that can e required.

It is unnecessary to state here, that the influence of the ncumbent atmosphere upon the surface of the liquid me- al within the open tube is too inconsiderable, even at the ommencement of the scale, to deserve notice, and at a igher temperature diminishes to nothing; especially if he whole of the liquid contained in the thermometer, *as ught to be the case in the use of every thermometer*, be ompletely immersed or subjected to the temperature, the legree of which it is intended to indicate.

A method similar to the above, I should think, might e applicable to the purpose of showing in a ready way the legree of expansion in metals by heat; but the elongation of a cylinder of any metal, by increase of temperature, is much too small to admit of its being a *convenient measure* of temperature.

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* The thermometer case and graduated rod are at present formed of ipe makers' clay previously prepared by having been exposed to a suffi- ient degree of heat.

The scale of this new thermometer is an exact continuation of the scale n the mercurial thermometer; the *lower degrees* of the former correspond- ng with, or indicating like temperatures with, the *upper degrees* of the nercurial thermometer.

I should not despair, however, (availing myself of every advantage, viz. increasing the length of a metallic wire, by giving it a spiral form, in order to comprise a considerable length in small compass; with the application of the lever-index, and a good magnifier,) of constructing a thermometer upon this principle, so as to render the scale apparent even to single degrees; using silver for the lower temperatures, and platina for the higher, or employing iron wire, *only* up to its ultimate point of expansion in a solid state [*].

I am, sir,
Your obedient servant.
RICHARD WALKER.

Queen-street, Oxford,
Aug. 6, 1810.

XX. *On Crystallography. By M. Hauy. Translated from the last Paris Edition of his* Traité de Minéralogie.

[Continued from p. 69.]

THEORY OF THE LAWS TO WHICH THE STRUCTURE OF CRYSTALS IS SUBJECTED.—GEOMETRICAL PART.

Preliminary Notions.

1. The theory which I here propose to submit to calculation has for its object, to determine all the different forms which may arise from a superposition of decreasing laminæ following known directions and laws, on the various faces of a solid, the figure of which is also given [†].

2. The solid which I call *nucleus* or *primitive form* is always one of the six following: 1st, the parallelopipedon; 2d, the regular hexahedral prism; 3d, the rhomboidal dodecahedron; 4th, the octahedron; 5th, the tetrahedron, which in this case is always regular; 6th, the bipyramidal dodecahedron.

3. By subdividing each of these solids parallel to its different faces, and sometimes also in other directions, we obtain the integrant molecules, which are always either parallelopipedons, triangular prisms, or tetrahedrons.

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[*] For the means of rendering exceedingly minute divisions distinct, see a method described in the Monthly Magazine for May 1810.

[†] I presume that my readers are acquainted with that part of my treatise in which the same theory is detailed by simple reasoning. I shall therefore now confine myself to resuming in a succinct manner the most general principles of this theory.

4. When the nucleus being a parallelopipedon is divisi-le only by planes parallel to its six faces, it is evident that he integrant molecule is itself a parallelopipedon similar o this nucleus.

5. But even when the integrant molecules differ from he parallelopipedon, they are always situated in the interior f the nucleus, in such a manner that being taken by small roups they compose parallelopipedons; and the decre-nents which give the secondary forms are always made by ows of these parallelopipedons as in the case first men-ioned.

I give the name of *subtractive molecules* to the small arallelopipedons, divisible divisible, the subtraction f which determines the its of the laminæ of su-erposition.

It follows from what has said, that the subtractive nolecule is a kind of uni which we may refer the tructure of all crystals in , so that we are at liberty o adhere to the data whi nishes in the application f calculation to every pos stalline form. To know fterwards if this unity be ble, or if it has fractional arts, is a matter of observ hich may be interesting in atural history, but indepe which the theory would ot admit of our proceedir ds the object in view.

6. In the case where th us itself differs from the arallelopipedon, we may substitute for it a solid f that form, either by abs from some of its faces, f there are more than six multiplying the subdivi-ions always in the direction ne natural joints, if it be tetrahedron. But we frequently obtain more simple re-ults, by giving the preference to the true nucleus.

7. The decrements undergone by the laminæ of super-osition may be effected in all imaginable directions. The mits of these directions are the edges and the diagonals of he faces of the nucleus. Between these two limits there s an infinity of intermediate ones, according as the small olids, the rows of which determine the quantity of the de-rements, are considered as double, treble, quadruple, &c. f the subtractive molecule. I call *decrements on the edges* hose which take place parallel to the edges of the faces f the nucleus; *decrements on the angles*, those which take lace parallel to the diagonals; and *intermediary decre-nents*, those which are made parallel to lines comprehended etween the edges and the diagonals.

I shall now successively treat of the different primitive orms above mentioned, and give, relatively to each of them, the method of calculating the results of all the laws of decrements of which it is susceptible. I shall begin with the parallelopipedon, which is as it were the term of comparison to which the other forms refer.

1. *Theory of the Parallelopipedon.*

8. Let AG (fig. 1, Pl. IX) be a parallelopipedon, the faces of which may have whatever respective dimensions and measurements of angles we please. Let us conceive this solid subdivided, by plans secting parallel to its different faces, into a multitude of elementary parallelopipedons which will be the integrant molecules. Each of the same faces will be separated in its turn into a certain number of small parallelograms, which will be the exterior faces of as many molecules.

If we choose any two of the six faces in question, provided they are opposite, we may consider the solid as an assemblage of laminæ distinguished by the secting plans parallel to these very faces.

9. Let us now imagine new laminæ formed of small parallelopipedons similar and equal to the foregoing, which are placed as if in steps above various faces of the generator parallelopipedon, in such a manner that the facets in contact coincide exactly, like what takes place in the interior of this solid. Here there are three cases to be distinguished. The first is that in which the laminæ extend by their edges so as to envelop completely the generator parallelopipedon, which will grow without changing its form. The second is that in which the laminæ would remain on a level by their edges with the faces adjacent to the generator parallelopipedon, in which case it is easy to see that they would form re-entering angles at the places of the ridges DC, BC, CG, &c. In the third case, the laminæ will go on decreasing, following certain directions, in such a manner that each will be exceeded by the foregoing in a quantity equal to one or more rows, either in breadth or height.

Of these three cases, the first is relative to the primitive forms given immediately by crystallization, and admits of no difficulty. The second is foreign to our views, because nature presents us with no example of it in simple crystals. We shall dwell at some length upon the third, which is properly the object of the theory.

10. Let us conceive in the first plaee that the decrements are produced in breadth on all the ridges by subtraction of an equal number of rows, and let us confine ourselves for

the moment to the consideration of the effect of the decrement which takes place parallel to the ridge BC, ascending above the parallelogram ABCD.

If we suppose that the form of the integrant molecule which is similar to the generator parallelopipedon is determined, and that the law of decrement is known, it will be easy to find the angle formed with ABCD by the face produced in virtue of this decrement.

Let ag (fig. 2) be one of the molecules, of which the faces analogous to those of the parallelopipedon, fig. 1, are marked with the same letters. From the point c I draw cs and cr perpendicular on bc. Now, by the hypothesis, the relation between these es is given, as well as the angle rcs which measur incidence of $abcd$ upon $bcgh$.

Now let op (fig. 1) be stance between the ridge BC and the first lamina rposition, which distance is regarded as being meas the plane ABCD. It is clear that op is equal to c) multiplied by the number n of rows subtracted. fore $op = n \times cr$. From the point p (fig. 1) raise g upon that of the lateral faces of the first lamina, s turned from the same side, and equal to the he his face. We shall have $pu = cs$ (fig. 2) and o r. Complete the triangle upo (fig. 1). It is visi the line ou will coincide with the face of the seco ystal, which rises on the ridge BC, and that the a u will measure the incidence of this face on th elogram ABCD. Thus, since in the triangle upo we know the two sides op, pu, and the comprehended angle opu, it will be easy to get the angle pou which gives the incidence wanted.

11. The triangle pou is called *mensurator triangle;* and I shall subsequently give this name to all the triangles which perform the same function.

12. Let us now consider the effect of the decrement which takes place parallel to the same ridge BC, by descending on the face BCGH. Let oih be the mensurator triangle, in which oi is the distance between the ridge BC and the first lamina of superposition, ih coincides with that of the lateral faces of this lamina, which looks towards the ridge BC, and besides it is equal to the height of the face in question; finally, oh is laid on the face which results from the decrement.

Let n' be the number of rows subtracted. We shall have oi (fig. 1,) $= n' \times cs$ (fig. 2). Also ih (fig. 1) $= cr$

(fig. 2), and $oih = rcs$. Thus it will be easy to determine the angle which the face produced by the decrement forms with BCGH (fig. 1)

13. It may happen that the two decrements which act on both sides of the ridge BC have such a connexion with each other that the two faces which will result from them will coincide upon one and the same plane, so that the side oh of the triangle oih is upon the direction of the side ou which belongs to the triangle upo, as we see in fig. 3. To prove this, we may remark, that in this case the triangles upo, oih are similar, as well on account of the equality of the angles opu, hio, and the parallelism of the sides op, ih, as on account of the coincidence of the sides ou, ho upon one and the same direction.

Therefore $pu : op :: oi : ih$.

Or rather cs (fig. 2) $: n \times cr :: n' \times cs : cr$.

Which gives $n' = \frac{1}{n}$.

That is to say, the case in question will happen every time that the decrements which take place on proceeding from BC towards GH are in the inverse ratio to those which take place in going from BC towards AD, or, what comes to the same thing, at all times when there is on one side a decrement in height equal to that which shall be on the opposite side. We may easily conceive that the two faces will be still on one and the same plane in the peculiar case of a decrement by one row on both sides.

14. Hence we may conclude, that in all circumstances similar to those which have been cited, we may make abstraction of one of the two decrements, by considering the face which results from it as the continuation of that which arises from the other decrement.

We see what would have been necessary for determining in a similar way the incidences of the faces produced by the other decrements upon the analogous faces of the generator parallelopipedon.

15. The greatest number of faces which the secondary solid can have, is twenty-four, since the generator parallelopipedon has twelve ridges, each of which is the line of departure of two decrements which act in an opposite direction. These faces will all be triangles, or some triangles and others trapeziums, according as the generator parallelopipedon will be found more elongated in one direction than in the other, or as the decrements which will take place parallel to certain edges will follow a more rapid course than those which would act parallel to other edges.

The smallest number of faces which the secondary crystal can have is twelve. Then all the decrements considered two by two, setting out from one and the same ridge, will be inverse to each other.

The simplest case is that in which the generator parallelopipedon being a cube, we have $n=1$ and $n'=1$. On this hypothesis, the secondary solid is a dodecahedron with rhombic planes all equal and similar, as we have explained in the reasoning part of the work.

16. We now proceed to the method of determining the decrements upon the angles. But we may previously remark, that in this case the decreasing parts of the laminæ of superposition form an [illegible] ernately re-entering and salient, in such a way, ho [illegible] that all the ridges of molecule situated at the plac [illegible] the salient angles are on one and the same plan: [illegible] ll consequently designate the series of these ridges b [illegible] ame of *lateral face*.

This being done, let [illegible] ceive that decrements in breadth are produced by e [illegible] mbers of rows on all the angles of the parallelopip [illegible] g. 1; and let us take for instance that which tak [illegible] upon the angle BCD. Let Ckl be the mensurat [illegible] le, in which Ck measures the distance between the [illegible] and the first lamina of superposition, kl is regar [illegible] eing applied on the corresponding lateral face, [illegible] ht of which it measures, and Cl coincides with [illegible] of the secondary crystal, produced by the decreme [illegible] stion.

Having traced the dia [illegible] db, fh (fig. 2) on the bases of the molecules, I draw ct perpendicular upon db, and xz perpendicular as well upon db as upon fh.

Let N be the number of rows subtracted. We shall have Ck (fig. 1) $=$ N $\times$ ct (fig. 2), and kl (fig. 1) $= xz$ (fig. 2); besides the angle Ckl (fig. 1) will be equal to that formed by the plane $bdfh$ (fig. 2) with fgh. Now these three quantities are regarded as being known, since the form of the molecule is determined. Thus it will be easy to find the angle kCl (fig. 1) which measures the inclination of the face produced by the decrement upon the parallelogram ABCD. We shall conduct ourselves in the same manner in order to calculate the effects of the decrements on the other angles.

17. Let us now consider the hypothesis in which the decrements which take place on the two angles DCG, BCG would have such a connexion with that which acts on the angle BCD, that the faces produced by these three decrements would coincide on one and the same plane.

Let AG (fig. 4) always be the generator parallelopipedon. Suppose that the decrement which takes place in breadth upon the angle BCD has such a measurement, that the lower edge of the first lamina of superposition passes by $m\,r$, in which case such of the lines Cm, Cr will contain as many ridges of molecule equal to $c\,d$, or $c\,b$ (fig. 2), as there will be rows subtracted by the decrement. Having taken upon CG (fig. 4) a part Cc equal to $c\,g$ (fig. 2), make a plane pass by the points m, c, r. I say that this plane is parallel to the face which results from the decrement. In order to prove it, having drawn indefinitely the lines $m\,s$ and $r\,u$ parallel to CG, I prolong them each upwards, so as to have Mm, or Rr equal to Cc. Now these prolongations Mm and Rr represent two of the ridges situated on the lateral face of the first lamina. Therefore the face produced by the decrement passes by the points M, R. But besides it passes by the point C, which is the term of departure of the decrement; therefore the plane MCR coincides with it. Now the small lines Cc, Mm, Rr, being three longitudinal ridges of molecule, situated parallel to each other between the two planes mcr, MCR, it is visible that these two planes are themselves parallel, *i.e.* that mcr is parallel to the face which arises from the decrement.

The same reasoning applies to the hypothesis in which the decrement should take place in height. In this case it would be necessary, in order to make the plane mcr be parallel to the face produced, that we should have $cm = cd$ (fig. 2); $cr = cb$; and that the line Cc (fig. 4) should contain as many times $c\,g$ (fig. 2) as there would be ranges subtracted in the direction of the height.

18. Let us suppose that the plane MCR is prolonged above the faces CDFG, BCGH, and consider the prolongations as two faces which would be the effect of two decrements, the one upon the angle DCG, the other upon BCG. These decrements being equal, we shall confine ourselves to that which acts upon the angle DCG. Since the plan cmr is parallel to the face which results from this decrement, it is clear that cm coincides with the lower edge of the first lamina of superposition applied upon CDFG, and that Cr contains as many ridges of molecule as there are ranges subtracted in height.

19. If the decrement relative to the angle BCD takes place by one row, it is evident that the two other decrements relative the one to the angle DCG, the other to the angle BCG, will also take place by one row: since then the three lines Cm, Cr, Cc, are equal each to one ridge of molecules,

nolecules, the three decrements must necessarily have the ame measurement.

20. But if the decrement relative to the angle BCD is roduced by more than one row, then the two others will ecessarily be intermediate, and it will be sufficient to have he law of the first decrement for determining the two thers. Let us suppose, for example, that the decrement in he angle BCD is made by three ranges in breadth. In his case C m and C r will each of them be equal to three idges of molecules, and C c will be equal to one ridge. herefore the decrement on the angle DCG is produced in uch a manner that there are three ridges of molecules subracted in the direction of C one alone in the direction f CG; and besides this it is made by three rows a height, since C r answ hree ridges of molecules. t is the same with the dec which takes place on the ngle BCG.

21. In all cases of this d on, the theory only considers the effect of the decre which takes place accordig to the ordinary laws, é there results from it a uch more simple solutio he two other decrements, f which abstraction has b de, are considered as interevening in a subsidiary to second the effect of e first, and prolong towa parts adjacent the face to hich it has given birth.

22. The greatest numbe es which the secondary rystal can have, in the h is of a decrement on all e angles, is twenty-four, ere are eight solid angles ach composed of three plane angles, which are the terms f departure of as many decrements. The *minimum* of e number of faces in the same hypothesis is eight; and lthough strictly speaking there are always twenty-four decements, we only consider eight, which gives us the fality of employing ordinary laws only, for determining the orm of the secondary crystal.

23. The simplest case is that in which the generator pallelopipedon being a cube, all the decrements are done by ne row. The secondary solid is then a regular octahedron. But it may happen that the three decrements which take ace around one and the same solid angle are all interediary. In this case it is sufficient that one of them be etermined, in order to render it easy to conclude from ence the two others, by the help of a construction similar that which we have previously employed.

24. Let us suppose that fig. 5 represents the generator llelopipedon, marked with the letters relative to the

method of indicative signs. Let us conceive that there is made upon the angle O, ascending, a decrement which produces a face parallel to the plane nrs, and the expression of which is $(\overset{4}{O}D^3F^4)$, whence it follows that $on = 3cd$ (fig. 2), $Or = 4cb$, and $Os = 2cg$.

This being done, the expression of the decrement on the left of the angle O will be $({}^{\frac{1}{4}}OD^3H^2)$, and that of the decrement to the right of the same angle $(O^{\frac{4}{3}}F^4H^4)$.

25. In order to determine the angles formed by the faces produced by the intermediary decrements with the corresponding faces of the nucleus, what presents itself as the simplest is to consider every little group of molecules, which results from the decrement, as forming one single molecule; which brings back the calculation to that which is employed for the ordinary decrements on the angles.

Let us take for example the decrement on the angle O ascending, represented $(\overset{4}{O}D^3F^4)$. It is easy to judge that in this case, the group which represents two subtractive molecules placed the one above the other, is that which we see fig. 6, and in which the side mn is composed of three ridges of molecule, the side np of four ridges, and the side nk of two ridges, on account of the decrement by two ranges in height.

Having traced on the bases the diagonals mp, io, I draw vt perpendicular upon mp, then us perpendicular as well upon mp as upon io.

Let nty (fig. 7) be the mensurator triangle, in which nt being regarded as lying on the plane AEOI (fig. 5) will be equal to the same line (fig. 6). Besides, we shall have ty (fig. 7) $= us$ (fig. 6), and the angle nty (fig. 7) will be equal to that formed by the plane $mpoi$ (fig. 6) with the triangle iko. Thus it will be easy to find the angle ynt (fig. 7) which measures the inclination wanted.

26. The solutions of problems of this kind are often simplified in practice, by a series of the regular form of molecules. Let us suppose, for example, that the latter are cubes. Let us designate each of their ridges by unity. We shall have (fig. 6) $mn = 3$, $np = 4$, $nk = 2$, $mp = \sqrt{(mn)^2 + (np)^2} = \sqrt{25} = 5$. $nt = \frac{mn \times np}{mp} = \frac{12}{5}$. $us = nk = 2$.

Thus also nt (fig. 7) $= \frac{12}{5}$, $ty = 2$.

Thus $nt : ty :: \frac{12}{5} : 2 :: 6 : 5$. Besides, in this same case, the angle nty is straight; from which we see how easy it is to find the angle ynt.

27. The mensurator triangles relative to the decrements on the angles may be substituted for those which we have considered in the decrements on the edges, and serve equally well for determining the secondary forms. Let us suppose, for example, that AG (fig. 8) represents a cubical nucleus, which undergoes decrements by two ranges on the four edges of the base ABCD, and that we wish to know the angles of the pyramid SADCB produced by this decrement. Having trac
iagonals BD, AC, I draw
from their point o of inte the line op perpendicular
upon CD, then sp. If I pon po the part pr equal
to two ridges of molecule, om the point r I raise ru
perpendicular upon ABCI which by the hypothesis
will be found equal to one of molecule, the triangle
upr will perform the fur f the ordinary mensurator
triangle, and by means of ht angle urp, and of the
relation 2 : 1 between the r and ur, we shall easily
find the incidence of DSC the base ABCD, as well
as the values of the other For, on account of the
similar triangles upr, sp y thing is reduced to the
calculation of the angles traight pyramid in which
the side BC of the base, s double of po, is to the
axis os in the relation of

On the other hand,—I e upon Co the part Cn
equal to two diagonals of mo e; and if from the point n I raise nz perpendicular upon ABCD, Cn will represent the distance from the point C to the first lamina of superposition, taken in the direction of Co, and nz will be equal to one ridge of molecule; from which it follows that the triangle zCn may also perform the function of mensurator triangle.

We shall therefore have $Cn : nz :: 2\sqrt{2} : 1$, and because the triangle zCn is similar to the triangle sCo, the question considered under this new point of view will be reduced to seek the angles of a straight pyramid, in which the demi-diagonal Co of the base is to the axis os, as $2\sqrt{2} : 1$, which is sufficient for having all the rest. We shall have occasion more than once thus to substitute one mensurator triangle for the other, when there will result from it more facility in resolving the problems.

All the details upon which we have acted ought to be regarded as preliminary notions intended particularly to enable students clearly to understand the use of the mensurator triangles which will incessantly recur in the applications which we shall make of the calculus to the laws of decrements. We shall now proceed more particularly to the methods relative to this object; and as the rhomboid, which likewise comprehends the cube, is of all the kinds of parallelopipedon the most fertile in diversified results, and at the same time that which is the most easily adapted to the employment of general formulæ, we shall first give the theory of this solid; after which we shall resume that of the parallelopipedons of a different form.

⁂ Understanding that an English Translation of the whole of Mr. Haüy's valuable work on Crystallography is now preparing for the press, we intend, for the present, to suspend our labours upon it, as a full translation cannot fail to answer the object we had in view, better than those disjoined portions which can alone be admissible into the pages of a periodical work. —Should the proposed translation not appear in some reasonable time, we may hereafter resume our labours.

XXI. *Dr.* HEALY *on Cupping.*

To Mr. Tilloch.

No. 1, Clarendon-street, Dublin.

SIR, I REQUEST you will permit me to contradict an observation which has been made, in the Retrospect of Discoveries, stating that the mode which I propose for cupping without the assistance of the syringe, is so far from new, that it occurred nearly 2000 years ago, to Hero of Alexandria, and that the figure is exhibited in the *Mathematici Veteres.*—I consulted the Parisian edition, and find his contrivance (as described page 207) entirely different from mine. Suffice it to say, a partial exhaustion is produced by the mouth from a secondary cavity, and two stopcocks are made use of. The syringe, which is an improvement, and answers for the secondary cavity of Hero, is the usual mode at present of producing the vacuum, and not, as the observer states, the spirit lamp or tow. The apparatus which I propose will still, I imagine, be found new, more œconomical, and less complicated, than any that has been hitherto adopted.

Your much obliged,

July 19, 1810. ROBERT HEALY, M.B.

XXII. *Observations on the Purity of Standard Gold.* By M. FABBRONI, *of Florence, corresponding Member of the French Institute. To which are subjoined Notes by* M. D'ARCET, *Assay Master of the French Mint*[*].

ALMOST all naturalists (following perhaps implicitly the assertion of Pliny[†]) maintain that native gold is never found pure; *i. e.* entirely free from alloy, chiefly of silver, and that the finest is scarcely from 0·875 to 0·917, or 21 or 22 carats. The gold in dust, in spangles, or in sand, which is brought from Africa, is most frequently within these limits. I have seen som[illegible] from the country of Bambuck in Africa, which [illegible] 927, or 22 carats and one-fourth; in the mint of [illegible] e it has also been seen at 0·958, or 23 carats: th[illegible] l had been brought from Morocco. The carat in [illegible] y is divided into eighths.

It is probable that in [illegible] rly ages money was coined with native gold, in th[illegible] n which it was found, there being no grounds for [illegible] ng that they took pains to refine it.

It has been thought t[illegible] oldest gold coin known is that of Battus IV, whic[illegible] melted or struck at Cyrene in Africa in the time of [illegible] tus: it does not appear that the standard of this gol[illegible] nown. Of all the Grecian coins which are in the [illegible] of the collectors of medals, the most ancient are th[illegible] ful pieces of Philip the father of Alexander. Th[illegible] prising man, who from his infancy conceived the [illegible] f ascending the throne of Macedon, and becoming master of Greece, was fortunate enough to find several rich mines of gold, which he knew how to prize. Mount Pangea annually furnished him with gold to the amount of 5,229,000 francs, and from thence he derived the most powerful resources for the success of his political designs and military talents. It is not known whether this metal of Philip's underwent any particular operations before passing to the mint. There are some grounds, however, for thinking that it was used in the state in which it was found.

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[*] *Annales de Chimie*, tome lxxii. p. 25.

[†] Pliny says, lib. [illegible], that there is no kind of gold more perfect than spangle gold; that gold obtained by searching the beds of rivers does not require melting, and that it is native and perfect gold. But Pliny says in the same book that lead is more malleable and heavier than gold; which proves that the gold which he regarded as pure was in reality an alloy. He says likewise a little farther on, that all gold is mixed with silver, and that the gold which is the least alloyed with silver came from Albicratum in Gaul, and that it contained only $\frac{1}{36}$. From all this it is evident that Pliny's opinion is not to be followed, and recourse must be had to experiments.

Patin assayed a *statera* of gold of this king (the denomination of his coin among the Persians and Macedonians), and found it to be 23 carats and a half, or 0·970. We cannot allow ourselves to believe that the metallurgists of that monarch thought to purify gold by adding to it only a 48th part of alloy, but we can easily suppose that the gold was found in this state of fineness.

If alloys were added to gold from a bad design, or with the mistaken idea of covering the expense of manufacturing it; this has degenerated into fraud, and has no limits: if the alloy was added with the view of making the money harder, it was a futile attempt. Neither of these motives could sway with Philip, because he enjoyed abundant mines of gold, and because, as he wished to appear generous, he would have made his coin of pure gold, if he thought it necessary to refine it: or he would have added more alloy, if policy suggested that he should not employ it as it came from the bowels of the earth*. It should seem, therefore, that his mines furnished him with gold at 23 carats and a half (0·979), as it is found to be in his coins, if there be no error in Patin's analysis: but it might perhaps be interesting to confirm this fact by a new experiment.

Chevalier Fossombroni, an eminent mathematician, in digging the foundation of a house near Arezzo, found a statera of Philip in good preservation, which he was kind enough immediately to give us to be examined by analysis.

On one side of this piece, as in most of Philip's coins, there is a head of Apollo, and on the reverse a car with two horses: the name is on the exergue:—on similar coins we see under the legs of the horse a monogram or type indicating the mint where the coin was struck. On the piece in question there was a trident, which means Trœzene.

Fourteen of these coins are preserved in the cabinet of the Florence gallery: the face and the reverse of eleven of them are similar to that of Arezzo; but they have various distinguishing marks, one only bearing the same mint-mark

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* I transmitted to M. Mongez the analysis of an ancient coin with the effigy of Philip: its examination also proved, that under the reign of that prince alloys were used in the making of money, the composition of which was natural, or at least unknown; for it contained silver 368, gold 184, copper 448.

It is not likely that so complex an alloy would have been used at a period when the modes of analysis were so little known, as to fall far short of the degree of exactness which may be attained even by employing the touchstone and prepared acid now in use.

with that found at Arezzo. The weight of two of these, completely similar in external appearance, was precisely 176 grains of Florence each. The same weight was found in another distinguished by a monogram formed by a large K, and a little o; the same weight in another which had a thunderbolt; the same in another with a vase; and lastly, in another with a grain of wheat, the mark of the Leontini. These six weights of the largest stateræ which remain, and which are equal, gave grounds for concluding that the above was the weight prescribed for the Greek money. This being granted, we may infer that the drachma weighed 88 grains. (Romé de l'Isle assigns three grains more to
the great Attic drachma. e proof of the accuracy
of this calculation is to b d in the Athenian demi-
drachma, or Asiatic dra r the fourth part of the
statera of Philip, which ved in the same gallery:
this fragment weighs prec grains. The face of this
small piece of gold pres head of Hercules covered
with a lion's skin, and verse we see the bow, the
vase, and club. M. Mil communicated to me the
weight of five stateræ p in the Imperial library,
which are as follow: No. grains; No. 2. 161 grains
precisely: No. 3. 161 gra . 4. 162 grains precisely;
No. 5. 162 grains. The viest seem to be so from
having been less worn. T st would answer to 175·16
grains of Florence, and lighter by gr. 0·84 than
ours, which ought there be regarded as less worn
and more precise.

Mr. Greaves, in England, weighed two stateræ of Alexander, one of which was 133 English grains, and the other 133 and a half. He thought that the half-grain had been wasted by friction, and he concluded that the drachma ought to be reputed as being precisely at 67 grains. The second weight as given by Mr. Greaves would be equivalent to 87 grains and six-tenths of Florence. Snellius found the statera of Philip and of Alexander to weigh 179 Dutch grains, which makes 124 and a half English; and this, according to the foregoing comparison, would give to the drachma in Florence weight 87 grains 0·9; all of them being a little lighter, but closely approaching that which we had fixed at 88 grains.

Barthelemy, in France, found after various weighings that the drachma was precisely 81 French grains and an eighth: now, by the foregoing comparison, we ought to have for the drachma in Florence weight 87 grains and three-fourths. But this last author wishes to suppose a friction of seven-eighths of a grain for 2,200 years wearing, and he gratuitously makes the drachma to be 82 whole French grains, which would make 88 and a half of Florence weight. It is best to banish entirely from our calculations all suppositions of friction, because, by admitting this to have been the case, we might draw a variety of vague conclusions. The weight of 88 grains which we have assigned to the gold drachma is confirmed by a silver one of this very Philip, also preserved in the Florence cabinet: this piece has on its face the head of Hercules without a beard, covered with a lion's skin; and on the reverse a Jupiter sitting, holding the eagle in his right hand and a spear in his left. It is distinguished from the others by a lyre, and the letter A under the seat. This drachma is also a proof of the exactness of its weight in its half, also in silver, of the same king, which weighs precisely 44 grains: on its face is the head of Jupiter with the diadem; on the reverse is a figure on horseback, with the name on the exergue and a mark which is unintelligible. Besides, there are four pieces of four drachmas of Alexander of the same metal, the face and reverse of which are similar: all of these weigh 14 pennyweights and 16 grains, and prove completely that the weight of the drachma is 88 grains. These tetradrachmas are distinguished in the type by the addition of various signs, as we have said with respect to the staterse: one has in front a lamp, and under the seat a moon and a star: another has in front the letter T with a circumflex accent, and under the seat the letter E: another has in front a buckler, and under the seat a serpent: the fourth has in front a crown, and under the seat a monogram composed of M. Finally, we have also a real drachma of this king of the precise weight of 88 grains, and which is distinguished by a monogram formed by an H, the cross bar of which has a kind of circumflex accent.

Among the tetradrachmas of Thrace there is one in the Florence collection, and the twelfth of the list, heavier than the rest; it weighs precisely 14 pennyweights and 16 grains: here we have a proof of the identity of the weights of the Thracians and Macedonians, as long ago supposed by the learned.

After the weighing of the Arezzo coin was finished, it was submitted to the cupel and to quartation. The standard was found to be the same with Patin's examination, *i.e.* 0·979, or 23 carats and a half: it contained only half a carat, or 0.021, of silver.

The art of assaying was known in the earliest times, as tested by the Holy Scripture: we find it at such adegree f perfection even in the time of Pliny*, that by means f it the standard of gold was fixed at 21 carats, or 0·875, : 21 carats and 7-24ths (0·888) up to 23 carats and 1-32ds (0·973). In those days they must have assayed ı the dry way, first by separating from the gold the viler ıetals by means of lead, and afterwards even the silver, ith sulphur or with the sulphurets†.

They were also acquainted with the method of refining nd purifying gold in large quantities, by cementing or urning it, as Strabo informs us, with an aluminous earth, which by destroying the silver left the gold in a state of
urity. Pliny says that fo urpose they put the gold
n the fire in an earthen ith treble its weight of
alt; that it was afterward exposed to the fire with
wo parts of salt and one of eous *schistus:* this would
urely effect the decompos the salt, and the volatili-
ation of the muriatic aci tate of ignition and dry,
which would penetrate the e of the gold, and would
eparate the silver in the volatile muriate: this is
he object of the process entation among the mo-
lerns‡. But Agatharchide ansmitted to us a peculiar
nethod practised in the mi ted between the Nile and
he shores of the Red Sea, h we recognise the well-
nown property of the m cid in the separation of
ilver.

This writer says (if we st to the text being ac-
urate) that in these place inclosed in marble; that
he miners burn or calcine the ore; that they break it with

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* The art of assaying was certainly very imperfect at these periods: un-
ler the emperors the standard of gold and silver was still judged of by the olour assumed by the coin in the fire, and by the tint given to the touch-tone on which the metal was rubbed.

These methods, although practised by expert workmen, could yield but ery inaccurate results, which a variety of circumstances might influence; uch as a complexity in the alloy, or a different alloy, &c.

Archimedes would not have applied the laws of specific gravity to the letermination of the standard value of king Hiero's crown, if he had nown a better method.

We know also that under the triumvirate of Mark Antony every street of Rome erected a massive statue to Marcus Gratianus, who had discovered nd put in practice one of the processes of assaying mentioned above: this lenotes the infancy of an useful art, the first steps of which were strongly ncouraged as being intimately connected with public happiness.

† By employing the alkaline sulphurets, the solution of gold might have een effected: it is the metallic sulphurets, however, which ought to be used n this process.

‡ M. de Robilant, in his detail of the processes of the mint at Turin, ays that cementation is the mode of refining generally adopted at Venice, Genoa, and at Florence, where they make gold sequins almost entirely pure.

hammers, afterwards pound it, bruise it, and wash it; and finally, that the gold, when put into a close crucible, with a little lead, salt, a little tin, and barley meal, was exposed over the fire for five days.

The money-coiners of Darius certainly employed this method, or a similar one, when this enlightened king wished to give his subjects the noble and useful example of a mint made with the purest gold, similar to that of fine silver, which his satrap Ariander afterwards did.

To conclude:—It is not easy to form an intelligible idea of the docimastic method, which Agatharchides has transmitted to us. But if in the operation which he describes there is no mention made of *cementation*, but of a true and prolonged fusion, it remains to explain how we can reconcile with the object in view, the employment of a close crucible held over the fire, as he describes: far less can we comprehend the use of the barley meal.

When we reflect, however, on the ingenious method described by Hellot as being practised at Lyons, in order to refine, purify, and separate cupelled silver from the small quantity of lead which adheres to it after the first refining, we may perhaps comprehend what is meant*.

In Lyons they use crucibles thirteen inches high, and five broad at their orifice. About three inches deep of pounded charcoal is then put into the crucible and kept down by a lid, or rather a triangular piece of the crucible, which is kept in its place. On this lid or false bottom they put 60 or 65 pounds of long and thin ingots, in order to be melted and purified. The wind furnace employed for this purpose is 14 inches high, seven in diameter at the grate, and nine at the top. The metal in melting was observed to fall three inches from the edges of the crucible: *then, when it had acquired a sufficient degree of heat, it was seen to boil with the force and the agitation of water exposed to the heat of a strong fire;* and in this state it was kept for seven or eight hours.

The elastic fluid, which in this case is extricated from the charcoal at the bottom of the crucible, produces the agitation above alluded to; and it forms, as it were, a kind

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* It would be necessary, before deciding finally on the process detailed by Agatharchides and that which is practised at Lyons, to repeat them, taking the greatest care to apply the modern methods of chemical analysis, and above all the pneumato-chemical apparatus: it would be necessary to determine the nature of the gas, which passes through the melted silver, and ascertain why the gas is formed under a certain pressure, and why it does not pass out through the pores of the crucible. The experiment of M. Fabbroni does not seem to me to be conclusive.

f bellows ingeniously placed at the bottom of the cru-
ible.

We know that charcoal, when put into close vessels of
ietal or glass, is not altered, although it becomes red.
heory dictates this, and several experiments confirm it.
ut the observation of the fact related by the judicious
[ellot also proves, that in this case the charcoal below the
ielted silver is decomposed, and continually furnishes
astic fluid; since this excellent chemist found that silver
ept over a similar fire, without charcoal being placed be-
ıw it, undulated at the surface, going from the centre to
ie edges, and *vice versâ;* but that in fact it does not bub-
le with so much noise: fro nce then does the elastic
uid originate in this case?

Priestley, the founder of modern pneumatic che-
iistry, demonstrates in tl nest manner, what has
een since confirmed by s other experiments, that
ırthen vessels heated u iey admit light to pass
ırough them, are filters, or sieves, which allow even
ie external air to enter; t ɔric and light penetrate
y the bottom of the cru and with them air, at-
acted chemically by the ɔal which is inside: its
xygen coming in contact e charcoal, which is in a
ate of incandescence, infla ıortion of it, is combined
ith itself and with the ca ɔrming carbonic acid, an
astic fluid, which, by the rupted action of the fire,
ıgments and acquires a ıt elasticity to overcome
ie pressure of a column o inches of liquid silver,
hich is above, and passes through it agitating it violently.
he small residue of lead, which is united with and dif-
ısed over the mass, being put by a continual agitation in
ɔntact with the carbonic acid gas, and with the atmo-
ɔhere (the latter and perhaps the former are decomposed
y a greater affinity under certain circumstances), is oxi-
ated, and by the diminution of specific gravity is con-
rained to occupy the upper surface.

In fact, Hellot saw a kind of yellowish oil rise from the
ıside of the silver which floated on the top of the crucible:
iis oil was a pure oxide of lead melted and formed by the
ɔntact of the atmospheric air, which is continually re-
ewed. The refiners collect this melted oxide by absorbing
with glass or with sour earth; this earth being most
ısily removed from off the silver which it covers, and the
ıetal then remains limpid and pure.

On comparing this method with the process of Agathar-
iides, reported here so imperfectly, we may suppose that
the

the barley, or the barley meal, was used instead of charcoal, in order to form what the Lyonnese call the soul of the crucible, which was placed at the bottom of it, where it was retained by a covering (from which probably comes the expression of *closed crucible*), on which the gold in fusion was placed by means of a little lead (in order to vitrify the base metals which it might contain,) and some common salt, sulphuret of antimony or of lead, in order that they may lay hold of the fine silver and volatilize it with the lead, or reduce it to scoriæ. The elastic fluids, extricated from the vegetable matter by the action of the fire, form the office of bellows for incessantly agitating the metal during several days, which makes all the impurities swim above, and which ought to be skimmed off as the Lyonnese do.

Properly speaking, a fire which lasts five days gives rather an idea of the cementation of the moderns, and analogous to that which Pliny has communicated to us, than a real fusion in close crucibles,—a circumstance which would be directly contrary to the object in view. Thus, in Hungary, in order the better to open all the interior parts of the gold to the muriatic acid reduced into vapour in the cementation, they are accustomed to add lead to the mass, which is afterwards reduced into small hollow drops, or, in other words, into grains. It may be that the lead indicated by Agatharchides has the same object: the tin may have been taken by an equivoque for crude antimony, for galenum, or for the native sulphuret of lead: it is possible likewise that the barley meal was merely intended to serve for the equal distribution of the little salt, a stratum of which must be placed on the gold, and perhaps assisted to decompose it, as argil or sulphate of iron now does.

In order to obtain some light upon this curious subject, there were put into a crucible covered by another crucible turned upside down, thirty pennyweights of barley-meal, and an ounce of common salt: it was then made red-hot, and kept 36 hours in this state. There was put in, more for the sake of curiosity than any thing else, a thin piece of gold weighing 24 grains, and a piece of silver weighing 40 grains. The lower crucible was half full, and an opening was left at the joining of the two crucibles to let out the elastic vapour.

After this space of time, the apparatus being cooled was opened, and there was found a small earthy residue slightly saline and whitish, weighing eleven grains and a half. The gold was above; it was increased one-eighth of a grain in weight,

ight, because it was evidently whitened by the fusion of
y minute particles of silver detached from the small
gment of silver, which was then found adhering imme-
tely over the gold, in the form of agglutinated dust:
s fragment of silver weighed six grains and one-eighth.
e afterwards boiled this gold (which was silvered only
its surface) for some time in pure nitric acid; when it
t completely its silver colour; and when assayed it was
ind to be 24 carats.

We proceeded afterwards to examine the small earthy
idue, in which we found, in saline particles, only a
atoms of muriate of soda, and scarce a trace of muriate
copper. The muriate of hich, on account of the
s suffered by the piece of ught to have formed a
ight of 45 grains and a ha ertainly evaporated with
other elastic vapours. rains and a half only
muriatic acid concurred rmation of this muri-
: the thirteen pennyweig a half of the same acid,
ich besides contained tl non salts employed in
s experiment, have theref dissipated (by not pay-
attention to the little c by a decomposition ef-
ted by means of the vege tter which was joined
h it: but what is difficul ount for, and which is
eign to our object, is the tion of ten whole pen-
weights of soda, contai he common salt, and
ich ought to have remain at the bottom of the
sel: it had therefore bec atile, either by decom-
sition, or by a new comp and it had escaped by
aperture in the apparatus.

It is not probable, therefore, that Philip made use of simi-
methods of refining, either by flux or by cementation,
ause, we repeat, he would have reduced the gold to a state
perfect purity, as Darius wished to do subsequently; or
would not have limited himself to so small a portion of
oy, or perhaps this alloy would not have been silver. And
e employed gold in the state in which he found it, we
ist be forced to admit that nature furnished it at 23 carats
a half fine, or of the standard of (0·979*).

Many

Reaumur says (Mémoires de l' Académie des Sciences 1718, p. 87,) that
gold brought from the bed of the river Ceze is at the standard of 18
its eight grains.

old of the Rhone	..	..	..	20 carats.
of the Rhine	..	..	..	21¼
of the Arriege	..	..	..	22¼

eaumur also observes, that the standard varies in the same piece of na-
tive

Many persons will probably doubt that gold is found in nature so near a state of perfect purity, although Strabo intimates that perfectly pure gold was found in the Alps, and Pliny is cited as asserting that no silver was ever found in it. But without remaining in suspense with respect to the assertions and opinions of others, I am enabled from my own experiments to remove all uncertainty, having ascertained that native gold is 24 carats (1000).

I had for some time the charge of the fine collection of natural history belonging to our sovereign, who was fond of that science. His majesty possessed many specimens of mineralized gold and native gold, among which I remarked two well formed crystals of gold, viz. one cubical, the other prismatic with four faces, surmounted by a pyramid with four faces also. It would be interesting to know what substances united to the gold have determined these various figures formed naturally in the bowels of the earth, and which are totally different from those which are produced in the laboratories of the chemist after melted gold is cooled. The cube is very pale, the prism is higher coloured; but these two crystals, which I found by chance, (when choosing among several natural grains,) are unique in this depôt; so that it would be improper to subject them to an examination which would alter their form. An unshapen specimen, but at the same time a remarkable one, from the Brazils, enriches this collection. The weight of this piece is about fourteen pounds; and I examined a bit of it by the cupel and by quartation, without omitting also to examine its solution in the nitro-muriatic acid, with the sulphate of iron, and with neutral salts with a base of potash. I have been convinced by all these operations, that this was pure gold of 24 carats without any alloy of inferior metal.

From all that I can learn, therefore, it appears that gold is found in a native state in various degrees of purity, and to prove this has been the object of the present dissertation.

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tive gold. M. Deluc informs us that the gold found at Wicklow in Ireland was alloyed with a ninth part of its weight in silver.

M. Fabbroni is the first who has demonstrated that native gold is found in a state of purity: this is a most important observation; but it does not seem to destroy the idea that native gold is a natural alloy of gold and silver: a principle established by a great number of facts, and to which we as yet know but one exception.

It would be interesting to ascertain whether lead is present in ancient coins or medals: this would be the surest method of determining whether the ancients refined their gold, or employed it as nature presented it.

XXIII. An

XIII. *An Estimation of the Loss of Weight which takes place in cooking Animal Food.*

is well known that, in whatever way the flesh of ani-
als is prepared for food, a considerable diminution takes
ace in its weight. We do not recollect, however, to have
en any where a statement of the loss which meat sustains
the various culinary processes, although it is pretty ob-
ous that a series of experiments on this subject would not
without their use in domestic œconomy.

We shall here give the result of a series of experiments
ich were actually made o ubject in a public esta-
shment, premising that, y were not undertaken
m mere curiosity, but, on ntrary, to serve a pur-
se of practical utility, abs curacy was not attended
. Considering, however, t quantities of provisions
ich were actually examin s presumed that the re-
lts may be safely depende for any practical pur-
se. It would no doubt been desirable to have
own not only the whole tion of weight, but also
e parts which were separa n the meat in the form
aqueous vapour, jelly, fa ut the determination of
ese did not fall within th of the inquiry.

	lbs.	ozs.
28 pieces of beef weighin	280	0
Lost in boiling.....	73	14

Hence the weight lost by boiling was in this case
out 26½lbs. in 100lbs.

	lbs.	ozs.
19 pieces of beef weighing	190	0
Lost in roasting	61	2

The weight lost by beef in roasting appears to be 32 per
nt.

	lbs.	ozs.
9 pieces of beef weighing	90	0
Lost in baking	27	0

Weight lost by beef in baking, 30 per cent.

	lbs.	ozs.
27 legs of mutton weighing	260	0
Lost in boiling, and by having the shank-bone taken off....................	62	4
The shank-bones were estimated at four nces each ; therefore the loss by boiling was	55	8

The loss of weight in legs of mutton, in boiling, is 21¼
r cent.

35 shoulders

	lbs.	ozs.
35 shoulders of mutton weighing	350	0
Lost in roasting	109	10

The loss of weight in shoulders of mutton, by roasting, is about 31⅓ per cent.

	lbs.	ozs.
16 loins of mutton weighing	141	0
Lost in roasting	49	14

Hence loins of mutton lose, by roasting, about 35½ per cent.

	lbs.	ozs.
10 necks of mutton weighing	100	0
Lost in roasting	32	6

The loss of necks of mutton, by roasting, is about 32⅓ per cent.

We shall only draw two practical inferences from the foregoing statement:—1st. In respect of œconomy, it is more profitable to boil meat than to roast it. 2dly, Whether we roast or boil meat, it loses, by being cooked, from one-fifth to one-third of its whole weight.

XXIV. *Letter from M.* Vitalis, *Professor of Chemistry at Rouen, to M.* Bouillon Lagrange, *on the Amalgam of Mercury and Silver, called* Arbor Dianæ[*].

The process described by Baumé, and which is generally adopted for obtaining the amalgam of mercury and silver, known in chemistry by the name of *arbor Dianæ*, is not the only one which is capable of exhibiting the beautiful crystalline forms which characterize this curious production. I attained the same object, by modifying the common method; and this modification admits of our extracting with facility the metallic vegetation from the liquor, and preserving it, without any alteration, out of the vessel in which it has been formed.

The operation is very simple. In the nitric solutions of mercury and silver, both being well saturated and diluted with the quantity of water prescribed by Baumé, I suspend a small knot of fine linen rag doubled up, and containing five or six drachms of very pure mercury.

The metallic solutions soon penetrate to the mercury, which is inclosed in the rag, and we soon see some beautiful spiculæ formed and grouped around the rag, adhering to the nucleus of mercury, which serves as a kind of support to them.

[*] *Annales de Chimie*, tome lxxii. p. 93.

These spiculæ progressively increase
short time exceed an inch in length.

When we perceive that the metallic
no more progress, the piece of rag wit
be withdrawn from the liquor, and b
thread fastened to the cork of the bott
be suspended under a bell glass. The c
tetrahedrons, may thus be kept as long a

I have in my own laboratory a crys
description, which has preserved all its
these two years past.

It may be easily seen that, in the abov
of at le different from th
as c Fourcroy in *Systême*
chi

T metallic crystals ob
tho the softness of the t
blag s the common *arbor*
thin rtions of mercury
the ses; and I would ha
hav difference, if M. Vau
cor rcumstance, had not
ter the subject at full le
ing n some future num
de

T of the above crys
son uiries, which I ma
to a ture time.

XXV. *Analysis of the Atropa Belladonn*
QUELIN*.

The experiments which I am about to
with a view to ascertain whether this p
the same family with the tobacco plan
acrid principle which has been found i
which, as will be shown in the sequel, it

1. The expressed and filtered juice of t
a dark brown colour, with a bitter and n
is freely coagulable by heat, and by the ac
gall nuts.

2. The substance coagulated by fire in
donna is of a yellowish gray, becomes bla

---

* *Annales de Chimie*, tome lxxii. p

and presents a smooth and polished fracture like that of the resins. It burns with decrepitation, becomes soft, and gives out vapours of the smell produced by horn when subjected to the same operation.

3. The juice of belladonna, distilled until reduced to the consistence of liquid extract, only furnished a water which had a fetid, herbaceous taste, and by no means the acerbity of that of tobacco. The only re-agent among all those resorted to, which slightly disturbed it, was acetate of lead.

4. The juice concentrated to the consistence of extract having been treated by alcohol, a part was dissolved: the solution deposited upon cooling, crystals of nitrate of potash and a little muriate of potash.

The alcohol separated from these crystals of nitrate of potash, and evaporated, left as a residue a brownish yellow matter of an extremely bitter and nauseous taste, which, on being taken up again by alcohol well dephlegmated, left a new quantity of insoluble matter, and also deposited some crystals of the same salt.

The matter cleansed as much as possible by the above process, from the greatest part of the saltpetre and from the substance insoluble in alcohol, I evaporated the latter, and submitted the residue to the following experiments:

1. It is dissolved abundantly and speedily in water, and it is even deliquescent in the air.

2. The solution is of a yellowish brown; it has a very bitter and disagreeable taste.

3. It reddens in a very intense manner turnsole paper.

4. It is precipitated in abundance by the alcoholic solution of gall nuts, and is not so by the acetate of lead when the latter is sufficiently diluted in water; because, as this matter contains a little muriate of potash, it would precipitate the acetate of lead without this precaution.

5. This solution mixed with sulphuric acid diffused a very sensible smell of acetic acid.

6. The same solution is precipitated by the nitrate of silver in a true muriate of silver.

7. Caustic potash develops in the solution of this substance a fetid smell, very like that of an old ley which has passed over linen and begins to turn putrid: ammoniacal vapours also rise, which may be made perceptible by weak nitric acid, presented at some distance from the mixture.

8. The addition of some drops of sulphate of iron gives a much deeper colour to the solution.

9. The extract itself exposed to burning coals, bubbles

up, and exhales pungent acrid vapours,
cannot be distinguished.

We may conclude from the effects
lution of the extract of belladonna by th
above employed: 1st, that it contains a f
kaline muriate; 3d, a small quantity of a

The nature of the acid which exist
can be nothing but acetic acid, since
develops the smell of it, and the acetat
form any precipitate in it, which would
malic, tartarous, or oxalic acid. A par
to be combined with the potash, and
this hich communicates
mass f attracting humidity

B these salts nor thes
poiso to the matter; these
the ance itself: what th
comp thus forms out of
both ur poison? This is
chem yet overcome, and
behi that secrets the most
man .

F neans of ascertaining
feren between vegetable
prop metrically opposite, we
to th .

One means resorted to as t
guiding us he nature of the subs
soluble in alcohol, was its decompostion
introduced therefore two grammes and
glass retort, and administered the heat
water of solution was distilled: there
ammoniacal liquid, afterwards a thick
singular disagreeable smell.

The examination of the liquid prod
recognise a good deal of ammonia, pa
combined; for the addition of some dro
rendered the ammoniacal smell much s
was thick, black, and very acrid.

The charcoal remaining in the retort
me, and had an alkaline and *prussiated*
in boiling water, it yielded a ley which
sulphate of iron furnished a quantity of
considerable with respect to the small
employed. This charcoal after having
dried still weighed $\frac{7}{10}$ of a gramme.

The above quantity of charcoal, independently of that which was incrusted in the retort by the violence of the fire, and which I could not detach, is more considerable than any furnished by most of the other vegetable matters which I have hitherto had occasion to distill; for the 2·7 grammes of extract, in the state in which I took it, certainly contained more than 0·7 of a gramme of water and of nitrate and acetate of potash.

It seems that it also contains a great quantity of azote and of hydrogen, since it gave on distillation a great deal of ammonia, prussic acid, and oil. But as this matter may have contained a little nitrate, I supposed that a part at least of the azote, forming the ammonia and the prussic acid, had been produced by the nitric acid.

In order to clear up this doubt, I mixed six grammes of gum arabic, believed not to contain any azote, with a tenth part of saltpetre, and after submitting it to distillation I examined the products. The liquid which passed was in part ammoniacal, and its smell became still stronger by the addition of potash; which proves that an acid was formed at the same time with the alkali.

The charcoal remaining in the retort, weighing two grammes, and which was extremely phosphoric, contained prussiate of potash, like that of my matter. But although I employed in this experiment three times more gum, and probably more saltpetre, this mixture did not furnish such a great quantity of ammonia or prussic acid as the nauseous principle of the belladonna did.

Taking it for granted, therefore, that the saltpetre contained in the two grammes of this principle had given rise to prussic acid and to ammonia, we ought not to infer that the vegetable matter in question has not furnished some itself. This is the more probable, as its solution is precipitated by the infusion of gall nuts. However this may be, the experiment proves that it is difficult to judge by distillation, whether the organic matters which contain saltpetre are of a vegetable or animal nature.

The results of this analysis, although still very imperfect, are nevertheless sufficient to show that the article in question contains a considerable quantity of charcoal, hydrogen, and azote, and but little oxygen, if we may judge by the small quantity of carbonic acid which is formed during its decomposition in the fire.

From what has been said, may we be permitted to infer that the narcotic effects of belladonna on the animal œconomy are owing to the superabundance of the radical combustibles,

bustibles, and particularly to that of the
of the oxygen in the principle of this pla
hol ?—Without going the length of posi
it is nevertheless certain that all the ve
which produce analogous effects are rich
drogen, and azote, whereas substances th
genized produce contrary effects.

It must also be admitted, that many
equally abundant in these two principles
same virtues; but the azote, which is a
ciated with hydrogen and carbon in th
does not exist, at least in the same quantit
oth

Exa *Part of the Belladon*
ble in Alcohol.

1. dissolved in water
it th thing when agitated
2 abundantly precipita
infu
3. of barytes into a ma
sol acid.
4 of lime into a prec
lubl id.
5 eddens turnsole pap
6. silver produces no e
7 a crucible, it leav
hep

We may conclude from these effects, t
belladonna is composed of an animal m
potash, of acidulated oxalate with the sa
some nitrate, and that it contains no mur
conclude from these effects, that there ar
it, since the muriate of lime forms in
well as the nitrate of barytes.

I have ascertained by some experimen
that the precipitates produced in the so
stance in question by the nitrate of ba
first instance oxalate of lime, and in
of barytes.

The oxalate of lime had taken up wit
of animal matter, which gave it a brow
dicates that this salt has a strong affini
ters, and explains the reason of mural c
all know, are composed of oxalate of l
much deeper colour than the other spec

After having successively precipitated, as I have already said, the sulphate of potash and the acidulated oxalate of potash, I evaporated the liquor, which was always coloured, and which contained nitrate of potash and muriate of lime, and I treated it with the nitric acid in order to ascertain if it contained gum; but not having obtained an atom of saccho-lactic acid, I concluded that this substance does not contain gum. It is merely formed of oxalic acid and a yellow matter. This substance seems therefore to be entirely of an animal nature.

From what has been said above, we find that the juice of belladonna contains the following substances:

1. An animal substance, which is partly coagulated by heat, and partly remains in solution in the juice, in consequence of the free acetic acid which exists in it.

2. A substance soluble in alcohol, which has a bitter and nauseous taste, which on being combined with tannin becomes insoluble, and furnishes ammonia by its decomposition in the fire.

3. Several salts with a base of potash, viz. a good deal of nitrate, muriate, sulphate, acidulated oxalate, and acetate.

The refuse or husks of the belladonna, from which the juice had been extracted, having been washed in warm water, dried, and afterwards burnt, furnished ashes composed of a considerable quantity of lime, phosphate of lime, iron, and silex.

This lime announces that the plant contained oxalate of lime, which had been decomposed by the fire. It is by no means doubtful, that that part of the belladonna which is soluble in alcohol is not the only substance which in this plant produces a deleterious effect on the animal œconomy; for it is the only one which has any taste; and the well known effects of all the other substances which accompany it have nothing in them resembling those of the plant in question.

To put this assertion beyond all doubt, I administered to a dog a certain quantity of this principle mixed up with crumbs of bread.

First Experiment. About mid-day, I gave the animal a gramme of extract inveloped in ten grammes of paste.

Symptoms. In about three quarters of an hour the animal seemed inclined to sleep; he held down his head, and seemed unable to keep it up: he laid his head on the ground several times, and slight convulsions agitated his legs: his jaws also moved as in the act of chewing. These effects lasted about three quarters of an then resumed his former appearance.

Second Experiment. At two o'clock grammes of extract with twelve of paste ptoms re-appeared, but they were feebl duration.

Third Experiment. At three o'clock, low four grammes of the same extrac grammes of paste.

A few minutes afterwards he was seize but uncertain and difficult motion, chiefly region: he uttered some plaintive moans

At half past three he experienced great ing, and frequently fell on his hind fee was much obstructed. He endeavoure force his way through the wall, which i delirium: he was now seized with tre muscles.

At a quarter past four the animal lay d to be plunged in a profound sleep; his p peated faster than could be counted.

At half past four he vomited the pa taken, some time after which he rose up; with difficulty, sometimes falling on on times on his crupper. He held his h eyelids fell, and he did not distinguish continually ran against the walls, and t laboratory: his nose was no longer affe of ammonia, and his ears seemed also functions, for the loudest noise made him.

He had not lost his memory, however; him in the same posture in which he wa the paste, in order to give him some vin became furious, as if all his powers were From this moment, the symptoms whic insensibly diminished, and about eight o' ing he recovered all his senses; but he tigued. Next day he ate as usual.

Every one must recognise in the ab effects of narcotics, and drunkenness ca pitch, from which resulted a kind of de bable that if the animal had not vomited the matter before it produced its effects,

XXVI. *Case of Hydrocele, improperly treated as Rupture. By* John Taunton, *Esq., Surgeon to the City and Finsbury Dispensaries, and to the City Truss Society for the Relief of the Ruptured Poor.*

To Mr. Tilloch.

Sir, It is not the least of the evils which accompany a state of disease among the poorer classes of this large metropolis, that their complaints are frequently misunderstood, and consequently treated in a manner which tends to increase rather than to alleviate their sufferings. The superficial and hasty view which is but too often taken, even by regular medical practitioners, of the diseased victim of poverty on the one hand, and the allurements held out by mercenary and ignorant pretenders to medical skill on the other, are the causes of this additional affliction to the poor.

Those who officiate as medical officers to the numerous public charities which do honour to this great city, have daily opportunities of witnessing the melancholy effects of the errors thus committed. The following case of this kind, which occurred lately uuder my own inspection, and which had nearly terminated fatally to the patient, is one of the many illustrations of this observation which may be adduced.

Thomas Erskine, æt. 53, servant to Mr. Thomas Butcher, of Charing Cross, a few years ago received a kick in the scrotum, which occasioned a swelling, and which has continued ever since. At first it was attended with extreme pain; but this soon ceased, and the tumour assumed an indurated appearance. The poor man applied to two regular surgeons in his immediate neighbourhood, who informed him that his complaint was a rupture, and recommended a truss. Attracted by an alluring advertisement from some truss-makers in Soho, the patient applied to them: these gentlemen, after examining the patient, and affecting a great deal of medical and anatomical knowledge, confirmed the opinion of the surgeons, and applied a truss to the tumour, for which they charged the exorbitant price of a guinea. This happened three years ago, and the patient has ever since worn the instrument thus applied, with more or less inconvenience. A few weeks ago he was admitted a patient at the City Dispensary, when on examining him I found the case to be a decided hydrocele. The operation of tapping was immediately performed, and the patient in

a few days was restored to his former
Pilulæ rhei cum terebinth. formed the
which I found necessary to administer.

I am, &c

Greville Street, Hatton-Garden, Jc
August 24, 1810.

XXVII. *Proceedings of Learned*

ROYAL SOCIETY.

THE experiments detailed in Mr. Davy'
the r which we gave a b
last highly interesting, th
be ı ain bringing the sı
reade uring to present the
cise y form. But before p
we m lers to correct two ty
in oı n page 71, line 20, f
read l in line 22, for "
read
Tl rawn by Mr. Davy f
facts valuable paper is eı
to ex ten the theory of ch
great ny of the brilliant di
made tigable philosopher.
are th o which we allude :-
1st. tic acid is (as far
extends) a *simple substance*, which may
same order of natural bodies as oxygen
mined like oxygen to the positive surfac
binations, and like oxygen, combining
substances, producing heat and light.

2dly. That its combinations with ir
are analogous to oxides and acids in the
powers of combination, but they differ fı
for the most part decomposable by water

3dly. That hydrogen is the basis of ı
and oxymuriatic acid its acidifying princ

4thly. That the compounds of phosph
&c., with oxymuriatic acid, approach i
acids, and neutralize ammonia and other

5thly. That the combination of amı
phorus acidified by oxymuriatic acid is
pound, having properties like those of aı
decomposable at an intense red heat.

8thly. That oxymuriatic acid has a stronger attraction for most inflammable bodies than oxygen; and that on the hypothesis of the connexion of electrical powers with chemical attractions, it must be highest in the scale of negative power; and that the oxygen which has been supposed to exist in oxymuriatic acid has always been expelled by it from water or oxides.

FRENCH NATIONAL INSTITUTE.

The readers of the Philosophical Magazine must have seen from the accounts which have lately appeared in our pages, of the labours of the French chemists, that those gentlemen had questioned the accuracy of the inferences drawn by Mr. Davy from the numerous experiments he had made, in the course of his electro-chemical researches, respecting the nature of the alkalies and the earths; maintaining that the metallic bodies obtained from these substances, in place of being simple, as asserted by Mr. Davy, were compounds of the respective alkalies and earths with hydrogen; or, in other words, that the new bodies were *hydrurets*. Of this opinion were Gay Lussac, Thenard, and most of the French chemists. Berthollet among the rest warmly contested the correctness of Mr. Davy's inferences, and maintained the accuracy of the French conclusions. They have now, however, changed their opinion, and done justice to our countryman.

At a meeting of the French National Institute in the end of June, Messrs. Gay Lussac and Thenard read a notice containing the results of a great variety of experiments on the new metals; from all of which they conclude, after a most rigorous investigation, that professor Davy was perfectly correct in his inferences, and, with a degree of frankness honourable to themselves, renounce their former opinion that these new metals are hydrurets.

We cannot but take notice here of an assertion made in the Report of the Labours of the Institute, (published in a former volume of the Philosophical Magazine,) which savours of a blundering, but probably not intended, plagiarism. The Report states, that Messrs. Gay Lussac and Thenard discovered the mode of metallizing ammonia by potassium; whereas these gentlemen themselves, who have more than once uncandidly assumed Mr. Davy's facts, acknowledge this to have been that gentleman's discovery, in their paper on ammonia.

The result of the present contest, we cannot but hope, will serve as an admonition to the editors of some of our

periodical works, not to be hasty to com
questions of science, on the authority of
by jealous rivals for national fame. V
most respectable journal which has fallen
As to some more obscure writers, who ha
about "the *pretended discovery* of the
the alkalies," they will probably show a l
in their remarks in future.

XXVIII. *Intelligence and Miscellan*

M. lirector of roads a
king discovered a metho
wood ch in point of stre
promi f several centuries.
marka ance of their form
the ar has been construc
plan c ith, five leagues fron
ing two hundred fe
has b arge city, two hund
feet hes may be so cons
mit c or merchant vessels
them, ng made in the cen
opene asure. Another ad
by the at of being speedily
if it b stop the progress
arch may be ...oved in one day, and
another, without cutting the smallest piec

With respect to the advantages in p
resulting from the adoption of M. Wiebel
been estimated that a stone bridge of simi
a wooden one of a given size would co
florins, whereas the latter would cost on
and on the supposition that a wooden bri
100 years, it follows that, taking the int
cipal sum into the computation, there w
ing of eleven millions six hundred and
florins.

The Pharmaceutical Society of Paris h
following as prize questions for the prese

1. Ascertain as far as possible, wheth
vegetables an identical principle which cl
signated by the name of extractive?—O

the ancient classification adopted for pharmaceutical extracts, divided, according to Rouelle, into gummy, resinous, gummo-resinous, resino-gummy, and saponaceous extracts?—Can a more methodical and more exact classification be established by means of chemical experiments made on the principal substances in pharmacy furnished by extracts?—Indicate, according to the nature of their different constituent principles, the mode of preparation best adapted for each, and the nature of the menstrua which ought to be employed."

2. "What is the present state of pharmacy in France? what rank does it hold in the healing art? and what are the ameliorations of which it is susceptible?"

The prize offered for the best memoir on the first question is a gold medal of the value of 200 francs. That offered for the best paper on the second question is of the value of 100 francs. The memoirs to be transmitted to Paris on or before the 1st of October 1810.

The following account of a new optical instrument is extracted from a recent French journal: "It is well known that the art of perspective consists in representing on a plane surface objects in the position in which the eye perceives them. Descriptive geometry furnishes the means of doing this; but the method which it teaches presupposes science, and demands time. The painter, without having recourse to geometry, draws on a simple purview from habit and practice in his art. However excellent his eye, and however skilful an artist may be, he cannot flatter himself with obtaining geometrical precision. A new instrument has therefore been invented, by means of which every draftsman, without knowing the rules of perspective, may design with ease and correctness all kinds of subjects on every scale not exceeding 5 decimetres square. This invention belongs to M. Roggero, of the Conservatory of Arts and Manufactures.

"Some very ingenious instruments have been already contrived with this view, and among others that of Mr. George Adams, who has been peculiarly distinguished. But from the great number of joints of which the mechanism is composed, all of these instruments were more or less liable to disadvantages, which M. Roggero's instrument has overcome. He has also united solidity to precision in the transmission of the movements, besides having furnished his instrument with an achromatic glass, by means of which we may trace the perspective of objects placed at a distance.

On digging lately at Frescati in Italy,
ancient Tusculum, a quantity of arms, va
and a broken statue were found. The la
that of a Roman consul; and a few days
statue was found resembling that of a Ro

M. Vincenzio has lately published at
tific works: one entitled *Lettere scientifi*
Spiegazione di due fascetti di gemme a
author has also written a dissertation,
colossal horses of the Quirinal have
their places. and that they have been in
origi

ICHTHYOLOGY.

Mr. fishmonger in Ca
sent in a pilot fish, the or
that these coasts.—The
of th mprehends sevente
war d and twenty specie
the l Atlantic, chiefly to
The omewhat like that o
hea oth, and the snout
stan outh. It has two s
head ng along the back fr
tail; the belly, of similar
lour wnish, changing in
are se black belts. Mar
this fish frequently accompanies their ve
see it generally towards the fore part of t
gine it is employed in guiding and tracin
whence it has received its name. Pro
amusing itself, or pursuing its prey. It
the dog-fish and the shark; and swims
foot and a half from the snout of the l
its movements, and seizes with address
spoil which the shark allows to escape.
as not to exceed six inches, it will keep p
their swiftest course.—The one in Mr. F
was caught a few days ago in the Solway

A species of hemp, manufactured fro
particular kind of palm, which abound
and its neighbourhood, has recently b
country; and being made into cord, su
ments calculated to ascertain its stren

with the same length and weight of common hempen cord, the result was very satisfactory—it being found that hempen cord broke with a weight of 43lbs. three-fifths, while the African cord did not give way to less weight than 53lbs. two-fifths, making a difference in favour of the latter of 10lbs. in 43lbs.

LECTURES.

Theatre of Anatomy, Blenheim-Street, Great Marlborough-Street.

The Autumnal Course of Lectures on Anatomy, Physiology, and Surgery, will be commenced on Monday the 1st of October, at Two o'clock, by Mr. Brookes.

In these Lectures the Structure of the Human Body will be demonstrated on recent subjects, and further illustrated by Preparations, and the functions of the different organs will be explained.

The Surgical operations are performed and every part of Surgery so elucidated as may best tend to complete the operating Surgeon. The art of Injecting, and of making Anatomical Preparations, will be taught practically.

Gentlemen zealous in the pursuit of Zoology will meet with uncommon opportunities of prosecuting their researches in Comparative Anatomy.

Surgeons in the Army and Navy may be assisted in renewing their Anatomical Knowledge, and every possible attention will be paid to their accommodation as well as instruction.

Anatomical Conversationes will be held weekly, when the different Subjects treated of will be discussed familiarly, and the Students' views forwarded.—To these none but Pupils can be admitted.

Spacious Apartments, thoroughly ventilated, and replete with every convenience, are opened all the Morning, for the purposes of Dissecting and Injecting, where Mr. Brookes attends to direct the Students, and demonstrate the various parts as they appear on Dissection.

An extensive Museum, containing Preparations illustrative of every part of the Human Body, and its Diseases, appertains to this Theatre, to which Students will have occasional admittance.—Gentlemen inclined to support this School by contributing preternatural or morbid parts, subjects in Natural History, &c. (individually of little value to the possessors) may have the pleasure of seeing them preserved, arranged, and registered, with the names of the Donors.

Terms.

For a Course of Lectures, including the I
For a Perpetual Pupil to the Lectures and D

The Inconveniences usually attending
vestigations are counteracted by an antis
result of Experiments made by Mr. Br
Subjects, at Paris, in the year 1782, the
was delivered to the Royal Society, and
of June 1784. This method has since
proved, that the florid colour of the mu
and even heightened. Pupils may be acco
House. Gentlemen established in Practi
newing :al Knowledge, may
with a Dissect in privately

Th , Greville-Street, I

Mr. commence his Aut
Lecture , Physiology, Pathol
on Sa 6th, at Eight o'cloc
precis inued every Tuesda
Satur hour. In this Cou
is p comprehensive view
and Living Body, and
cause treatment of surgi
the m ing the different su
An a ity for professional i
also b the attendance of the I
so incl Finsbury and City Disp
Mr. Taunton urgeon. Further partic
on application to Mr. Taunton, at his h
street.

St. Thomas's and Guy's Hosp

The Autumnal Courses of Lectures at
Hospitals will begin the first of October,

At St. Thomas's. Anatomy, and the O
gery, by Mr. Cline and Mr. Cooper.-
Practice of Surgery, by Mr. Cooper.

At Guy's. Practice of Medicine, by D
Dr. Curry.—Chemistry, by Dr. Babingt
and Mr. Allen.—Experimental Philosoph
—Theory of Medicine, and Materia Medi
and Dr. Cholmeley.—Midwifery, and Dis
and Children, by Dr. Haighton.—Physio
the Animal Œconomy, by Dr. Haighton.
Diseases of the Teeth, by Mr. Fox.

N. B. These several Lectures are so ar

two of them interfere in the hours of attendance; and the whole is calculated to form a complete Course of Medical and Chirurgical Instruction. Terms and other Particulars may be learnt at the respective Hospitals.

Dr. Clarke's and Mr. Clarke's Lectures on Midwifery, and the Diseases of Women and Children.

The Winter Course of these Lectures will commence on Friday the 5th of October, at the house of Mr. Clarke, No. 10, Upper John-Street, Golden-Square.

The Lectures are read every day from a Quarter past Ten o'clock in the Morning till a Quarter past Eleven, for the convenience of Students attending the Hospitals. The Students will have Labours when properly qualified. For particulars apply to Dr. Clarke, No. 1, New Burlington-Street; or, to Mr. Clarke, No. 10, Upper John-Street, Golden-Square.

Mr. Stevenson, of Great Russel-Street, Bloomsbury, who as Pupil is intimately acquainted with the Practice of the late Mr. Saunders, is preparing a practical Work on a frequent Disease of the Eye, which we understand is nearly ready for publication.

LIST OF PATENTS FOR NEW INVENTIONS.

To Joseph Charles Dyer, of Boston, North America, now residing in Westminster, a patent (in consequence of a communication made to him by a certain foreigner residing abroad) for certain machinery for cutting and heading of nails and beads of all kinds and sizes, from strips or plates made of iron, copper, or any other metal capable of being rolled into plates.—July 26, 1810.

To Thomas Wade, of Nelson Place, Kent Road, in the county of Surry, gent., for his method, or process, of imitating lapis lazuli, porphyry, jasper, the various sorts or kinds of marble, and all other stones usually wrought, carved, sculptured, or polished; also inlaid or Mosaic work to be used for or in the formation or manufacture of chimney pieces, slabs, funeral monuments, and for every other purpose to which such stones and marbles are, or may be, applied.—July 26.

To Edgar Dobbs, of the Borough of Southwark, gentleman, for a variety of compositions for making a waterproof cement mortar and stucco, the same being also applicable as durable colouring washes for buildings.—August 2.

METEOROLOGICAL TABL

BY MR. CAREY, OF THE ST

For August 1810.

Days of Month.	Thermometer. 8 o'Clock, Morning.	Thermometer. Noon.	Thermometer. 11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's
July 27	60	62°	57°	29·50	10
28	60	57	55	·63	0
			56	·80	38
			55	·68	36
			54	·80	28
Aug			56	·82	20
			58	·95	42
			57	·70	26
			58	·54	33
			56	·64	45
			55	·64	46
			56	·62	30
			57	·70	29
			56	·95	32
			57	·72	39
			55	·57	41
			58	·81	36
			59	·71	44
14	60	69	55	·69	33
15	59	63	54	·50	36
16	50	50	49	·60	0
17	51	61	49	·92	65
18	49	66	51	30·20	63
19	50	61	52	·21	0
20	51	69	54	·28	60
21	56	69	58	·26	53
22	56	70	59	·03	50
23	58	74	64	·02	50
24	60	74	65	·01	61
25	61	76	64	29·99	59
26	60	71	68	30·01	48

N. B. The Barometer's height is taken at o

ERRATUM.

Our readers are requested to correct the following t
the name of the author of the valuable paper on tunn
of the present volume. For LENNOX *read* LENNON.

XXIX. *A Sketch of a History of Pus.* By GEORGE PEARSON, *M.D. F.R.S. Senior Physician of St. George's Hospital, &c. &c.*

I WAS induced to write this historical sketch for three purposes: *namely*; 1st, To inform myself of the facts already published on the subject. 2d, To, perhaps, assist some others with information. 3d, To manifest whether or not my own investigation had produced any accession of knowledge.

The word *pus*, so very commonly used in our language, is plainly the Greek word *πυος* or *πυον* abbreviated by the Latins, with the change of writing in Roman characters. It appears from the original writers that this term denoted any thick, white, opake, clammy, animal fluid, such as the matter of abscesses, and of ulcers or sores; also the thick milk called *colostrum* secreted immediately after parturition. I am of opinion that philological investigations are unsuitable in a writing of the same kind as this now offered; yet I think it may be useful or even necessary to remark, that the etymological import of the word *pus*, is that of *putridity* or *corruption*; which denotes a state of broken down texture, not only of animal and vegetable matters, but of any mineral substances whatever, such as stones and metals*. Accordingly the word *pus* appears to have signified, among the Greeks and Romans, an animal fluid from matter in a state of broken down aggregation, or of corruption; and such were the fluids above named. Hippocrates distinctly describes the pus of abscesses and ulcers from its simple, obvious properties; viz. it is a *thick*, *white*, *inodorous*, *uniform*, *smooth* fluid—when it is of a good or "laudable" kind. But according to its variations from these properties, it was asserted to be of a bad kind. It is especially said that good pus has not the least offensive smell. It was considered among the ancients to be of great moment to know the properties of this fluid, particularly for the purpose of determining whether or not the sputum in pulmonary disorders was produced by an ulcer

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* The acceptation of the term *putridity*, and of its derivatives according to their confined meaning since the cultivation of chemistry as a distinct science, and not according to the original extensive sense, is one of the causes of the erroneous doctrines of fevers called *putrid*, which disgrace the pages of some of our most eminent writers. The modern meaning of *putrefaction* being that of the process of new compositions and decompositions in animal or vegetable substances effected by chemical attractions, and characterized by fœtor, it is apparently incompatible for matter in a living state to exist during such chemical agencies.

or abscess. On that account mention i
with water and fire. Pus sinks, it is ob
mere water, but in salt water; while the
stance of the bronchial membrane call
and floats on water:—the former subst
fusible through water, but the latter is
contained in sputum, it emits a most o
burning on an ignited coal. From the
appears that the judgement was regula
of the circumstances of the discharge o
as well as of its properties: that from s
was distinguished from the abscesses call
pye lammation, such a
and third source from t
asce secretion from the b
with n its continuity or l
was consequence of the
lishe me time by the le
Vien most ingenious cou
Hu wson. Previously to
Mr. *itical Essays*, p. 142
in h as well as, I believe,
asse *rm* matter is produ
or l or breach, by inflan
by i ned by extraneous bo
oph t was, however, gen
dou r not this matter w
of u es. This indecision
bably, on prejudice rather than on any
of differences of properties. The proof
membranous surfaces, in their entire sta
of the same sort as that of sores and absc
the most important pathological fact br
sequently to the Greek writers.

About 40 years ago, Simpson of St. A
Gaber, Pringle, Cullen, Fordyce, Hun
his pupil Hendy[*], were the prominent
cussion of the point—the matter from
mediately derived. The first person who c
the product of vessels becoming, or at le
office of, glands, as far as I know, was Sir
observations confirmed this opinion; bu
an opake white matter from serum of
induced Gaber, Pringle and Cullen to a

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[*] *Tentamen Inaugurale de Secretione glandulari.*

duction of pus on the supposition of a similar deposition from effused serum in abscesses and ulcers. Fordyce applied his chemical science, of which he was a master, on this occasion:—he interpreted all the phænomena of suppuration by means of the principle of purulent fermentation, which compounds pus from the supposed constituent or elementary ingredients of any kind of animal matter,—muscles, nerves, membranes, blood[*], &c. This composition, however, was effected solely by the agency of vital powers acting on such animal matters. Hewson supported by new arguments the doctrine of the production of pus by secretion[†]. Hunter seems to have convinced the medical public, by his ingenious observations and reasoning, that pus is a secreted fluid; and, with the exception that inflammation is essential to its production, his doctrine has been for the last 20 or 30 years generally admitted to be well established[‡]. But the minds of the thinking part of the medical faculty seem to have been still left in an unsatisfied state with regard to the notion they ought to entertain of the substance to be called pus. On account of the few properties of it known, they probably, and very reasonably, apprehended that different things might be denoted by this term; or that things in reality the same might be denoted by different terms, being supposed to be different from one another. Accordingly, in the course of the last 40 years, inquiries into the nature and properties of this fluid have been instituted, and been espe-

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[*] Van Swieten seems to have entertained a somewhat like opinion:—"Pus non fit in vasis sed extra vasa, in vulnere generatur ab effusis humoribus, calore corporis fotis et mutatis Si enim pus omne in vulnere hærens lintèis carptis mollissimis abstersum fuerit tenui liquido non pure post horam vulneris superficies undique madida apparebit; sed si per viginti quatuor horas emplastro tectum fuerit vulnus, illo ablato pus apparebit. Unde pus fit extra vasa; sed materies, unde fit, per vasa adfertur."—*Commentaria*, tom. i. p. 250.

[†] Pus is found in cavities sometimes without ulceration;—globules are perceived in it like those of milk—the quantity, the time of production, and properties of pus are varied by the state of the constitution, particularly by the passions—purulent matter is only the coagulable lymph altered, chiefly by inflammation, in flowing through secretory vessels. See *Experimental Essays*, by William Hewson, F.R.S. 1772.

[‡] I am unable to state precisely the date of Mr. Hunter's doctrine on this subject; but I learn from the Dissertation on Pus, by E. Home, esq. F.R.S. 1788, that he had delivered it, for many years preceding this publication, in his lectures. Mr. Home states Mr. Hunter's conclusions; "that the vessels of the part take on the nature of a gland, and secrete a fluid which becomes pus."—"Pus is a secreted fluid, at least it is formed from a similar structure of vessels as other secretory organs from the blood."—"Changes in the constitution affect the state of the pus, which could not be the case if it were made up of the solids and fluids of the part." For further proofs I refer to the ingenious Dissertation of the author.

cially promoted by the honorary reward
associated bodies. Hence some impro
made. But physicians were still conti
of the disadvantage in practice, from
properties of pus not being satisfactorily
all, for the purpose of judging in pulmo
ther or not the sputum was purulent.
the late Mr. Charles Darwin receive
from the Æsculapian Society at Edinb
posed discoveries of the criteria by wh
are distinguishable. It was asserted th
ded to a dissolution of pus in sulphuric a
takes place, but such a dissolution of m
addition of water merely suspended fl
diffusible through diluted sulphuric ac
not;—that these effects are also observe
water. In 1787, Dr. Brugman, in his
tion on Pus, among a number of other
I do not think necessary to be notice
volatile alkali (carbonate of ammonia)
tity of pus becomes viscid, semi-trans
that caustic ammonia partially dissolv
yields a very viscid fluid, but on adding
deposited in a viscid state;—that all
pus, and still more so the earthy salts,
metallic salts;—that alcohol condenses
aqueous parts, but neither coagulates n
previously to these experiments, Mr. H
that pus is "coagulated by sal ammo
ammonia), which he, and subsequently
upon, as affording a criterion between p
fluids. Grassmayer is quoted by seve
fact that pus is precipitable in a gelatin
alkaline lixivium, but if mucus be pres
The mistake in the fact that pus was
was perhaps first exposed by my fellow-
which was subsequently confirmed by
foreign authors, as Plenciz, Murray, S
Quesnay, either adopted subsequently,
opinion that pus is a secreted fluid.
nious Dissertation on Pus was publish
find no accession of facts from that dat
time. His work is valuable, not only
vations, but for a just exposition of th
It is here attempted to be shown that
of globules swimming in a transparent

that the globules are formed in the fluid after its secretion, while lying upon the sore or other inflamed surface, in different times, according to the state of the constitution and secreting part, the pus being secreted in a transparent condition *;—that inflammation is the absolute cause of the formation of this fluid; that the globules in pus are not soluble in cold water like those of blood, but are decomposed by boiling water, and the fluid in which they swim is not coagulable by heat, but is by sal ammoniac, which serum or the blood itself is not: the globules also are smaller than those of chyle, but much larger than those of pancreatic juice; and they are of the same size, but less numerous than those of milk;—that the distance from the heart influences much the condition of the pus;—that the depravation of this fluid is in proportion to the flaky or curdy particles seen floating in the fluid with the globules; and that the flaky parts are in the greatest proportion where the inflammation has been least, or the process on other accounts most defective.

In this historical account of the fluid under inquiry, if I have omitted to state the observations of any other authors, that must be imputed to my unacquaintance with them. I also purposely do not notice various pathological facts; such as the effects of pus of variolous eruptions, siphylitic, and other contagious diseases. As it appears from the confession of physicians that the nature and distinguishing properties of pus have not been satisfactorily ascertained, I engaged in an inquiry into the properties of this substance; some of the results of which I shall offer to the public.

XXX. *Remarks on the Rev.* C. J. SMYTH's *Letter on Systems of Tuning Musical Instruments.* Vol. xxxv. p. 448.

AFTER bestowing due praise on Mr. A. F. C. Kollmann's improvement of the theory of musical composition, Mr. S. makes objections to Kirnberger's temperament, which Mr. K. had recommended on page 9 of his "New Theory;" and concludes with several assertions of little

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* In a former paper on Expectorated Matter, (see Phil. Mag. vol. xxxv. p. 12—20,) I erroneously assigned the discovery of the globularity of pus to Mr. Home. It is but justice in me to declare that this Gentleman no where claims the discovery: on the contrary, I have since found that he refers it to Mr. Hunter. I take for granted Hewson's observation of this fact already quoted was subsequently to Mr. Hunter's, but I find no authority for the exact period of the discovery by either party.

weight, because (and I consider his "
they are mere matters of *taste*. And i
should presume "organ-tuners will conti
same way as their ancestors did *before th*
are produced to prove the superiority of
perament" to theirs; for a person but
with the subject might from this suppos
ter unequal temperaments. However,
which, for my own part, I do decide
doubtedly Kirnberger's system is one of
the ancient system (as M. de Bethizy ob
p. 1 sounds in some of
alter insupportable to a
equa as been preferred b
purg lo, professor Chladr
emin 's and musicians: i
best ; but for the orga
uneq t is better, on accou
of th

A isical readers, I am
for drudgery of calcul
cou ll further merit our
to y gazine tables of the
tion d-lengths, and the
con the other unequal
have and I think he oug
for t m, as the chief end
cation s to compare it with K
show its superiority *.

As to the generality of tuners (and
very conceited men), I believe they know
thing of harmonics. They learn one m
and remember it as they would a tune, v
rule on which either is founded. Tha
memory alone are sufficient, after prop
well convinced; for I can tune my harp
curacy diatonically and without soundi
time, as it can be tuned in the usual way
and I have a pupil, only twelve years o
remembrance of sounds are so accurate,
in a different room from the instrument

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* It would be an improvement of the first colum
low the German *tablature*, described in art. 34 of
Grammar, 2d edit. 1809. Mr. S. in some future
much oblige me by stating precisely what he mea
tuning.

ber of notes that can be struck with one hand in any part of the piano forte which she has been a little while accustomed to play on.

I know one tuner who, after studying ratios a little, thinks with Eximeno :—"Qual sciocchezza non è questa, supporre la musica fondata in certe ragioni, che bisogna guastare per ridurre la musica ad esecuzione? Almeno insegnasse la matematica a far questo guastamento; ma dopo un grand' apparato di ragioni matematiche, ciascun le guasta per la pratica a modo suo. I Francesi hanno fatto per il temperamento del cembalo difusissimi calcoli; ma tutti egualmente capricciosi che inutili, poichè in fine *l'istinto* senza riguardo a' numeri c' insegna ad accordar gli strumenti, come c' insegna a metter insieme le lettere per formar le parole, &c." p. 71.

August 11, 1810. M.*

XXXI. *An Examination of the Instructions given in an anonymous Pamphlet published in* 1809, *for Tuning an Equal Temperament of the Musical Scale. By a* CORRESPONDENT.

To Mr. Tilloch.

SIR, THE table by your correspondent the Rev. Mr. Smyth, at page 452 of your last volume, has enabled me to make a comparison of the method of tuning laid down in a recent pamphlet, sold by Becket and Co.; and perhaps you will oblige me in laying the same before your readers, with the necessary plate and extracts from the pamphlet alluded to; which, after some well directed sarcasms, explains the nature of the musical scale and the necessity of temperament, by a professed extract from Earl Stanhope's pamphlet, that was reprinted in your 25th volume. It then proceeds to extract Mr. Emerson's opinion, in favour of the Equal Temperament; founded solely, on there being no *fifth* in it, which is tempered more than $\frac{1}{100}$ part of a note; while the more important circumstances are overlooked, that there are in it 12 major thirds, each of which is more than $\frac{1}{15}$ part of a note too sharp, and 12 minor sixths as much too flat; also 12 major sixths almost $\frac{1}{13}$ part of a note too sharp, and 12 minor thirds as much too flat.

In page 21 of the pamphlet, I find an expression, di-

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* Can any of your readers favour me with any particulars respecting the manufacture of *tuning-forks*, and the standard by which they are tuned?

rectly contrary to what your correspo
says, at page 450, as to the temperamen
the organ, viz. Organs are "universally
the equal temperament:" and the auth
trials which I have myself made of the
temperaments, amongst the latter of
commended by Earl Stanhope, have in
the equal temperament as the best for p

"The method of tuning a piano-fort
the equal temperament, is explained b
structions, and further elucidated by t
table*, to which the reader is desired,
refer."

"Observe, that the capital letters in th
dicate the notes when tuned; the smal
to be tuned from them; and the crosses
tuned, with which the tuning notes
chords, as will be shown in the instructi
crosses stand for the natural notes, exce
marked."

1. "By means of a tuning fork, tun
the middle C, and from the former, tu
perfect octave.

2. "From middle C tune g, next abo
than a perfect fifth: and in order *to asc*
too flat or not flat enough, try it in a
C's already tuned. If it make a tolerab
the C below, and at the same time a tol
with the C above, *it is well tuned*."

Here, sir, I have to remark, that
fifth Cg is to be flattened, supposi
of the usual concert pitch, or to mak
brations in one second of time, is j
to Mr. Smyth's table, that it ma
one quarter of a minute, or 15 s
time always to be understood when
are mentioned, in what follows:
tended *trial* in the latter part of thi
plicable and ridiculous, since ever
tempered concord, to the octave,
with itself, *whatever that degree of*
be, and consequently whether well c
purpose of an equal temperament, i
from this test, as would be the hou
day, on which such fifth was tuned.

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* See Plate V.

3. "From G tune g below it, a perfect octave, and *try* it in a chord with the C between them."

4. "From the last tuned G tune d *a little* flatter than a perfect fifth, and try it in a chord, with the G above it already tuned, *until* you have G d a good fifth and d G a good fourth."

This fifth G d must beat 9 times, and the fourth d G 18 times*, and the pretended *trial* is nugatory and absurd, for its professed purpose.

5. "From D, tune a, *a little* flatter than a perfect fifth, and try it in a chord, with C above it, *until* you have D a a good fifth, and a C a good third."

The fifth D a must beat 13 times: and the first trial or check of any use which occurs, is the *minor* third a C, beating 326 times, but which no organist in England would, I think, call "*a good third.*"

6. "From A, tune a below it, a perfect octave, and *try* it in a chord with D between them."

7. "From the last tuned A, tune e a flat fifth, and *try* it with C and G, *until* you have three good thirds, A C, C e, e G.

Now since the *minor* third A C beats 163, and the thirds C e and e G, 142 and 244 respectively, such must be *tried a long time,* so that all idea of perfect chords is forgot, before any musician would pronounce them "*good thirds.*"

8. "From E, tune b a flat fifth, and try it with G, *until* you have two good thirds E G and G b."

This fifth E b must beat 15 times, and the thirds above mentioned will beat 244, and 214, and consequently are not "good thirds."

9. "From B, tune b below it a perfect octave, and try it with D and G."

Here b D beats 183, and GB beats 214; on which I forbear to comment.

10. "From the last tuned B tune f♯ a flat fifth, and try it with D and b *until* you have D f♯ a good third, and f♯B a good fourth."

Now B f♯ beats 11 times, and 160 and 22 are the other beatings.

11. "From F♯, tune f♯ below it a perfect octave, and try it with A and D."

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* Because the *minor* consonance (the 4th) is the uppermost: see Dr. Smith's Harmonics, 2d edit. p. 93,

Here f*A beats 137, and DF* b

12. "From the last F* tune c* a fl
with A and F* *until* you have Ac* a go
a good fourth."

The fifth F*c* beats 8½, and th
120; of course the fourth C*F*
with its complemental fifth above i

13. "From C* tune g* a flat fifth,
and B *until* you have Eg* a good third
good third."

The fifth C*g* beats 12 times; a
08.

14 ıne g* below it, a p
try it ,

4, the half of the ab
a the repetition, and t
ary.

" oceed by tuning the
the e sharpened, which
in c g the upper note,
ward

15 ove middle C, tune
than and *try* it with A
have d, and Cf a good fo

fC beats 16 times
fo the trial of it is usel
for proving whether fC is sharpene
tity for an equal temperament of th
beats 190, and this furnishes a chec

16. "From F tune a* a sharp fifth, a
until you have a*D a good third, without
the fifth."

The fifth a*F beats 10½ times, ar
beats 127 times.

17. "From A* tune a* above it, a pe
try it with D and F."

Here A*D beats 127 times as abo
times, the same as the last, which sl
useless step.

18. From the last tuned A* tune d*
try it with G and A* (below) until you l
fourth, and d*G a good third."

Here the fifth d*A* beats 14 times, and the fourth A*d* the same, and the third d*G beats 169 times.

From the above comparisons of Mr. Smyth's table, with the rules in this pamphlet, it will I think appear plain, that the writer of them had no proper conception of the nature of an equal temperament, and that it is extremely *unlikely that he had ever heard or calculated such a temperament*, decidedly as he speaks of *his trials of it:* and I fear, sir, that this is no uncommon case, in the present rage for writing principles of music, principles of tuning, theories of harmonics, instructions for tuning, &c. &c. by persons who ought first to employ themselves, in studying the very elements of the science of harmonics.

Among the recommendatory criticisms for different Reviews, of the pamphlet before me, one for the *Phil. Mag.* has been forgotten, which I suggest should have run thus:

> A careful examination of the instructions contained in this pamphlet, for tuning an equal temperament, convinces us, that the assumed name *Musicus Ignoramus* of its author, is no *misnomer;* and that the *wit* displayed therein, vastly exceeds its science, or its usefulness.

I beg pardon for obtruding so long a letter on your attention, and am, sir,

August 6, 1810. Yours, &c. &c. &c.

XXXII. *An Analysis of several Varieties of British and Foreign Salt, (Muriate of Soda,) with a view to explain their Fitness for different œconomical Purposes. By* William Henry, *M.D. F.R.S. Vice-Pres. of the Literary and Philosophical Society, and Physician to the Infirmary, at Manchester.*

[Concluded from p. 119.]

Sect. III. *Account of the Methods of analysing the several Varieties of Muriate of Soda.*

The method of analysis which I adopted, in examining the several varieties of muriate of soda, was as follows.

When the salt was in a state of solution, a measured quantity was evaporated to dryness in a sand heat, which was carefully regulated, to avoid the decomposition of the muriate of magnesia, if any of that salt were present in the solution *,

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* Muriate of magnesia, according to Dr. Marcet, begins to part with its acid at a temperature a few degrees above that of boiling water. This fact explains the observation of Mr. Kirwan, that too great a heat, employed in the desiccation of muriate of magnesia, decreases considerably its solubility in alcohol. (Kirwan on Mineral Waters, p. 275.)

Each specimen of salt was reduced to
was dried, in the temperature of 180° of
the space of two hours. This was dor
different experiments might be made
quantities of salt.

I. *To separate the earthy M*

(A.) On 1000 grains of the dried a
(except in the case of the foreign sal
grains were used,) four ounce measur
poured, of a specific gravity, varying fr
at nearly a boiling temperature. To in
the irt of the salt, they
ther n a mortar, and the
glas. e they were digeste
and ted. The alcohol
by he undissolved part
lay four ounce measur
(vashings were evap
and a small portion of
adde e earthy muriates fro
salt, dissolved along wit
lutio ver, still contain a
mur was therefore again
solv and mixed with a so
of s for some minutes,
earth ted, and, after being
re-dis atic acid. This sol
porated to dryness, gave the weight of th
which had been extracted by alcohol †.

(B. a.) The dry mass thus obtained m
of muriate of magnesia, of muriate of
An aliquot part, therefore, was dissolve
the purpose of assaying it by the usual t
as in the case of the earthy muriates proc
muriate of magnesia alone was indicate
process was rendered unnecessary. Mu

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* In this and all similar cases, the heat was very c
wards the close of the process.

† By the analysis of artificial mixtures of pure m
earthy muriates in known quantities, I afterward
amount of the earthy muriates was not ascertained in
The deficiency of the latter salts was about one sixth
necessarily have been the same in all, it does not af
different varieties of salt, as to their proportion of t
numbers in the 5th column of the table (indicatin
riates) be increased in the proportion of six to five, w
the quantities in each variety of salt.

in no instance found uncombined; but in the majority of cases (as in the earthy muriates obtained from Cheshire) salt was mixed with muriate of magnesia.

(B. b.) To the solution of two earthy muriates was added fully saturated carbonate of ammonia, which has the property of throwing down lime in combination with carbonic acid, but has no effect on the muriate of magnesia at ordinary temperatures. The solution of the latter salt, along with that of the excess of carbonate of ammonia, was therefore separated by filtration; and to the filtered liquor a solution of phosphate of soda was added, according to the formula of Dr. Wollaston *.

(B. c.) By direct experiments I had learned that 100 grains of muriate of magnesia, when thus decomposed by carbonate of ammonia, conjoined with phosphate of soda, give 151 grains of an insoluble ammoniaco-magnesian phosphate dried at about 90° of Fahrenheit. Hence it was easy, from the weight of the precipitate, to calculate how much of the former salt was contained in the mixture of muriate of lime and muriate of magnesia. Thus, if 20 grains of a mixture of the two muriates yielded 15·1 of ammoniaco-magnesian phosphate, it is obvious that the mixture must have consisted of equal weights of muriate of lime and muriate of magnesia.

(B. d.) The estimation of the proportion of muriate of lime, in a mixture of this salt with muriate of magnesia, was sometimes performed in a different way. To a cold solution of a known weight of the two salts, super-oxalate of potash was added; and the precipitate was collected, washed, and dried at about 160° Fahrenheit. Of this precipitate I had previously found that 116 grains are formed by the decomposition of 100 grains of dry muriate of lime. From the quantity of oxalate of lime it was easy, therefore, to infer that of the muriate, from whose decomposition it resulted; and this subtracted from the weight of the two salts, gave the weight of the muriate of magnesia.

II. *To separate and estimate the earthy Sulphates.*

(C.) The portion of salt which had resisted the action of alcohol, was dissolved by long boiling in sixteen ounce measures of distilled water, and the solution was filtered. On the filter a small quantity of undissolved matter generally remained, which was washed with hot water, till it

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* See Dr. Marcet's analysis of the Brighton Chalybeate, published in the last edition of Saunders on Mineral Waters.

ceased to have any action. The weigh
portion was then ascertained.

(C. a.) By this operation were dissol
muriate of soda, but all the other salts,
hol, which might be mingled with it.
carbonate of soda was added; and the
most cases gave, on this addition, an ab
was boiled briskly for several minutes, i
of the earthy carbonates, which were se
main dissolved by an excess of carbonic

(C. b.) The precipitated earths were a
and were well edulcorated with boiling v
bei uor first decanted fr
To ids (after the addit
riati required for satur
baryt till it ceased to occ
preci phate of barytes was
ficie ted; and its amount

T onates, an excess of
add h, and the mixture
all e d. It was then eva
calc d heat, and the wei
sulp ined.

(D ulphates were was
quan water. In several i
of w ined, was extremely
bein a very minute portio
lime, salt, solely, the resi
to be composed.

(D. b.) But in other cases, a considera
ensued; and in these, to the watery solu
mixture of equal parts of saturated soluti
of ammonia, and phosphate of soda. A
or less copious was produced, which was
90° Fahrenheit, and weighed.

(D. c.) By direct experiments I had de
grains of this precipitate result from the
100 grains of sulphate of magnesia, of
dryness, as to lose 44 grains out of 100,
low red heat. Hence 100 grains of ammo
phosphate indicate 111 grains of crystall
desiccated, sulphate of magnesia*. Fro

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* The assumption that crystallized sulphate of mag
per cent. of water, though it was correctly true with t
I operated, is below the average, which, I find from

the ammoniaco-magnesian phosphate, it is easy, therefore, to infer the proportion of sulphate of magnesia in any mixture of the two earthy sulphates.

(D. d.) It was possible, however, that in addition to the sulphates of lime and of magnesia, the quantity of which had been determined by the foregoing process, the specimen of salt under examination might contain also an alkaline sulphate. To decide this point, it was necessary to compare the amount of the acid, deducible from the weight of the sulphate of barytes (C. b.), with that which ought to exist in the sulphate of lime and sulphate of magnesia actually found by experiment. But to make this comparison, some collateral experiments were previously necessary.

(D. e.) By these experiments, I found that sulphate of lime prepared by double decomposition, then calcined in a low red heat, and afterwards dissolved in a large quantity of boiling distilled water, yields, when precipitated by a barytic salt, in the proportion of 175·9 grains of sulphate of barytes from 100 of the calcareous sulphate*. The same quantity of ignited sulphate of lime (=128 grains dried at 160° Fahrenheit), precipitated by super-oxalate of potash, gives 102·5 of oxalate of lime; or, precipitated by sub-carbonate of potash at a boiling heat, 74·3 grains of carbonate of lime†. One hundred grains of crystallized sulphate of magnesia (=56 desiccated) afford, when precipitated by muriate of barytes, 111 or 112 of the barytic sulphate.

(E.) By a comparison of the above proportions with those obtained in the analysis of any specimen of common salt, we may learn whether it contain other sulphates beside those with earthy bases. For example, if the precipitate (D.) consist of carbonate of lime only, and bear to

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about one half the weight of the salt. Mr. Kirwan states the water of crystallization to be 53·6 in 100 grains; but this, I believe, a little exceeds the truth.

* This result corresponds, within a fraction of a grain, with one obtained in a somewhat different way by Dr. Marcet, and very nearly with an experiment of my friend Mr. James Thomson, who found the barytic sulphate, precipitated from 100 grains of sulphate of lime by nitrate of barytes, to weigh 173 grains.

† On reversing this experiment, I found that 100 grains of carbonate of lime, saturated with sulphuric acid, and calcined in a low red reat, afford 135 of sulphate of lime. A similar experiment of Mr. Thomson gave 134·6 grains. Dr. Marcet also informs me, that from 93·55 grains of pure marble he obtained 125·95 grains of sulphate of lime, proportions which exactly coincide with those of Mr. Thomson.

the sulphate of barytes (C. b.) the propo
or very nearly so, we may infer, that
present but that of lime. The same c
low, if, after having decomposed one
solution (C.) by muriate of barytes, an
oxalate of potash, we find that the sulph
to the oxalate of lime, the proportion c
Now these proportions were, as nearl
pected, obtained in the analysis of Nor
whence we may conclude, that the onl
contains is gypsum, or the sulphate of li

It must be remembered, however, t
sulph in any variety of co
be in mplete desiccation,
parts exposure to a red h
nece either to increase,
of 5 ate of the sulphate
by th , or, more simply,
grai barytes indicate 73
of li Fahrenheit, =57 i

(l ate of lime and sul
wer d, and other sulphat
sibl in the varieties of s
the me a little more con
case, ng the quantity of b
the .) I estimated how
bary respectively to afford
pare quantity, with that
obtained. The earthy carbonates, for exa
from 1000 grains of Lymington salt, wh
been digested with alcohol, were conver
of calcined sulphates, consisting of 19 g
phate of magnesia, and 12 grains of dry
Now from the magnesian sulphate 38 gra
barytes should result, and from the sulphat
the sum of which is 59. But the quantit
was 59·8. There is only, therefore, an exc
the actual above the estimated quantity,
too trivial to be admitted as an indicatio
with an alkaline base; and arising, pro
avoidable errors in the experiment.

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* This I find to be the loss sustained by 100 grai
dried at 160°, and then ignited. The same quantity
selenite, I learn from Dr. Marcet, loses 20·7 grains,
[illegible]ng red heat.

(F. a.) If in any mixture of salts, free from the earthy muriates, we are certain that no other sulphates exist beside those of lime and magnesia, their estimation becomes extremely simple. Decompose two equal quantities of the salt in question, the one by muriate of harytes, the other by oxalate of potash. From the weight of the latter precipitate, we may calculate the quantity of sulphate of lime. Suppose, for example, the oxalate of lime (as was actually the case with the precipitate from 1000 grains of Lymington salt) to weigh twelve grains; these denote 15 of sulphate of lime, dried at 160° Fahrenheit, which quantity, if decomposed, would give 20½ of sulphate of barytes. The latter number (20⅛), subtracted from the weight of sulphate of barytes actually obtained (say 60), gives 39⅞ grains for the sulphate of barytes resulting from the decomposition of sulphate of magnesia. The quantity of the latter salt, it will be found therefore by applying the rule already given (D. e.), must be 35 grains.

(F. b.) The same object may be accomplished by decomposing two equal quantities, the one by oxalate of potash, the other by the compound solution (D. c.). From the weights of the precipitates, it is easy to calculate from how much of the calcareous and magnesian sulphates they have resulted.

(G.) When the salt left by alcohol was known to contain muriate of soda and sulphate of magnesia, but no sulphate of lime, the presence of alkaline sulphates was investigated in the following manner. The salt was dissolved in water, and the solution was divided into two equal portions. To the one muriate of barytes was added, and to the other, the compound precipitant of carbonate of ammonia, and phosphate of soda. If the sulphate of barytes, thus produced, bore to the ammoniaco-magnesian phosphate the proportion of 112 to 90, it was concluded that no other sulphate had been decomposed, but that with base of magnesia.

(H.) At one time I expected to have ascertained the quantity of sulphate of soda, in an artificial mixture of that salt with sulphate of magnesia and muriate of soda, by the following formula. To a solution of the three salts, heated to a boiling temperature, I added sub-carbonate of ammonia, which decomposes the sulphate of magnesia only. I had then a solution containing muriate and sulphate of soda, with sulphate of ammonia, and some carbonate of ammonia. This solution was evaporated to dryness, and

the mass was sufficiently heated to exp
salts. I found, however, that at this te
phate of ammonia acted upon the m
produced an additional and not incons
sulphate of soda.

Having determined, by the foregoi
quantity and kind of the earthy muria
the insoluble matter, and the proportio
weights of all these different impurities w
and the sum being deducted from the
submitted to experiment, the remaind
the amount of the pure muriate of s
under examination *.

Though I purposely refrain from givi
several analyses, which were made acc
going plan, from the conviction that
tedious and unnecessary, yet there are
which it may be proper to mention n
be done in the form of a table.

1. The *brine* which I examined wa
and was sent to me in the state in whic
the spring †. At the temperature of 56
the specific gravity of 1205. It was
lost a little of its transparency when
heat, in consequence of the depositi
quantity of carbonate of lime and oxi
immediately precipitated by muriate o
ammonia, and alkaline solutions, bot
Eight ounce measures, evaporated to dr
gave 1230 grains of salt, which, for the
I term *entire* salt. It proved, on ana
one thousand parts ‡;

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* I have deemed it unnecessary to state, in t
acid and base in the several varieties of muriate o
be estimated from the proportion, deduced by D
of soda, in 100 of the pure muriate. In this dete
100 parts of luna cornea, after being melted and
of 19·05 parts of acid, to 80·95 oxide of silver.
nearly with the recent one of Gay Lussac, wh
to combine with 7·60 oxygen, and this oxide t
real muriatic acid.

† I have lately been informed that this brine
rock-salt mine, into which, from the impossibili
solid form, it was allowed to flow. Hence it was
of soda.

‡ The specific gravity and proportion of earth
appears to differ considerably in the brine of
land's Cheshire Report, p. 45, &c.

Carbonate of lime and oxide of iron	2
Muriate of lime, and muriate of magnesia, in nearly equal proportions	5
Sulphate of lime	19
Muriate of soda	974
	1000.

2. The *mother liquor*, or brine that remains after separating all the common salt, which it is thought worth while to extract, had the specific gravity of 1208. The dry salt contained,

Muriate of magnesia	35
lime	32
Sulphate of lime	6
Muriate of soda	927
	1000.

3. The *clearings* of the brine, which are raked out of the pan when the salt first begins to granulate, contained in 1000 parts,

Muriate of soda	800
Carbonate of lime	41
Sulphate of lime	159
	1000.

4. Of the substance called by the workmen *pan-scale*, two specimens were analysed, the one containing a large proportion of muriate of soda, the other very little. The first variety consisted of

Muriate of soda	950
Carbonate of lime	10
Sulphate of lime	40
	1000.

The second variety was composed of

Muriate of soda	100
Carbonate of lime	110
Sulphate of lime	790
	1000.

Circumstances, however, are constantly occurring to vary the proportion of ingredients, both in the clearings and in the pan-scale. If, for example, the brine be short of the point of saturation with common salt, it acts, when admitted into the pan, upon the muriate of soda which the pan-scale contains, and we obtain the second variety. But

if the brine be fully charged with salt, it
of the muriate of soda, carried down al
sum; and then the first species of pan-sc

5. The *salt oil*, or mother liquor from
cimen of which I received from Dr. T
specific gravity of 1277. It was abun
by muriate of barytes; by pure ammon
carbonate; and was not changed by
either immediately or after an interval of
thousand parts of the dry salt consisted

Muriate of magnesia ....
Sulphate of magnesia ....
Muriate of soda ....

6. The *salt brine*, or liquor which drai
salt, had the specific gravity of only 118
by the same tests as the salt oil, but less
dry residue contained

Muriate of magnesia ....
Sulphate of magnesia ....
Muriate of soda ....

7. The *mother liquor*, or *bittern pa*
sented, on analysis, an unaccountable
similar fluid sent from Scotland, and g
proportion of sulphate of magnesia. A
tity of this salt had, moreover, crystall
which contained the liquid. Its specifi
One thousand parts of the dry salt conta

Muriate of magnesia ....
Sulphate of magnesia ....
Muriate of soda ....

8. The pan scale from Lymington c

Muriate of magnesia ....
Desiccated sulphate of magnesia
Carbonates of lime and magnesi
Sulphate of lime ....
Muriate of soda ....

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* The proportion of these carbonates I was by
m determining.

From the very near approximation of the proportions between the sulphate of barytes and ammoniaco-magnesian phosphate, obtained in the analysis of all these products of sea water, to those which result from the decomposition of two equal quantities of sulphate of magnesia, it may be inferred that they contain no sulphate of soda *. For example, to decide whether the Scotch salt contains an alkaline sulphate, or not, I dissolved 1500 grains in a pint of boiling water, and evaporated till fourteen drachm measures only remained, the common salt being removed as soon as it was formed. The residuary liquid was divided into two equal portions, one of which gave 18½ grains of sulphate of barytes, and the other, 14 grains of ammoniaco-magnesian phosphate. The proportion between these numbers is so nearly that which has been already assigned, (viz. 112 to 90,) that we may safely infer the total absence of sulphate of soda. This salt, indeed, is considered as incompatible with muriate of magnesia; but after digesting, for two or three days, 100 grains of the former, with 20 of the latter, evaporating to dryness, and washing the residuum with repeated affusions of alcohol, I found that two grains of the muriate of magnesia had escaped decomposition.

Manchester, June 19, 1809.

XXXIII. *Analysis of the Scammonies from Aleppo and Smyrna; to which are subjoined some Observations on the red Colour given to Turnsole by the Resins.* By *Messrs.* Bouillon Lagrange and Vogel †.

The two species of scammony in question are procured from the root of a plant which grows in Syria. It seems that it is by an incision made in the root that the juice is extracted; each root yields about two drachms only. The juice thus extracted is dried in the sun, and then exposed for sale: at least it is in this way that the finest and purest scammony is obtained. Frequently, however, the inhabitants of Syria and Natolia, in order to procure a greater quantity of the sap, extract it by expression, not only from the root, but from the stalks and leaves: occasionally also they adulterate the scammony by mixing the juice procured from

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* I employed more attention in investigating the presence of sulphate of soda in the products of sea water; because this salt is stated to be one of its ingredients by the Bishop of Llandaff, (Chemistry, vol. ii. p. 62,) and by other chemical writers.

† *Annales de Chimie*, tome lxxii. p. 69.

it with that of some other milky an
sometimes they increase its weight by t
coal or other foreign substances. In
that scammony does not contain any of
matters, we ought to break the pieces o
such as are brilliant within, rejecting
too black, burnt, or sandy.

The scammony of Aleppo is light, o
liant and transparent in its fracture.
very compact, heavy, and of a deeper co
difficult to reduce into powder than tha

Examination of the Scammony of A
scammony is pure, it melts entirely or
iron, and gives out nauseous vapours:
water, the liquor is of a milky whitenes

Boiling water makes it run into a ma
comes yellow, has a bitter taste, and is
acid, which proves that this substanc
with ashes, as some authors assert.

Alcohol at 40 degrees forms a sligh
aqueous liquor, and with the acetate of
lowish flakes soluble in the nitric acid.

The alcoholic tincture of scammony i
low colour. This liquor reddens turn
remains, after the evaporation, a resin
and transparent.

This resin is entirely dissolved in th
is coloured yellow. The addition of
turbs the liquor.

This substance is equally soluble in
potash, even cold, and the liquor acqui
if this solution be made with the help
is brown. Water even in great quanti
tate resin. Even when saturated by t
does not separate the resin. This t
resin, acid, and potash, ought to excite t
titioners: it would perhaps be possible
a solvent for resins which water does n

That part of scammony which is i
when dried, acquired a gray colour.

When treated with boiling water, it
and alcohol precipitated it in white fla

In order to determine the proportion
principles of the scammony of Aleppo,
of this substance, which we dissolved in
was coloured yellow. There remained,

by alcohol, a matter of a gray colour, which, when dried, weighed 0·26.

The alcoholic solution was evaporated to a syrupy consistence. Cold water precipitated from it a resin forming a homogeneous mass: the supernatant liquor was transparent and colourless. Evaporated to dryness, we obtained a brown matter soluble in water and in alcohol, forming a precipitate by the acetate of lead. This substance seems to be what is called the extractive matter: its weight was found to be 0·2 after having been dried.

The resinous mass separated and dried had a yellow colour, and weighed 0·60.

We afterwards treated the 0·26 of matter which was insoluble in alcohol, with boiling water. There remained after the evaporation a gluey matter, weighing 0·3, having all the characters of gum. The rest was merely the refuse of vegetable matter and a little silex.

The distillation of the scammony of Aleppo presented nothing remarkable. It gave as products, a very acid brown liquor and a light blackish oil. The charcoal resulting from the operation was black, brilliant, and compact; it contained carbonate of potash, carbonate of lime, alumine, silex, and a little iron.

Examination of the Scammony of Smyrna.—The fusion of the Smyrna scammony is less complete than that of Aleppo: instead of going into a mass with boiling water, it became knotty, and the water was dyed yellow. It is neither acid nor alkaline: the acetate of lead precipitates yellowish flakes from it.

100 parts of this scammony taken up by boiling alcohol, although less charged with resin, gave a deeper-coloured tincture than that which was made with Aleppo scammony. We obtained from the evaporation of the alcohol a brownish transparent resin, the weight of which was 0·28. We found 0·66 of insoluble matter in the alcohol. This residue treated with boiling water coloured it yellow: it had a putrid sweetish taste, and alcohol precipitated from it flakes soluble in water. The liquor evaporated left a thick gluey matter like mucilage, soluble in weak nitric acid when warm; precipitating, on cooling, a white pulverulent matter which presented all the characters of mucous acid.

In this experiment, the water had only taken up 0·8 of the matter which was insoluble in the alcohol. The rest was submitted, with the help of heat, to the action of the nitric acid, which dissolves it with effervescence. Am-

monia

monia added to this nitric solution forme
luble in potash. The potash and the ox
also occasioned a precipitation. This re
therefore of alumine and carbonate of lim
fuse of vegetable matter, and that substan
luble in water and in alcohol, a substanc
be oxygenated extract.

This substance incinerated left a whit
ble in a great measure, and with efferves
riatic acid. This solution contains alun
little iron. The portion not soluble in
when treated by potash, gave a siliceous
additi

Th
ad served to precipit
after
, a brown substance
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ting humidity fron
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and abundantly pre
aqueous
the acetate of lead
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perties of the extract

It
re from this analysi
parts
mmony are thus cons

....
....
inciple ....
d earthy matter, &c.

2.
yrna scammony contains:

Resin ....
Gum ....
Extractive principle ....
Vegetable and earthy matter ....

As the resin obtained from both kin
much the same, excepting that the Alep
transparent and friable, whereas that c
highly coloured and more difficult to pulv
would be useful to ascertain if there wa
their medicinal properties. Several phy
made experiments with both kinds on in
habits, and have observed no difference
tive properties.

We may conclude therefore, from w
scammony is a true gum resin mixed wi

matter. It contains indeed much less gum than the other gum resins, but enough, however, to form a milky liquid with water.

The action of the alcoholic tincture of scammony on turnsole, naturally led us to ascertain whether the property of reddening this blue colour was owing to an acid. Our experiments not having enabled us to acquire a direct proof, we tried some resins in a comparative manner, which we submitted to the following experiments.

1. Sandarach. This resin is converted into a knotty or grumous mass on being boiled with water. The filtered liquor remains clear: when properly evaporated, it slightly reddens turnsole tincture: the taste is bitter: it does not change the infusion of violets, is not precipitated by alcohol or by the acetate of lead; which proves that it contains neither gum nor extractive principle. It is therefore a pure resin.

The resin which had been treated with boiling water, was dissolved in alcohol. This liquor reddens turnsole tincture strongly, and has no action upon syrup of violets.

We also digested sandarach reduced into powder in alcohol, adding to the liquor, when warmed and filtered, boiling water, which precipitated the resin from it. The filtered liquor was turbid upon cooling. It had the strong smell of sandarach resin: its taste was bitter; and its action on turnsole tincture was so weak, that we could not presume the existence of a free acid.

2. Mastich. This substance presents nearly the same phænomena with the above: the resin however runs into a mass in boiling water like turpentine. The water has a bitter taste, and has no action either upon turnsole or upon violet syrup. The resin, on the contrary, reddens turnsole tincture strongly.

3. Olibanum forms in hot water a thick magma, which is separated with difficulty from the liquor, even by filtration. This water has a blackish brown colour, is not precipitated by the acetate of lead, and does not change the colour of turnsole, but alcohol precipitates it in abundance; which proves that this substance is composed of gum and resin.

The alcoholic tincture reddens turnsole tincture strongly.

If we carefully heat in a sand bath the resins which have most action on the colour of turnsole, no acid is sublimed.

When treated with lime according to Scheele's process, no calcareous benzoates are formed.

4. Ammoniacal gum resin, myrrh, gum elemi, galbanum,

banum, tacamahaca, resin of common
pentine, oil of turpentine, and several
gummo-resinous substances, gave the
those obtained from the scammonies, sa
banum. From these facts we may infer
ficult to resolve this question: Is it to t
acid in the resins, that we ought to ascrib
turnsole?

If the acids alone had the property of
vegetable colours, we should not hesitate
existence of this property in the resins, alt
have not yet proved it. As to the infus
which the resins have no action, this pr
the sublimated benzoic acid, which stro
sole tincture, and which does not change
lets. Has this acid, notwithstanding its
any analogy to the resins? We shall abs
on this subject, although we are induced
substance is a compound of a *vegetable*
quantity of resin, which perhaps gives it
lastly, as all the vegetable acids are sol
still difficult to ascribe to the presence of
perty which resins have of reddening t
probable therefore, until some new exp
contrary, that we may regard it as bein
racters of the resins to redden the blue c

XXXIV. *On prime and ultimate Ratios; cation to the first Principles of the fl*
By Mr. MARRAT.

RATIO denotes the relation which two
each other.

The two quantities must be of the sam
no comparison can be made between the

The measure of a ratio is obtained by
part, or parts, one term of the ratio is of
let a and b denote the terms of a ratio,
any ratio; then, its measure is had by
part, or parts, a is of b.

Let us denote a by 6, and b by 2, then
measure of the ratio $\frac{6}{2}$.

If $a=2$ and $b=6$, then, $\frac{2}{6}=\frac{1}{3}$; or $\frac{1}{3}$
the ratio of $\frac{2}{6}$; and so on for other quan

The part b of the ratio is called the ***antecedent***, and a is its ***consequent***.

The antecedent may be equal to the consequent, and then the ratio is called a ratio of equality; though it would be more proper to say, the *terms* of the ratio are equal:—when the terms of a ratio are equal, its measure is always equal to unity.

If the terms of a ratio vary, the measure of the ratio may have any magnitude whatever; and if one term remain constant while the other varies, the measure of the ratio will vary with the varying term.

Let $\frac{a}{b}$ represent any ratio, and let a remain constant, while b is variable; it is obvious that if b decrease, the measure of the ratio will increase; and, when b is become indefinitely small, the measure of the ratio is then indefinitely near to a; and when b entirely vanishes, the measure of the ratio is exactly equal to a.

On the contrary, when b increases, the measure of the ratio decreases. Again, let b remain constant while a is variable: then, as a increases the measure of the ratio increases, but it decreases as a decreases, and when a entirely vanishes the measure of the ratio is equal to $\frac{1}{b}$.

As another example, suppose we have the ratio $\frac{x+a}{x}$, where x is variable and a constant; the measure of this ratio may vary through all possible degrees of magnitude, as in the preceding example.

1. Let x continually increase; then, the measure of the ratio $\frac{x+a}{x}$ will decrease; and when x is indefinitely great, it will become nearly a constant ratio, or a ratio of equality; that is, the *terms* of the ratio will be nearly equal: because the addition of a to a quantity x which is indefinitely great, will alter the measure of the ratio only in an indefinitely small degree: hence it continually verges to a ratio of equality as a limit.

2. Let x decrease; then, the measure of the ratio $\frac{x+a}{x}$ will increase; and when x is indefinitely small, the measure of the ratio is indefinitely near to a: when x vanishes, the ratio is equal to a, exactly.

From the above illustrations it is exceedingly obvious, that a ratio in which the terms continually vary, or where one is variable and the other constant, or where part of one term is constant, as in this latter example,— it is obvious, I say,

I say, that the measures of such ratios ne
limits which we have assigned to them:
ever, continually approximate towards the
measures of the ratios differ from those li
any assignable difference, they may be *sai*

This being allowed, it is evident that
those ratios, after having supposed that th
to such ultimate states, or limits, we con
mate towards true results; and when the
tained, differ from the true results by a qua
small, they may be said to be indefinitel
and in practice the indefinitely small err
lected, sensible magnitude.

This vton meant in his fi
Princi says that "quantitie
of qua n any finite time con
to equ e the end of that tim
the one than by any given d
ultima

It w those results *true* wh
proxim tely near the truth,
thor of so much advantage
errors sics of the fluxional
was ve te in *Philalethes* (su
Jurin) the error occasione
certain antity did not affect
operati not in the least tend
argume y the *author* of the
was the error in *principle* that he struck
quantity of error that the making use of
might produce.

The conclusions obtained by the metho
not *absolutely true*, nor did Newton ever
such; they are approximations, which pr
errors; and had his host of defenders pro
than this, all the arguments that could ha
forward against this method must have va

But instead of giving up what was evi
all the varied arguments which imagination
could suggest, were brought forward in or
the difficulties which Berkley had pointed
effect; for truth is at all times consisten
what is once wrong can never be proved t

Newton was desirous of determining th
linear figures; this was at all times a gr
and had exercised the talents of philosop

The first method which the ancients made use of for this purpose was the method of *exhaustions:* an example of the use of this method is given by Euclid in the second proposition of his twelfth book, where he compares the circle and square, and proves that any two circles are to each other as the squares on their diameters: the same method was also made use of by Archimedes in determining the quadrature of the parabola. The argument here made use of is called *reductio ad absurdum,* which, though strictly logical, is often tedious, because every proposition must be divided into two cases, in one of which it must be shown that the former of the two quantities to be compared is not greater than the latter; in the other, that it is not less. It was with a view of shortening this mode of reasoning, that Cavalerius invented the method of *indivisibles:* in this method, every line is supposed to be made up of a number of other lines whose lengths are indefinitely short, and every curvilinear figure is considered as a polygon of an indefinite number of sides:—these principles were in many cases extremely easy and convenient, and produced true results; they, however, often led their followers into perplexities, and sometimes into error.

It was to avoid the tediousness of the method of *exhaustions,* and the errors in the method of *indivisibles,* that Newton invented his method of *prime and ultimate ratios;* the principles of which he laid down in the first lemma of the *Principia,* as observed above. Several eminent mathematicians have endeavoured to demonstrate Newton's lemma: it however certainly admits of no direct proof; it is itself a definition, and requires only to be illustrated, or explained.

By introducing the doctrine of motion into geometry, much has been effected. Newton employed his method of prime and ultimate ratios to the quadrature of all kinds of spaces, by supposing one or more of the sides of the figure to be in motion, and to generate those figures by the motion of their extreme points. The application to right-lined figures was natural and easy; and to apply it to a square, we will suppose that the square is generated by the motion of two right lines perpendicular to each other, and that move parallel to two other right lines placed at right angles.

Let x denote the length of each side at any given position of those lines, and let $\dot{x}$ be the increase, in the length of each side, caused by the motion of the two moveable sides; then, $x+\dot{x}$ will be the length of each side so increased,

creased, and the fluxion of the area is
is $= 2x\dot{x} + \dot{x}^2$. Now in order that we
without affecting the result of the operati
pose that $\dot{x}$ is a quantity less than any a
it is only in a nascent state: according t
the error, by neglecting $\dot{x}^2$, will be ext
will no way affect the fluxionary increa
but, except $\dot{x}$ vanishes, and that woul
fluxional increase altogether, we are oblige
that the result is not strictly and logically

Again, let x and y denote the sides of
by the motion of those sides, let them
$y + \dot{y}$; th[illegible]on of the area of t
be $(x+$[illegible]y, or $= x\dot{y} + \dot{x}y + \dot{x}\dot{y}$
the rect [illegible] be neglected, $\dot{x}$ and
finitely [illegible] nascent state; but
is comm [illegible] however trifling it m
will not [illegible] geometrically true.
do not p [illegible] s which are exactly
observ [illegible] give us approximati
the tr [illegible] n any assignable q
small, [illegible], may be esteemed as
to the [illegible] conclusion. To proce
be of n [illegible] pplication of those p
linear [illegible] in every book of flux
been giv [illegible] may probably be of som
as it m [illegible] y serve to elucidate th
science, [illegible] been the instrument b
the improvements in philosophy have
light. The principle of such a science o
blished upon a sure foundation; and shou
and be of any use in removing the cavil
made against the fluxionary calculus, a se
to philosophy, and the writer of this es
hope to be excused for endeavouring to
thing towards elucidating the elements o
ful but too much neglected studies.

I remain, sir,

Your very h

Boston, Sept. 10, 1810.

XXXV. *Comparative Examination of the Mucous Acid formed by the Action of the Nitric Acid: 1st, on the Gums; 2dly, on the Sugar of Milk. By M.* LAUGIER*.

M. VAUQUELIN ascertained by his experiments on gum arabic and gum tragacanth the existence of a very considerable quantity of lime in these substances.

The perusal of his experiments suggested the following reflections:—

1. What becomes of the lime contained in these gums when we treat them by the nitric acid, with the view of procuring mucous acid?

2. Is it not combined with the oxalic acid which is formed almost at the same time with the mucous acid?

3. The oxalate of lime being more insoluble in water than the mucous acid, is it not precipitated with this acid, when we wash the residue after the operation? and does it not alter in a sensible manner its properties?

4. What ought to be the means of ascertaining the presence of the oxalate of lime in the mucous acid obtained from the gums, and of separating this calcareous salt from the acid whose purity it injures?

With a view to resolve these questions, I undertook the following experiments:

I digested with eight parts (480 grammes) of pure nitric acid at 360° one part (60 grammes) of gum tragacanth; I heated the mixture until it was reduced into a honey-like substance, and I added a sufficient quantity of water.

The latter would not dissolve a white pulverulent matter, which I gathered on a filter, and which when dried in the air weighed 9 grammes and a half, and this was mucous acid. The liquor containing the soluble portion of the mixture was of a yellow colour. I evaporated it, and did not take it from the fire until I saw it covered with a slight pellicle which was formed at its surface: by and by, upon cooling, the liquor deposited a great quantity of crystals, some in laminæ, others in needles very well defined as oxalic acid. With the view of separating this last acid from the mucous acid, I poured upon the mixture alcohol at 40°, which dissolved the oxalic acid without touching the mucous acid which I collected on a filter. The second portion of mucous acid weighed two grammes 0·10. The alcoholic solution furnished, on a gentle evaporation, a co-

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* *Annales de Chimie*, tome lxxii. p. 81.

loured mass, which I redissolved in wate
tain whiter and purer crystals.

The mother waters of this second po
acid and of oxalic acid, contained a mixt
malic acids, which I separated from each
which I shall not detail, because they wou
from the principal object of my experimen

The first portion of mucous acid w
weighed nine grammes and a half, it was v
dried it had the grumous appearance of s
the substance which I employed in my e
jected the second portion, because it did
the sam

W ascertaining the prese
of lin us acid, I mixed one
acid n parts of distilled w
this m nine grammes and
acid. whole to a heat of
during s, taking care to sti
time, action of the solven
super n which ammonia
duced n of a white earthy
ment ll the physical prop
careo

A f weak nitric acid, a
ment g liquor, and left to
same d with ammonia a
oxalat

It re portions of weak nitric
added, to clear entirely from oxalate of
acid submitted to the experiment. Eve
monia, when mixed with the decanted a
separated from it a quantity of calcareous
portion of which diminished at each diges
manner. The ninth portion exhibited
traces of it.

The eight precipitates united together g
of two grammes three decigrammes.

It was important to ascertain, if this s
was foreign to the mucous acid, and whos
physical characters appeared to me to be
of the oxalate of lime, was really this calc

With this view I boiled this substance,
solution of carbonate of potash; and wh
decomposition of the two salts seemed to
pleted, I collected on a filter the portio

posited. This sediment, not so white as the first calcareous salt, and in coarser powder, was dissolved with great effervescence in the nitric acid. Its solution, which was of a sharp pungent taste, was not precipitated by ammonia itself, but very abundantly by the oxalate of ammonia.

The liquor which floated above this carbonate of lime, and which contained an excess of carbonate of potash, was supersaturated by the acetic acid and evaporated to dryness; the residue was treated by alcohol, in order to separate the acetate of potash from the oxalate of the same base which is not soluble in this liquid. The mixture when heated for a few moments was thrown on a filter, where the oxalate of lime remained, whereas the alkaline acetate passed through with the alcohol.

The portion insoluble in this liquid was dissolved in distilled water: a drop of this solution mixed with half a spoonful of lime water, formed in it a pulverulent precipitate, evidently oxalate of lime; and the same solution furnished by evaporation crystals of oxalate of potash.

The experiments which I have described, cannot leave any doubt as to the nature of the calcareous salt, the presence of which alters the purity of the mucous acid obtained from gum tragacanth.

The same experiments repeated on gum arabic, and on that which is known in commerce by the name of gum of Bassorah, which is insoluble in water, furnished me with nearly the same results.

I observed, that in proportion as the mucous acid lost by the nitric acid the oxalate of lime which rendered it impure, it assumed a more flaky appearance.

Scheele, to whom we owe the discovery, at first called it saccho-lactic acid, because he obtained it by treating the sugar of milk with nitric acid. This denomination ceased to be convenient, the moment it was proved that it might be procured from the gums by a similar process; and this induced M. Fourcroy to substitute the appellation of mucous acid for that of saccho-lactic acid.

But is the mucous acid furnished by the sugar of milk perfectly similar to that which we obtain from the gums? Is it altered like the latter by containing a remarkable quantity of oxalate of lime; or rather does it contain but the smallest quantity of this calcareous salt; or, finally, is it totally deprived of it? It appeared to me to be interesting to find out a solution for these questions, and I set about applying the process just described to the mucous acid furnished by the sugar of milk.

I took, in consequence, one part of su
I boiled with eight parts of nitric acid of
with the foregoing. I separated by de
portions of mucous acid which were fo
to the residue a new quantity of nitric
portion of mucous acid was deposited, v
with the first gave a total weight of twelv
fifth part of the sugar of milk submitted

I remarked that, after washing, this
luted in water, had an appearance equall
the gum when it was deprived of its o
the weak acid. This remark inclined
this ac purer than that of t
opin ned by the nitric a
actio l not take up from it
tity c ne, after a long conti
the am t take the slightest e
natant

In s, what leaves no de
fect ucous acid of the su
circ easily and entirely d
wate solubility in boiling
it en purity than the muc
ever r has been purified b
men the latter, well fr
lime, , when it is boiled with
insol natter forming the o
which s gray horny semitransp
in appearance to the mucous substance
molecules of animal concretions, alth
coals it does not furnish the ammoniac
of animal compounds, and which furni
carbonate of lime. The too small qu
tained of it did not admit of my making
would have thrown more light on the n

From the facts detailed in this men
the following consequences :

1. There exists a very remarkable diff
mucous acid procured from gums, and
tain from the sugar of milk by the actio

2. This difference consists in the fi
altered by the mixture of a quantity of
proportion to that of the earth which
whereas the mucous acid of the suga
offer the slightest trace of this calcare
perfectly pure.

3. We may procure the mucous acid from the gum in the same state of purity, by a very simple process, which consists: 1st, in taking from it, by successive digestions in very weak nitric acid, the whole of the oxalate of lime which it contains: 2d, in boiling it in water, which dissolves it without dissolving the flaky matter which the nitric acid does not take up.

4. When thus deprived of substances foreign to its nature, the mucous acid of gum is entirely similar to that of the sugar of milk, enjoys all the properties which characterize this acid, and may be employed with the same advantage in the most delicate experiments which require that this acid should be of a perfect purity.

I am convinced that there is a circumstance in which the mucous acid obtained from gum is mixed with mucite of lime, instead of the oxalate which I have mentioned. This happens when we substitute in the preparation of the mucous acid, the nitric acid diluted in water, instead of the concentrated nitric acid, and consequently when we conduct the operation slowly instead of hastily. It is easy to ascertain the difference of the results which we obtain.

If we employ the weak acid, the mucous acid is at first produced alone, and it is precipitated, carrying with it the lime, with which it forms a salt nearly insoluble, and we may separate it from the mixture before the formation of the oxalic acid, which requires the concentration of the acid. If, on the contrary, we make use of concentrated nitric acid, the formation of the two acids, although always successive, is very thick; and we may easily conceive that in this case the oxalic acid, as soon as it is formed, seizes the lime, in virtue of the more powerful affinity which it exercises on this earth.

I shall add another fact which led me to recognise a singular property in mucous acid, which I intend to examine more in detail than I am able to do at present.

When we gently evaporate to dryness the solution of pure mucous acid made in boiling water, without separating the crystalline sediment which is formed during evaporation, we observe that the moment there is no more liquid, the crystals become yellow, then brown, and are converted into a viscous tenacious-like matter, which undergoes a kind of fusion, and becomes very hard on cooling. The mucous acid which has undergone this change, has a much more acid taste than usual; it is infinitely more soluble in water,—has become entirely soluble in alcohol, and has therefore changed its properties in part. I thought

at first that I had thus produced the c
mucous acid either into malic acid or ta
the experiments which I made to verify
not yet appear sufficient to permit me to v
on the nature of the change which takes
periment which I have described.

XXXVI. *On the Prussic and Prussous*
R. Porrett, *Junior, of the T*

Considerable differences of opinion
most ists respecting the c
pruss greeing with Fourcr
that its component part
Ber , who dispute its pres
in h prussiates, asserts,
fact xygen to make a par
that nown affinities of it
add nstances under whi
can ght that it does."
opi nt of some decisive e
may for ever at rest ;
am am induced to thi
scri

S I proposed to mys
a m ng a triple prussia
pure ould be free from
which in general use are s
ing on the means most likely to attain t
to me, that I should succeed if I decom
iron by double elective attraction rath
employing, instead of a pure potash, th
bination with a substance uniting the
ility when combined with potash, st
oxide of iron, and insolubility when u
The only substances I could think of
requisite properties were the succinic ac
the high price of the former precluded
pose, I determined to employ the latte
one ounce of dry sulphuret of potash, a
half of the best prussian blue, previous
powdered, and put them into a Floren

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* From *Transactions of the Society for the Enco*
ures, and Commerce, vol. xxvii.——The Society
to Mr. Porrett for this communication.

filled with distilled water; a disengagement of sulphuretted hydrogen, of ammonia, and of caloric immediately took place. The materials were boiled slowly together for three hours, occasionally replacing the water which evaporated. The whole was then thrown on a filter; what remained on the filter was black, and consisted of sulphuret of iron and undecomposed prussiate of iron. The liquid that passed through, I found on trial to consist of triple prussiate of potash, and hydroguretted sulphuret of potash. In order to complete the decomposition of the latter, I boiled the liquid again, for the same time as before, with another half ounce of prussian blue, and when cold filtered it. The filtered liquid (A) was now nearly colourless, and free from hydroguretted sulphuret. On pouring a little of it into a solution of oxy-sulphate of iron, I was very much surprised to find that solution changed to a deep blood-red colour, without any precipitate ensuing, instead of forming with it a precipitate of blue prussiate of iron. So unexpected a phænomenon determined me to undertake an examination of this liquid; with this view I subjected it to the action of the chemical agents mentioned in the following table.

TABLE I. with Liquid A.

Chemical Agents.	Effects.
Paper stained with turmeric	No change of colour.
Paper stained with litmus	Do. Do.
Potash	No disengagement of ammonia, nor any apparent change.
Lime	Do. Do.
Diluted sulphuric acid	An expulsion of sulphurous acid; the liquid becomes slightly opalescent.
Nitric acid (pure) ...	The acid assumes a red colour, but this effect is not permanent.
Oxy-muriatic acid	This acid loses its smell.
Muriatic acid (pure) .	No change.
Muriate barytes	A white precipitate.
Tincture of galls	No change.
Nitro-muriate platina	A heavy brilliant ochre, yellow precipitate.

TABLE I.—(Continued

Chemical Agents.	Effec
Muriate gold	Dark olive brow
Nitrate silver	A precipitate a quickly passir and lastly to
Sulphate silver	A dull white precipitate.
Oxy-nitrate mercury .	A white precipi
Oxy-nitrate lead	A white precipi
Supersulphate copper	A dull white pr
Muriate bismuth	No precipitate.
Sulphate iron	No change.
Oxy-sulphate iron ...	The solution ass red colour.

The effects of the sulphuric acid a
barytes clearly proved the existence of
in the liquid, while that of the oxy-sul
cated the presence of some other prin
liquid was indebted for its peculiar char
tion of this principle in a pure state
necessary preliminary operation to its
a few trials I succeeded in effecting thi
following is the process I employed.

The liquid was evaporated by a gent
upon the saline residuum alcohol was po
to extract any thing: by this means th
phite and sulphate of potash was left bel
hol when filtered held in solution that p
the red tingeing property with solutions
hol was now got rid of by distillation, a
in the retort was redissolved in water.
gave the following results with the dif
lutions.

TABLE II. with Liquid

Metallic Solutions.	Effe
Nitro-muriate platina	A precipitate Table I. but tity, and lon
Muriate gold	Light olive pre reduced.

TABLE II.—(Continued.)

Metallic Solutions.	Effects.
Nitrate silver	A grayish white precipitate, not changing colour.
Sulphate silver......	A clear white precipitate.
Nitrate mercury.....	A copious white precipitate.
Oxy-nitrate mercury.	A white precip. in small quantity.
Nitrate lead	No precipitate.
Oxy-nitrate lead	No precipitate.
Superacetate lead....	No precipitate.
Hyperoxymuriate lead	A slight white precipitate.
Supersulphate copper	A dull white precipitate.
Muriate tin	No precipitate.
Muriate bismuth....	No precipitate.
Sulphate iron.......	No change.
Oxy-sulphate iron...	Same as Table I.
Oxy-sulphate manganese	The crimson colour disappears; no precipitate.
Sulphate zinc.......	No change.
Nitro-muriate cobalt.	No precipitate.
Nitrate nickel	No change.

It is necessary to remark, that in the preceding table, as well as in Table I, several of the nitrates and muriates were slightly reddened, though not in a degree to be compared with the oxy-sulphate of iron. I have not noticed this in the table, because I am not certain whether this effect was not owing to a minute portion of oxide of iron which might have been introduced into those solutions by the acids employed to make them, as both the nitric and muriatic acids of commerce generally contain some; an excess of nitric acid, even if pure, might also cause this effect, as Table I. may convince us. The solutions with which this effect occurred to me were those of bismuth, silver, mercury, lead, cobalt, gold, and platina.

The liquid B is not altered by exposure to the air.

Its effect on oxy-sulphate of iron is the same, whether this sulphate is neutral, or contains an excess of acid, or is supersaturated with carbonate of ammonia.

Sulphuric acid destroys the colour produced on oxy-sulphate of iron, provided the three liquids are in a concentrated state. If there is much water present, no change ensues.

Having obtained the tingeing principle B, separate from

the other salts with which it was cont
myself to what were its formation and th
appearance of the prussic acid, during the
owing? I could imagine but five causes
likely to have been efficient, concernin
made a question to be resolved by experi

Question I. Was it owing to the com
the oxide of iron from the triple of pr
phur, and the subsequent decomposition
siate by the heat of ebullition long conti

Question II. Was it owing to the
phurous acid produced?

Q is it owing to the act
rette

Q as it owing to a c
pruss nd sulphur?

Que s it owing to the de
pruss hydroguretted sulph

T st question, it is onl
tend afforded by long-c
the of potash. I shall
as I led by professor Pro

T te of ammonia, car
and ssiate that escapes de
afte cessive distillations:
no a the products of thi
the had the latter cont
potash, it must have changed turmeric
it contained carbonate of ammonia, it n
same, and likewise have given out amm
potash and lime were added; it must als
the solution of copper; and had it con
potash, it must have produced prussiate c
to the green sulphate of that metal: it
ferring to Table I. that none of these effe
Were further evidence necessary of the
two liquids, it might be mentioned th
poured alcohol on the saline residuum o
the prussiate, which took up a part th
prussiate of potash: had any of the tin
present, the alcohol must have dissolved
it could not have escaped his observation.
fore, ample grounds for negativing the fi

In order to answer the second questi
phurous acid gas for a long time through
ple prussiate of potash; the prussic acid

sulphite of potash formed; but this sulphite was not mixed with any tingeing salt. On the supposition that the disappearance of the prussic acid, in the liquid A, might have been owing to its having been expelled entirely by the sulphurous acid, and that the tingeing liquid resulted from the mutual action of the other principles, namely, the oxide of iron and hydroguretted sulphuret of potash; I subjected a mixture of these materials to long boiling, but could not by this means produce a liquid that tinted oxy-sulphate of iron red. Sulphurous acid gas, passed through water in which prussian blue was diffused, did not in the least affect that compound. These experiments completely refute the opinion on which the second question was grounded.

To enable me to reply to the third question, I passed sulphuretted hydrogen gas for several hours through a solution of triple prussiate of potash, on which it was found to have no effect.

We shall be little disposed to allow that there is any foundation for the fourth question, when we consider the circumstances of the last-mentioned experiment, in which sulphur in the state of the most minute division was offered to the triple prussiate, without any combination ensuing; and also when we compare the effects of the metallic solutions in Table II. with those which would ensue with liquids containing sulphur. But, if any doubt should still be entertained on this subject, the following experiment will perhaps remove it: Into a solution of prussiate of mercury throw some pieces of phosphuret of lime, the oxide of mercury of this prussiate will thus be reduced and separated from the liquid which is to be filtered; some of this liquid poured into carbonate of iron turns it red, the red colour soon disappears, and a white precipitate begins to form; this white precipitate soon changes to green, and if a little nitric or oxy-muriatic acid be now poured upon it, it becomes a perfect blue prussiate of iron. This experiment, in which a liquid turning a solution of iron red was produced without the employment of a particle of sulphur, goes very far to negative our fourth question; and when considered in conjunction with the preceding ones, we can hardly do otherwise than dissent from the supposition which gave rise to that question.

But if the experiment last adduced tends to refute the fourth question, it very strongly supports the fifth; for the changes of colour observable were undoubtedly owing to successive stages of oxidation by the contact of the atmosphere. In confirmation of this question, it may likewise

be asserted, that the long boiling witl
sulphuret is a powerful de-oxidating pro
said to me, If it is really true that the p
de-oxidated by this process, you ought t
pose that acid from the solution B t
struck me very forcibly; and being a
last proof of the truth of my deduction
recomposition of this acid by several
for some time without success: I had a
particular satisfaction of succeeding
agency of nascent hyper-oxymuriatic
I em following:

A muriate of potash w
tom Over this some of
with iluted sulphuric aci
heat then applied to the t
and as ent action comme
withd process the prussi
duc red beyond the pos
by t blue prussiate of iro
to a and red sulphate o
pru produced at once,
the c acid, a solution o
iron acid.

H eeded in proving tha
ciple was sub-oxidized pr
object that principle in a
must recollect that we have hitherto con
combination with potash, with which it
salt; this circumstance gave me reason
acid, and I therefore determined to atte
by abstracting its base by a stronger aci
was the process I employed for the purp

The liquid B was evaporated nearly to
into a retort with diluted sulphuric acid;
adapted to it, and about two-thirds of
over by a gentle heat; what remained in
phate of potash. The receiver contained
with a faint, sour, disagreeable smell,
taste. This liquor I have named, in co
principles of the new nomenclature, *pru*
salts *prussites*, of which the liquid B co
known, namely the prussite of potash.

The effects of the prussous acid on th
these solutions, as far as I have tried then
following table.

TABLE III. with Prussous Acid.

Chemical Agents.	Effects.
Muriate lime	No change.
Muriate barytes	No change.
Muriate gold	The gold precipitated metallic.
Sulphate silver	Copious white precipitates.
Nitrate silver	
Prussiate mercury	No change.
Nitrate mercury	Copious grayish white precipitate.
Oxy-nitrate mercury	Very slight precipitate white.
Oxy-sulphate iron	Solution turns blood-red. No precipitate.
Nitro-muriate platina	No precipitate.
Nitrate lead	No change.
Oxy-nitrate lead	Solution becomes red, but hardly any precipitate formed, unless heated, in which case a copious white precipitate ensues. The red colour disappears, a rapid action takes place between the two liquids, and some of the nitric acid of the solution is decomposed.
Hyper-oxymuriate lead	A slight precipitate, probably of muriate of lead.
Super-sulphate copper	Solution becomes slightly turbid.
Muriate bismuth	No precipitates.
Nitrate nickel	
Muriate tin	
Nitrate cobalt	
Sulphate iron	
Sulphate manganese	
Sulphate zinc	

I cannot conclude this part of my memoir without giving a more simple and expeditious process for preparing prussite of potash, than that which I at first discovered. It is the following:

Pour a solution of prussiate of mercury into hydroguretted sulphuret of potash, till the mutual decomposition of the two liquids is completed; prussite of potash is instantly formed, and may be separated by filtration from the solid combination of the sulphur and mercury.

I wish also to observe, that the pro
blue I have mentioned for boiling wit
much larger than is necessary, as I have
obtaining prussite of potash when the pro
blue was only equal to that of the sulphu
with the latter in two distinct and eq
prussite of potash thus obtained is, how
much larger quantity of hydroguretted su
a greater portion of prussian blue is empl

Whether the prussous acid can be a
time and future experiments must decide.
to be a very delicate test of silver and of

Th eriments, by provi
oxyg id, give it a stronge
fore ing placed among th

Th d possessing strong
than curious, though not
of th gen in diminishing
quan rtain fixed proportio
the p alogous to the oxy-

T empt which gave ris
that of this memoir, I
that d in producing pure
pota the process before t
prod ensued, and by su
tion rom sulphates and su
of b lphur by acetate of
from the acetate of potash thus formed b
but on account of the complication of thi
tate to recommend it for general use.

Tower, London, April 21, 1809. ROBERT

P.S.—It is essential to the success of t
which the prussic acid is regenerated from
nascently per-oxygenized muriatic acid,
acid remaining in the liquid, after the oxy
should be neutralized by an alkali previous
the solution of iron, which should like
neutral.

May 8, 1809. ROBERT

X

XXXVII. *Memoir on the Muriate of Tin. By M.* Berard, *Ex-Professor of Chemistry in Montpellier.*

The preparation on a large scale of muriate of tin has furnished me with the opportunity of observing some interesting facts. They will serve as appendages to those which have been described by various writers, and perhaps may tend to accelerate the discovery of an uniform and certain method of preparing and using the *composition for dyeing scarlet,* which is a kind of salt of tin.

The solution of tin by the muriatic acid, as described by various authors, and as practised by Baumé, is operated by pouring on one part of the metal in a state of very minute division, four parts of common muriatic acid, and assisting the chemical action by the heat of a sand-bath. The water, which serves as a vehicle to the acid, is decomposed, the oxygen oxidizes the metal, which is then combined with the acid, while the hydrogen is liberated in the gaseous state, carrying off with it some particles of the metal employed, which render it very fetid. But the action is slow, and the solution is effected in an imperfect manner. I observed that a very great part of the acid employed was evaporated and lost; and that if we wished to operate the entire solution of the metal, we must not only add acid in the place of that which was evaporated, but also keep up the action by heat for several days. I tried to perform this operation in the cold way, and two months were insufficient. Bayer and Charlard, in their inquiries upon tin, employed six months in the operation.

M. Chaptal assists the chemical action between the muriatic acid and the tin, by placing, when he has prepared this acid, the metal in the vessels of Woolf's apparatus, in which was the water which ought to receive the vapours. The heat which is extricated produces the best effect, and the action becomes very brisk towards the end of the operation. But this ingenious contrivance still leaves something to be desired, in so far as the acid only dissolves the fourth of its weight of tin, and we must terminate the solution by other means.

We may operate the solution of tin still better by receiving into a large receiver, in which we have introduced a sufficient quantity of the metal in minute division, the vapours of muriatic acid, which are liberated from a mixture of muriate of soda in powder and of sulphuric acid weakened to the 40th degree of Baumé's areometer. On operating a

simple distillation in this manner, the va
acid are very easily condensed and combin

If we direct vapours of oxygenized m
vessel containing tin and common mur
lution takes place perfectly, and in a shor
at 20° then takes up one third of its weig

I tried various mixtures of muriatic ac
from one-sixth part of the latter up to
them acted on the tin with extreme heat
substances being forcibly ejected from the
of nitric acid, or the aqua fortis of com
Baumé's areometer, and twelve parts of
acid, at 20°, form a mixture very well ac
lution is thereby operated
time. mixed takes up abo
weigl solution extends to t

I t action of the muria
of th , alternately concur
small ting its solution, and
pletel pose I filled a large
the ti t with muriatic acid
hours the acid into another
alrea e 25th degree. The t
the in contact with the
abso eric oxygen gas, an
loric, e metal very hot, and
when saucer is speedily e
soon an to cool, I replaced
acted wi r, and in a short tim
35°. I withdrew it again, in order to give
air to the tin, and I replaced it in the same
might once more act. I repeated this ope
to time until the action ceased. In two d
was at 45°, which it would attain even in o
ployed a series of saucers filled with tin: wh
upon some of the vessels, the air acts on th
the operation is never interrupted.

The muriatic solution of tin, when re
speedily with oxygen gas from the atmosp
Pelletier, Guyton Morveau, and other cele
have observed. It is sufficient to turn upsi
glass, full of atmospheric air, on a capsule
with this liquor, in order to see the latter
bell-glass until all the oxygen be absorbed.
is still more rapid, and becomes almost total

when the bell-glass is filled with pure oxygen gas. In order to facilitate the combination of the oxygen gas with this recent solution, I made to pass through it a great quantity of atmospheric air, by means of a pair of bellows, the pipe of which goes to the bottom of the liquor. When it is not sufficiently saturated with tin, it takes a new portion of it in proportion as it absorbs the oxygen from the atmosphere.

The oxygenated muriatic acid gas is absorbed by this solution with great energy, as Pelletier has very aptly observed. He had even proposed the solution thus saturated with oxygenized muriatic acid gas for dyeing scarlet. I requested several artists to try it, but none of them adopted it. It should seem that the combination of atmospheric oxygen gives it nearly the same properties with those of oxygenized muriatic acid gas. When it has absorbed much of the oxygenized muriatic acid gas, it becomes proper for dissolving a new quantity of tin; and as soon as it has dissolved it, again its state becomes changed, having become capable of absorbing more oxygen.

The muriatic solution of tin at 45° of density gives upon evaporation crystals of muriate of tin. The crystallization takes place the more easily the less recent the solution is, or the greater the quantity of oxygen which the solution has absorbed. The mother water, which swims above the crystals, is of a very great density, particularly after several crystallizations. The density is still more considerable, if we set it to evaporate before exposing it to the air. It is even sometimes slightly smoking, and may then furnish crystals by diluting it with pure water. A flask containing 14 parts of distilled water, contained 28 parts of mother water coming from the first crystals. The same flask contained 31 parts, when this same liquor had given by concentration several layers of crystals. These mother waters are capable of being combined with the oxygen of the atmosphere, when the solution has not been previously saturated with it. It is sufficient to expose them to the air, or to act with a pair of bellows, as already pointed out with regard to the simple solution. This combination produces more crystals; and if the exposure of the mother waters to the air takes place over a very great surface, we obtain a muriate of tin crystallized in very thin and slight scales Baumé had observed this last method of crystallizing. The oxygenized muriatic gas is combined with the mother waters with a good deal of energy, a considerable quantity of caloric is extricated, and after cooling, the liquor goes into a silky

a silky mass of crystals of muriate of t
the crystals of muriate of tin by solutio
and by crystallization, they assume mor
more density.

The crystallized muriate of tin is ver
water; the solution takes place very spee
a considerable decrease of temperature.
of temperature in the experiments which
Reaumur, the temperature of the atmosph
substances employed being 5°. The mix
waters and of pure water produces no chan

As I had observed that these mother
little fuming on being concentrated, I tri
the l ated mother waters
muri uld not obtain a mu
to th own by the denomi
liquo obtained at first a w
and a uriate passed into t
it was the neck of the retor
form he name of *butter o*
same muriatic acid gas a
thro ated mother water o
it be d gave crystals on
pure ught to observe tha
quor ales vapours much t
abun and the denser it is.

Th of muriatic acid an
of so als and of mother
effect ss of acid; and we s
been said, that all of them are susceptible
tions in their state. We must not be asto
if the results which they produce in dyein
and so different from each other. The l
of muriate of tin seems to be that of ver
formed crystals. It is in this state that th
always to be employed in dyeing, by ass
greater or less quantity of pure nitric a
the shade which we wish to obtain: su
can alone be always uniform and give con

By taking advantage of the facts cont
moir, it would be easy to describe a simple
process for preparing on a large scale the
crystals: I have nevertheless met with som
ing difficulties in the execution, which I
removing; and the full description of m
given in a subsequent memoir.

XXXVIII. *The Case of a Man who died in consequence of the Bite of a Rattle-snake: with an Account of the Effects produced by the Poison.* By EVERARD HOME, *Esq. F.R.S.**

OPPORTUNITIES of tracing the symptoms produced by the bite of poisonous snakes, and ascertaining the local effects on the human body when the bite proves fatal, are of such rare occurrence, that no well described case of this kind is to be met with in any of the records that I have examined. I am therefore induced to lay before this Society the following account, with the view of elucidating this subject, in which the interests of humanity are so deeply concerned.

Thomas Soper, 26 years of age, of a spare habit, on the 17th of October 1809, went into the room in which two healthy rattle-snakes, brought from America in the preceding summer, were exhibited. He teased one of them with the end of a foot rule, but could not induce the snake to bite it, and on the rule dropping out of his hand, he opened the door of the cage to take it out: the snake immediately darted at the hand, and bit it twice in succession, making two wounds on the back part of the first phalanx of the thumb, and two on the side of the second joint of the fore finger. The snake is between four and five feet long, and when much irritated bites the object twice, which I believe snakes do not usually do.

The bite took place at half past two o'clock. He went immediately to Mr. Hanbury, a chemist in the neighbourhood. There was at that time no swelling on the hand, and the man was so incoherent in his language and behaviour, that Mr. Hanbury considered him to be in a state of intoxication, and gave him a dose of jalap to take off the effects of the liquor, and made some slight application to the bites. It appeared on inquiry, that the man had been drinking, but that before he was bitten there was nothing unusual in his behaviour. After leaving Mr. Hanbury, the hand began to swell; which alarmed him, and he went to St. George's hospital. He arrived there at three o'clock. The wristband of his shirt had been unloosed, and the swelling had extended half way up the forearm before his admission. The skin on the back of his hand was very tense, and the part very painful. At four o'clock the

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* From the Philosophical Transactions for 1810, Part I.

swelling extended to the elbow, and at h
reached half way up the arm, and the p
to the axilla. At this time Mr. Brodie,
my absence, first saw him: he found
man's answers were incoherent: his pul
in a minute, and he complained of sick
of aqua ammoniæ puræ, and thirty drops
vitriolici in an ounce of mistura campho
him, but did not remain on his stom
were bathed with the aqua ammoniæ pur
forearm had compresses wetted with c
applied to them. At five o'clock he too
spiri mpositus, and 30 d
an f of mistura camp
mai. ach. At six o'cl
stro t seven his pulse w
30 d the same quantity
puræ water. At half pas
peat lock he had the fe
pres was cold, his pulse
stro The dose was in
of and repeated. At
o'c become very violer
pul ut fits of faintness
15 the pulse was not
the s were less depresse
of ad two stools. A
o'cl im. The hand, w
arm were much swelled up to the top of
into the axilla. The arm was quite c
could be felt in any part, not even in th
ing preventing me from feeling the axill
degree of accuracy. The wounds made
just perceptible; those on the finger
His skin generally was unusually cold.
to diminish his alarm of danger, and f
fectly collected: he said he hoped he s
one o'clock in the morning of the 18t
stinctly: his pulse beat 100 in a minu
faintness came on occasionally. The
peated every hour.

At eight o'clock in the morning of t
beat 132 strokes in a minute, and was
swelling had not extended beyond th
neck, but there was a fulness down
was extravasated under the skin as low a

the back on the right side a mottled appearance. The whole arm and hand was cold, but painful when pressed; the skin was very tense; on the inside of the arm below the axilla, and near the elbow, vesications had formed; and under each of the vesications there was a red spot in the cutis, of the size of a crown piece. The skin generally over the body had become warm. He was low and depressed; there was a tremulous motion of his lips, and the faintings recurred at nearly the same intervals as in the preceding evening. The last dose of medicine was rejected by vomiting, but some warm wine remained on his stomach. The arm was fomented. At twelve o'clock, in addition to the above symptoms, there was a starting of his limbs. He had attempted to take some broth, but his stomach did not retain it. The skin of the whole arm had a livid appearance, similar to what is met with in a dead body, when putrefaction has begun to take place, unlike any thing which I had ever seen in so large a portion of the living body. An obscure fluctuation was felt under the skin of the outside of the wrist and forearm, which induced me to make a puncture with a lancet, but only a small portion of a serous fluid was discharged. My colleague, Dr. Nevinson, was present at this visit, and we agreed to continue the internal use of the volatile alkali, with the view of rousing the stomach to action, not considering it as having any specific power over the poison. At eleven o'clock in the evening, finding that his stomach did not always retain the medicines, nor even small quantities of brandy, which were given him, I directed the volatile alkali to be left off, and two grains of opium to be given, and repeated every four hours. At this time his pulse was scarcely perceptible at the wrist, the fainting fits were not less frequent. The vesications and red spots were increased in size.

October 19. At nine o'clock in the morning his pulse was scarcely perceptible: his extremities were cold; the vesications were larger, and the size of the arm was diminished. He was drowsy, probably from the effect of the opium. He had taken nothing but brandy during the night. At three o'clock in the afternoon he was more depressed: spoke only in whispers: the vesications were increased: the fainting fits less frequent. The arm was diminished in size, and he had sensation in it down to the fingers. At eleven o'clock at night his pulse beat 130 in a minute, and was low. The opium was left off. A stool was procured by clyster. He was ordered to have a glass

of camphorated mixture occasic
as often as he could be induced
October 20. He had dozed
his spirits were better, and h
nine o'clock he took coffee fo
took some fish for dinner, but
stomach ; he therefore took br
half an ounce at a time, as larg
on his stomach.

October 21. He had slept
but was occasionally delirious :
Brandy and jelly were the or
be size of the ar
tender.
. He had slept
s pulse beat 98
er, and brandy at
me full and stron
dy. The right s
lamed and painf
om the extravasa
. His pulse con
though reduced i
e exposed cutis
were procured b
al and porter fo
evening he had

October 24. There was no

October 25. His pulse ha
in other respects he was nearly
opened by medicine.

October 26. The arm was

October 27. The inflamm
creased : his tongue was furre
quent. He attempted to sit
and the pain prevented him.
spirits of wine and aqua amm
tities.

October 28. A slough ha
inside of the arm below the a
on, for which he was ordered
In the night he had a rigor.

October 29. The purging
100 in a minute, and was
formed on the outside of th

and half a pint of reddish brown matter was discharged with sloughs of cellular membrane floating in it. The lower part of the arm became much smaller, but the upper part continued tense. A poultice was applied to the wound. The lower portion of the arm and the forearm were covered with circular stripes of soap cerate. He was ordered to take the bark, and allowed wine and porter.

October 30. The redness and swelling of the upper part of the arm had subsided: the pulse was 100 in a minute. The purging had returned. The bark was left off: the chalk mixture and laudanum were given, and an opiate clyster administered.

October 31. The pulse beat 120 in a minute. The discharge from the abscess had diminished, the purging continued, and at night he had a rigor.

November 1. The pulse was 120. His voice was feeble; he had no appetite; was delirious at intervals. Ulceration had taken place on the opening of the abscess, so that it was much increased in size. He drank two pints of porter in the course of the day.

November 2. His pulse was very weak; his countenance was depressed; his tongue brown; the ulceration had spread to the extent of two or three inches. Mortification had taken place in the skin nearer the axilla. His stomach rejected every thing but porter: in the night he was delirious.

November 3. The mortification had spread considerably: the purging continued: the forefinger, which had mortified, was removed at the second joint.

November 4. He died at half past four o'clock in the afternoon.

Sixteen hours after death, the body was examined by Mr. Brodie and myself, in the presence of Mr. Maynard, the house surgeon, and several of the pupils of the hospital.

With the exception of the right arm which had been bitten, the body had the natural appearance. The skin was clear and white; and the muscles contracted.

The wounds made by the fangs at the base of the thumb were healed, but the puncture made by the lancet at the back of the wrist, was still open. That part of the back of the hand, which immediately surrounded the wounds made by the fangs, for the extent of an inch and a half in every direction, as also the whole of the palm, was in a natural state, except that there was a small quantity of extravasated blood in the cellular membrane. The orifice of the abscess was enlarged, so as to form a sore on the outside

of the arm, elbow, and forearm
Around this, the skin was in a
than half way up the outside o
wards, on the outside of the
hered to the biceps flexor m
muscles in the forearm, by a
brane. Every where else in th
axilla downwards, the skin wa
and between these parts ther
with an offensive smell, and sl
resembling wet tow, floating i
natural appearance every wh
which was next the abscess.
abscess, blood was extravasate
and this appearance was obser
back as far as the loins, and
over the serratus major anticu

In the thorax the lungs h
The exterior part of the loose
it is exposed, on elevating the
a dried bladder. The cavity
half an ounce of serous fluid
ance, from an admixture of
into the aorta, a small quanti
had a similar appearance. T
tained coagulated blood.

In the abdomen, the cardia
moderately distended with fl
much contracted; the inter
very turgid with blood. Th
healthy appearance. The ga
of healthy bile. The lacteal
empty; they had a natural a

In the cranium the vessels
were turgid with blood; th
more water than is usual, a
cells connecting the pia mat
is to be observed, that thes
its membranes are very frequ
diseases, which terminate fat

The following cases were
friend Dr. Patrick Russell:
and Mr. Claude Russell ve
knowing the subject of th
taken an interest. As the
circumstances with that wh

inserted them in this place, as well as an experiment which I had an opportunity of making in the West Indies, on the effects of the snake's poison on animals.

A boy, a slave of a gentlemen in India, was bitten by a snake called Kamnlee by the natives, in the lower part of the arm, at eight o'clock in the evening. The blood flowed very freely for some time. He died next day at noon in great pain.

A sepoy, 60 years of age, was admitted into the hospital of his regiment, under the care of Mr. Perrin, assistant surgeon, at four o'clock in the afternoon of the 15th of October, 1802, in consequence of his being bitten by a cobra di capello on the back part of the hand. At the time of his admission he complained of pain running up the arm. He immediately took a drachm of eau de luce, and this dose was repeated every half hour, and the same remedy was applied externally as a lotion to the arm and forearm. At four o'clock in the morning of the 16th of October, the pain began to increase, and the arm to swell with great hardness and stiffness, and tumour in the axilla, with much inclination to vomit. He took twelve grains of Dr. James's powder, which brought up a great quantity of bilious matter. He drank copiously of warm water, but no perspiration was induced. He appeared relieved for a short time. At eight o'clock in the morning the arm was distended, painful, and discoloured. He took four ounces of brandy, and repeated it every hour until twelve o'clock, with a drachm of eau de luce occasionally. At this time he was a little revived. The brandy was reduced to two ounces, which were carefully and regularly given every hour, until twelve at noon on the 17th of October, when the arm was more free from pain, but much swelled, hard, and black: his spirits and pulse also were considerably relieved. The eau de luce was now omitted, but the brandy was continued every hour, until twelve o'clock at noon on the 18th of October, when the stiffness and tumor in the axilla had disappeared; the arm was still swelled, but was softer, and less painful. The brandy was omitted: at night he took six grains of Dr. James's powder. On the 19th of October the arm was less, softer, with little or no pain; a blister was formed and burst on the back of the hand, which discharged three ounces of black fœtid pus. On the 20th, an abscess burst on the hand, in the same situation as the blister, which discharged a large quantity of a fluid having an offensive smell. He was directed to

take a drachm of Peruvian bar
hours. On the 22d the swelli
charge was considerable. From
but slowly recovered, with the
finger, which remained permane
the other fingers were affected i

In this case, the swelling
coming on, and less extensive;
axilla, which preceded it, was
absorption.

In the year 1782, while in the
the following experiment:

A spotted dark coloured snak
having the poison fangs on each
responding surfaces grooved, so
poison, was put into a square ti
which a half-grown rat was co
great terror, and remained crouc
box, with its eyes fixed on the s
some distance, they were allowe
in this situation: I then raised
caused the snake to slide along
came in contact with the rat,
The rat died in a minute after
mediately from the box by mea
The wounds made by the fangs
of blood immediately below th
viding the skin with a scalpel, th
it was found entirely destroyed:
from the ribs, and from a sma
The parts immediately surroun
ingly inflamed; as far as I coul
pearances very much resembl
muscles of a dog's thigh, by
arsenic, in consequence of wh
sixteen hours.

Fifteen hours after the deat
was bitten by the same snake.
and bit the snake in the neck,
died in about ten minutes. T
for about six hours, and then di
death, the bite was found to ha
side of the navel, and the abd
were in the same state as in the
gree.

It appears from the facts which have been stated, that the effects of the bite of a snake vary according to the intensity of the poison.

When the poison is very active, the local irritation is so sudden and so violent, and its effects on the general system are so great, that death soon takes place. When the body is afterwards inspected, the only alteration of structure met with, is in the parts close to the bite, where the cellular membrane is completely destroyed, and the neighbouring muscles very considerably inflamed.

When the poison is less intense, the shock to the general system does not prove fatal. It brings on a slight degree of delirium, and the pain in the part bitten is very severe: in about half an hour, swelling takes place from an effusion of serum in the cellular membrane, which continues to increase with greater or less rapidity for about twelve hours, extending during that period into the neighbourhood of the bite; the blood ceases to flow in the smaller vessels of the swoln parts; the skin over them becomes quite cold, the action of the heart is so weak, that the pulse is scarcely perceptible, and the stomach is so irritable, that nothing is retained in it. In about 60 hours these symptoms go off, inflammation and suppuration take place in the injured parts, and when the abscess formed is very great, it proves fatal. When the bite has been in the finger, that part has immediately mortified. When death has taken place under such circumstances, the absorbent vessels and their glands have undergone no change similar to the effect of morbid poisons, nor has any part lost its natural appearance, except those immediately connected with the abscess.

In those patients who recover with difficulty from the bite, the symptoms produced by it go off more readily, and more completely, than those produced by a morbid poison which has been received into the system.

The violent effects which the poison produces on the part bitten, and on the general system, and the shortness of their duration, where they do not terminate fatally, has frequently induced the belief, that the recovery depended on the medicines employed; and in the East Indies eau de luce is considered as a specific for the cure of the bite of the cobra di capello.

There does not appear to be any foundation for such an opinion; for, when the poison is so intense as to give a sufficient shock to the constitution, death immediately takes place, and where the poison produces a local injury of suf-

ficient extent, the patient also
recover.

The effect of the poison on t
diate, and the irritability of tl
there is no opportunity of ext
fairly taken place, and then ther
effects being produced.

The only rational local trea
dary mischief, is making ligatu
to compress the cellular memb
swelling, which only spreads i
skin; and scarifying freely the
effused serum may escape, and
soon as it is formed. Ligature
but with a different view, na
being absorbed into the system

XXXIX. *On extracting liqu*
Pear

THE high price of sugar in Fr
stances connected with the w
chemists to endeavour to disco
charine substitutes may be ext
stances produced in the Old Wo
buc has lately published in the
experiments on extracting suga
these were chiefly for the purp
tity produced by different varie
duced in France, it will be qu
reader to state the process and

Boil eight quarts of the ju
pan for about a quarter of an l
pose of neutralizing the acid o
rate portions, about two mi
drachms of finely pounded ch
effervescence in the juice, by tl
from the chalk in the form of g
tinued for eight or ten minutes
be kept stirred, to multiply the
juice and the chalk.

The whites of three eggs be
water are then to be added at c
stirred into it for the purpose c

it still boil for a quarter of an hour. The white of the eggs, coagulating by the boiling, entangles the impurities of the juice, which is then to be strained through a flannel strainer supported at the four corners.

When about half-cooled strain it again, that it may be well clarified.

By these operations the juice loses about one-third of its weight. What remains is to be reduced to about one-half of its bulk by boiling; after which the heat must be lowered; but the evaporation must be continued below the boiling point, until the syrup be so concentrated, that on cooling it may be of the consistence of common treacle.

Those who are acquainted with chemical processes will know when it is sufficiently concentrated, by observing the pellicle formed on its surface. A vessel capable of containing a quart, or two pounds of water, will contain 2lb. 10 oz. of syrup or liquid sugar.

This liquid sugar is represented as savoury, fresh, and capable of sweetening water very well, or even milk without curdling it.

In one of M. Dubuc's experiments the juice had a milky look, even after the white of eggs was added. To remedy this, he employed twelve drachms of powdered charcoal, and stirred and boiled the mixture for about ten minutes; after which he strained it once through a conical bag, and when nearly cold passed it through the filter a second time, the sediment of the first filtration being left to make the filter the closer.

The success of this experiment induced him to try to obtain the clarified liquid sugar by using chalk and charcoal only, without employing white of eggs. To six quarts of apple-juice boiled for a quarter of an hour, he added, at four separate times, two minutes from each other, a mixture of seven drachms of chalk, and one drachm of small coal in fine powder. The boiling was continued till the liquid was reduced one half: when half cooled it was passed through flannel, as directed above, and when nearly cold was strained a second time, and lastly it was evaporated with the above-mentioned precautions.

The process for extracting the saccharine matter from pears differs not at all from the above; but more chalk seems to be required to saturate and separate the acid.

If the fruit be suffered to lie bruised for about 24 hours before expressing the juice, the produce of sugar will be greater,—this process contributing in some way or other to the development of the saccharine principle.

When the boiling heat is too
of the syrup becomes darker.
gest that the process might be in
ter bath, instead of applying th
or kettle ?

XL. On Mus

To Mr. T

SIR, IT has long been a matt
musicians, that no method has y
musical time. The terms Largo
&c. seem to be mere terms of
finite characters of time; for i
these terms mean any portion o
performer is left entirely to use hi
To remove this imperfection, Lo
vented an instrument called The
the purpose of measuring time b

But this instrument, though
could never be brought into prac
of adjusting it at the beginning
difficulty which the performer e
mechanical rules.

Another chronometer of a mo
since been invented, consisting
and inches, with a plummet affi
this instrument is to prefix one
ment, and also the length of the
once during its performance.
be attended with as much unce
that a person would be in walki
time it would take him to walk

Although these modes of intr
hitherto failed, yet I am inclined
use of time-keepers, it will be v
future composers to fix the time
be misunderstood even by a you
done very correctly without an
pocket-watch which shows min

Let the composer take notice
and seconds that elapse durin
movement, according to the t
should be played or sung, and l
at the beginning of it. The w

dante, Presto, &c. should still be used as terms of expression,—not as the definite characters of time.

Suppose, for example, that a piece consists of three movements:—the first is performed in 10′, 40″; the second in 6′, 30″; and the third in 8′, 10″. These figures being written at the beginning of each respectively, will convey an exact idea of the author's time to all future performers. And thus a check may be put upon the licentiousness of the fiddle-stick; for some performers are so rapid in their movements, as to neglect both taste and expression. This rapid mode of playing seems to be a growing evil; for it has been said by good judges of the subject, that Handel's music was performed much slower a century ago, than it is in our best concerts at this time.

St. Austin Street,
Sept. 23, 1810.

W.

XLI. *Comparative Analysis of Socotrine and Hepatic Aloes.* *By M.* Tromsdorff. *Extracted by M.* Vogel[*].

Besides the two kind of aloes known by the name of socotrine and hepatic, there are two others, one of which, *lucid aloes*, is extremely rare, and the other, *caballine aloes*, is so inferior, and so variable in its qualities, that M. Tromsdorff did not think it worth alluding to in his inquiries.

After having spoken of the natural history and of the extraction of the juice of the plant, an analysis of the two kinds is given, and it is this part of his work that we proceed to notice.

Experiments on Socotrine Aloes.

Action of water. a.) Four ounces of socotrine aloes pounded, were boiled with three pounds of distilled water in a silver vessel. The aloes, being entirely dissolved, presented a transparent liquid of a deep yellow; but, when allowed to cool, a yellow powder was precipitated. When the liquor was quite cold it was decanted and filtered, and a brown transparent mass remained at the bottom of the vessel.

After desiccation, this substance weighed one ounce, and exhibited the following character:—

1. It was transparent, of a brownish yellow, very brittle, and of a bitter taste.

2. It melted at a gentle heat.

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[*] *Annales de Chimie,* tome lxviii. p. 11.

3. It was insoluble in water,
and in liquid potash.

4. When a lighted candle
with a brisk flame.

From the above it is evident
resinous part of the aloes. It i
this great quantity of resin, join
aloes, is easily soluble in warm
from it on cooling.

b.) The aqueous solution, wl
of dissolved parts, acted in the f

1. It was perfectly transparen
when placed in contact with the
colour, but without being turbid

2. It reddened turnsole paper

3. The alkalis and the alka
of the property of reddening tl
solutions produced no other ch

4. Some drops of muriate of
duced a black colour.

5. The nitrates of silver and
nitric acid restored its transpare

6. The sulphuric, nitric, and
from it a small quantity of a y
like a resin, and which did not

7 A solution of animal gela
in it.

c.) The aqueous solution w
the sand-bath: there remained
of a bitter taste. It was com
cold alcohol.

Ether which was digested w
not coloured with it, and did r
it.

These properties induced the
the aloes for the principle whic
by the name of *saponaceous prin*
essential character of which i
alcohol, but insolubility in ethe

This saponaceous principle is
as in saffron, rhubarb, &c.: it
there are different species of a

Action of Alcohol. a.) Fou
gested with 16 ounces of alcoh
plete, and there only remained
neous matter which was contai

b.) The alcoholic liquor was of a deep yellowish red. When mixed with its weight of water, it was introduced into a retort, and the alcohol was distilled from it.

After cooling, the liquor was not turbid: it was then evaporated to dryness, and the dry mass being redissolved in boiling water, precipitated, after cooling, resin, which when dry weighed an ounce. This experiment in other respects only confirmed the proportion of resin found after the treatment with water.

Experiments on Hepatic Aloes.

Action of Water.—Sixteen ounces of hepatic aloes were subjected to the same experiments with socotrine aloes. The aqueous solution left, upon cooling, three ounces of resin, the water having dissolved 13 ounces of matter.

The solution was also acid, and blackened the muriate of iron at the maximum; it was slightly disturbed by the nitrates of silver and of lead.

When evaporated to dryness, there remained a mass very soluble in hot and cold water, without affording any resinous sediment.

Alcohol dissolves it also, but ether has no action on it.

b.) The three ounces of resinous precipitate being dissolved in alcohol, there remained a residue weighing two ounces insoluble in this menstruum. We shall speak of this presently.

c.) The alcoholic liquor, when evaporated to dryness, left a resinous mass, which had the following properties:

1. Insolubility in warm or cold water.
2. Great solubility in alcohol, in ether, and in a solution of caustic potash.
3. It melted easily at a gentle heat, and was soon carbonized.
4. Great inflammability, burning with a brisk flame.

d.) The two ounces of residue (b), insoluble in alcohol and in ether, were divided into three parts, and treated as follows:

1. Distilled in a retort, there passed into the receiver a fetid oil, with an ammoniacal liquor, and a great quantity of charcoal remained.
2. The concentrated or the diluted acetic acid had no action on it.
3. A boiling solution of caustic potash dissolved the substance entirely. The liquor was not disturbed by an addition of water, but the acids precipitated from it a brown spongy mass, which was somewhat elastic.

This precipitate when colle
yielded an ammoniacal liquor,
that the substance in question
gulated vegetable albumen.

Action of Alcohol.—Four our
solved in alcohol: there remain
ing 4½ drachms, which was all

The alcoholic solution was e
residue was boiled with water.
but upon cooling the resin s
means we obtained three ounc
and 2¼ drachms of resin.

From all the above experime
following consequences:

1. Socotrine aloes are compl
ter. The resinous part is sepa

2. It is also dissolved in alc
sidue.

3. The parts which are sol
bitter principle than those whi
though these last are not entire

4. The hepatic aloes differ f
as they contain an albuminous
resin than the latter.

5. It is not completely disso
coagulated albumen resists it.

6. It is not wholly dissolved
in which we may distinguish it
aloes, even when their physical

XLII. *Analysis of Aloes.*

§ I. Aloes are procured fro
the same name: at Morviedri
furnishes three sorts, which o
the way in which they are pr
the substance in question is ext
densis, which, as well as the for
some writers as a variety of th
is cultivated in the most wretc
a distinct species from the abo
good quality; but the purest a
in bladders from the island of
trance of the Arabian Gulph i

---

* *Annales de Chimie*

tained by cutting transversely the leaves of the *aloë perfoliata socotrina*, placing earthen vessels underneath it to receive the juice, which is thickened in the sun.

The aloes which was made the subject of the examination is of a yellowish red, and semi-transparent: it presents, in its fracture, several yellow points which glister on a red ground: reduced to powder it is a fine yellow colour: it has a very bitter taste, and a smell which some persons think is not disagreeable: it does not become electrical on friction.

When exposed to a heat of 80°+0 of Reaumur, it begins to soften, and then melts: on account of its being easy of fusion, it is much easier to pulverize it in winter than in summer. If we present a piece of it to the flame of a candle, it melts with a crackling noise, and inflames.

§ II. 50 grammes of aloes were distilled at a heat very gentle at first, and incapable of decomposing it, when the following products were obtained: 1st. Eight grammes of water charged with an essential oil which gives aloes their smell. 2d. At a greater heat there passed over 8·7 grammes of almost colourless water, in which I found one gramme of acetic acid, but no ammonia, on adding quicklime in powder to the liquor. 3d. Five grammes of a heavy red oil soluble in alcohol. 4th. A great quantity of oleaginous hydrogen gas and carbonic acid. 5th. There remained in the retort (which had begun to melt) twenty grammes of a hard charcoal very voluminous and honeycombed, which retained a great quantity of hydrogen, which we saw burnt by exposing it a long time in a crucible at a strong heat in order to incinerate it, which was impossible: it preserved all its blackness, its shining appearance, and a great hardness: it had lost however 12·5, which I attribute in a great measure to the hydrogen. The 7·5 grammes which remained did not contain any potash. This charcoal was treated with muriatic acid: the filtered liquor was precipitated by ammonia, which separated oxide of iron and a small quantity of phosphate of lime: the carbonate of potash precipitated some decigrammes of carbonate of lime.

If we heat nitric acid on this charcoal, we obtain a small quantity of tanning matter which precipitates strong glue.

§ III. Aloes in powder, bruised in a glass mortar with cold water, yielded a mass which, squeezed through the hands, was tacky like turpentine. We succeeded in obtaining a complete solution by adding water in successive quantities, but it required a great quantity; the last portion which remained to dissolve was similar to the first in point of

bitterness and its other prop
frothy on being shaken.

One hundred and forty-eigh
of Reaumur were sufficient en
of aloes, with the exception o
pure ligneous matter: the liqu
and deposited part of the mat
of aloes in water increases in s
of heat, that we may obtain a
ceases to deposit any sediment

When tried by the re-age
water presented the following

1. It reddened turnsole tin
ner.

2. The alkalis and lime wa
without precipitating any thi

3. The sulphate of iron pr
precipitate of the same colou

4. The decoction of gall
precipitate. The supernatant
weaker in colour.

5. The subacetate of lead
this liquor. The supernata
lourless.

6. The nitrate of copper a
produce slight sediments in i
me to be true chemical co
muriate of soda and of the o
as much. These saline matt
of aloes in the same mann
water, by weakening the ac
very soluble matter which is

The above solution of alo
colour, was put into three b
pint, was entirely filled with
which was of the same cap
open: the third, being a m
filled. In two months and a
were observed: The liquor
its colour without alteration;
dark red, and was discolour
acid, which produced a fla
quantity of mucus was for
these two last bottles had ac
would seem, in fact, that the
logous to gelatine; for the d

it a precipitate very abundant in comparison of that which is produced in the recent solution of aloes.

These facts, in my opinion, amply prove that aloes does not constitute a species of the resins.

§ IV. Alcohol at 38° entirely dissolves aloes very speedily, particularly if heat be employed; which announces the absence of gummy or extractive matter in this substance. The filtered liquor was of such a deep red colour that its transparency could scarcely be perceived: water produces an abundant sediment in it of a pale yellow colour, owing to this liquid which is retained in it, for it resumes its primitive brown colour on desiccation.

If we evaporate the alcoholic solution of aloes, we remark that the least motion, the slightest breathing on the liquid, produces a kind of crystallization in it, which disappears and then is reproduced. Although alcohol dissolves this substance very well, this is not the case with the fixed and volatile oils. I exposed to heat a mixture of oil of olives and aloes, and this last substance remained in a melted state at the bottom: the essence of turpentine, which I boiled with the aloes, acted nearly in the same manner: the volatile oil nevertheless assumed a slight amber colour.

§ V. Alkaline solutions dissolve aloes cold and with much facility: combinations are formed in which the bitterness seems in some measure marked. Acids produce in these solutions abundant precipitates which are coloured on desiccation. The volatile alkali diluted in water, also dissolves aloes perfectly: after having filtered the liquor, it was of a deep red colour: and it was evaporated slowly, to drive off the excess of ammonia. In proportion as this liquor was thickened the surface exhibited a continual motion, which seemed to indicate a tendency to crystallization; for we remarked other needles which successively appeared and disappeared. On continuing the evaporation almost to dryness, we obtained crystals in needles attached to a resinous-like mass: on heating this matter with a certain quantity of lime and water, a very evident extrication of ammonia takes place.

§ VI. The weak acids have not a very remarkable action upon aloes: nevertheless they dissolve it better than water, which whitens the solution of aloes in distilled vinegar. The mineral acids act much more energetically upon it. Nitric acid dissolves it very well when cold, and there results a deep red liquor, from which water throws down an abundant precipitate.

Ten grammes of aloes were treated in a retort with eighty

grammes of nitric acid at 36°,
fire with caution. There was
tion of abundant red vapours.
retort was removed from the
contained was of a deep yellow
cooling a great quantity of a fla
quor, when evaporated to the
luted in water and filtered.
yellow substance, which, after
formed one fourth of the aloes
I thought at first that this mat
which had escaped the action
lowing properties soon convi
with some analogy to the yell
matter which Messrs. Fourcro
the action of the nitric aci
which differs from it in severa

The yellow aloetic acid, whe
a very fine yellow colour, and
crystallize, reddens blue tu
with the alkaline carbonates.

It has an agreeable aromati
gently heated. It melts like
vapour mixed with bitterness,
residue.

When distilled at a gentle
products of vegetable substan
producing at the same time a
dant charcoal remained, form
stance employed.

This acid is not very solubl
hectogrammes and a half of t
dissolve entirely two decigran
of the fine red colour of arter
produced in it a precipitate o
the sulphate of iron heightens

Fifteen grammes of alcoho
decigramme of this yellow a
very deep red colour.

The mineral acids, warm, di
out extricating any thing fr
afterwards on account of its

Potash forms with it a co
lizing, and of a deep-red.
violence of gunpowder, eit
heat, or by touching it with

its combustion a slight charry trace, and a remarkable smell of prussic acid, which might lead us to suspect the presence of azote.

We may easily produce this red detonating substance, by pouring on the yellow acid of aloes a slight warm solution of caustic potash, which has but a weak dissolving action upon it.

The nitric liquor, from which the yellow aloetic acid has been separated, was saturated by potash. A very small quantity of red detonating matter was deposited at the end of four-and-twenty hours. Nitrate of lime, which was poured upon it, produced an abundant precipitate of oxalate of lime: when well washed and dried it weighed 3⅛ grammes. The liquor separated from the oxalate of lime was precipitated by the nitrate of lead. The sediment, when treated with one third of its weight of weak sulphuric acid, furnished about one gramme of malic acid, partly dried.

§ VII. It results from the above facts, that aloes is not a gum resin as has been thought, since we do not find in it either the one or the other of these associated principles: nor can we class aloes among the resins, although it resembles them much more than the gums. It is therefore a principle *sui generis*, which I propose, from its properties, to call *resino-amer*. This principle is probably widely diffused, and has its species like other vegetable substances. It is this which had been at first confounded with the resins, which have been sometimes taken for oxygenated extractive matter, and which M. Vauquelin has amply described in his interesting memoir upon different species of quinquina. It is also the same substance which is deposited more or less abundantly from the decoctions of many of the bitter plants, in which febrifuge virtues have been for a long time recognised; such as the *artemisia absynthium*, the *centauria calcitrapa* and *benedicta*, chicory and fumitory *.

It is true that the virtues of these plants have been reckoned less efficacious than the astringent febrifuges: and I am persuaded that in kina, the principle which acts specifically against the fever, and the periodical return of diseases, is owing to the combination of the resino-amer with tannin, or some similar substance. My colleague, Dr. Haldats, directed by these views, is about to enter upon some important experiments, of which he will give an account,

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* It appears to me that the resiniform matter found in the bile by M. Thenard greatly resembles the *resino-amer* of aloes.

and which may perhaps lead to
coveries.

We know that aloes taken i
tonic, and are powerfully antise
Surgeons daily use aloes in tin
ulcers, caries, and gangrenes, w
it have this antiseptic propert
know it besides for its febrifu
but it has certainly never been
to purge the instant it is unite
have had occasion to verify.

XLIII. *A Fatal Case of Ingu* TON, *Esq. Surgeon to the ries, and to the City Truss Ruptured Poor.*

To Mr.

SIR, SHOULD the followin
attended with some importa
worthy of a place in your valu
of it will give me pleasure.

Mr. J. H. æt. 53, an able-b
tution, has always lived a v
good health, has been subject
many years; for which compla
which prevented him from su
ence.

On the 5th of August the
abdominal ring, and formed
in the left side of the scrotum
and painful on pressure, but
considerable difficulty by a su
tient.

The abdomen continued pa
ing referred principally to the
stomach, with a sensation of
warm bath were employed wi
remained in a constipated stat
either by medicines taken b
glysters, several of which wer

The hiccough became ve
taken by the mouth was reje
matter was vomited in larg

much furred; the pulse irregular, frequent, and intermitted. There was also great thirst and fever.

The countenance became livid; the eye had that peculiar stare which often precedes death from strangulated hernia; the extremities became cold; the skin generally cold and clammy, in a partial state of cold perspiration.

These symptoms ended in death in 15 days from the first attack; nor does it appear (although the symptoms of strangulated hernia continued from the beginning of the disease) that any attention was ever directed to the hernia, beyond that of pressing the protruded viscera within the external abdominal ring on the first day of the disease.

The medicines were cathartics, opiates, saline draughts, and glysters. Fomentations to the abdomen and the warm bath were also used.

These particulars were related to me by two of the professional gentlemen who attended him, as I did not visit the patient during life, but only attended to examine the parts after death, when the following appearances were noted.

The whole of the thoracic viscera were healthy. The gall bladder was distended with bile, and contained several small biliary calculi.

The liver, spleen, pancreas, and omentum were healthy; the stomach, duodenum, jejunum, and ilium were much distended with flatus. The jejunum and ilium inflamed: the inflammation increased as the intestines were turned downwards to the left abdominal ring, through which a convolution of the ilium had protruded about twelve inches before its termination in the cœcum. The protrusion formed a tumour about as large as a middle-sized apple, and situated on the anterior part of the spermatic process, between the peritoneum and abdominal muscles, so as not to form any tumour visible on the external part of the body; but there cannot be a doubt but it might have been discovered during the life of the patient by pressure.

The stricture was produced by the peritoneum only.

There were not any adhesions between the hernial sac and intestine, nor had the sac suffered from chronic inflammation. The portion of intestine contained in the sac was highly inflamed, but not in a state of gangrene. The intestines below the stricture were empty and much contracted; the inflammation extended along the intestine only about four inches below the part where the stricture was situated.

The rest of the abdominal viscera were perfectly healthy.

The appearances, on dissection, of this case show, that

if

if an operation had been perfor
son to suppose that the life
been preserved.

It also proves the necessity
part of the abdomen usually t
symptoms of that disease exist.

I cannot too earnestly recom
of an operation in strangulate
usual means of reduction *.
circumstance alone, many valua
community, and their families
dows and orphans become a b
for their support only on paroc

Sept. 26, 1810.

XLIV. *Proceedings o*

ROYAL ACADEMY O

This academy has proposed t
for 1810 :—*In Mathematics.*
and figure of a cylinder, such a
jected at a certain elevation or
is continually impelled by the
The substance which feeds th
and the weight of the body
case, 1. What is the curve de
the inflammable matter contai
such a manner that the inflam
to each other, nor perpendicu
turbations will the rocket be
prevented or corrected ? 3. A
linder be perforated and hollow
a greater surface, and to increa
issues from it, it is required to
most advantageous for the exc
that attention be paid, if possib
sure of the air; but yet the p
best answer to the above three

In Natural Philosophy.— I
stowed great pains on seeking
that subsists between electricit
hibit phænomena so similar an
servations and discoveries have

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* Few, if any, would be the fatal cas
performance of the operation were suffi
tions on Surgery.

secuting these researches. The older philosophers have left us numerous experiments on this subject, which do not exactly correspond with the principles of the experimental philosophy of the present day. Some philosophers have made new and important experiments, which have not been sufficiently examined or repeated. The Royal Society, thinking that this part of experimental philosophy may be considerably improved, offers a prize to the writer, who, taking experience for his guide and support, shall give the best exposition of the mutual connexion between electricity and magnetism.

In Philosophy.—1. There are persons who still deny the utility of physical doctrines and experiments in explaining the phænomena of the mind and soul: others, on the contrary, contemptuously reject psychological observations and reasons, in researches which relate to the body, or restrict the application of them to certain diseases. It would be useful to discuss these two opinions, to show and establish more clearly how far psychology and natural philosophy may be combined; and to demonstrate, by historical evidence, what each of these sciences has hitherto contributed to the advancement of the other. 2. The idea of an universal and characteristic language, proposed by Leibnitz, having never been sufficiently explained by himself, and appearing to have not been understood by any person, the question is, to give an accurate and luminous designation of that language, to point out the way that is capable of leading to this desirable object, and at the same time to examine how far the methods hitherto tried in certain sciences, for instance, in mathematics and chemistry, might be correctly applied to philosophy and the other branches of human knowledge. For the best answer to each of these questions the academy offers a gold medal of the value of fifty Danish ducats. Answers to all, except the last, the term of which is extended to 1811, must be sent before the conclusion of 1810, either in Latin, French, English, German, Swedish, or Danish, to M. Buyge, professor of astronomy at Copenhagen.

WERNERIAN NATURAL HISTORY SOCIETY.

At the meeting of this Society, on Saturday 21st of July last, Mr. Campbell of Carbrook read some observations on the cause of the antilunar or inferior tide, impressing the Newtonian theory on that subject; and Dr. Thomas Thomson read an account of two natural combinations of hydrogen and carbon, viz. carburetted hydrogen and supercarburetted hydrogen, neither of them containing any oxygen.

XLV. *In-*

XLV. *Intelligence and*

To Mr.

SIR, AN extraordinary accid
neighbour, Mr. Watts, chemist
excited the attention of several
am anxious your ingenious read
ticulars respecting it; and if yo
ing briefly an account of the af
it might in future prevent a mo

Mr. Watts had taken into h
boy of aquafortis, and from son
lowing morning, his warehou
there being a great quantity o
many parts of the building.
the carboy was on fire, and mo
saw the remains of the basket
yard. The air quickly revive
doubt but I could very easily h
Particular inquiry was made re
appears to have been perfectly c
no turpentine, or other inflam
of the spot where the carboy s
surprised all who have seen it,
such materials without the ai
some of your scientific corres
for this strange event, which d
rally known, and may point ou
more serious conflagration. I

Your o

Lancaster Court, Strand,
11 September, 1810.

The French Government ha
superb remains of Roman arc
cleared from the rubbish with
several centuries confounded.
which disfigured these monume
sequently been removed, and t
of the original architecture ha
repaired.

BETHLEM HC

Application was made to Parl
for an Act to enable the Gover

exchange, with the City of London, the present contracted site of the hospital, for a piece of ground, containing nearly twelve acres, situated in Saint George's Fields; on which spot the unhappy subjects of mental derangement will, in addition to their former advantages, possess such superior requisites of air and exercise as they have never yet enjoyed, which are not only likely to add in a considerable degree to their comfort, but also to accelerate their cure. The plan of the ancient structure is very capable of improvement, and has long indeed required it. The Governors therefore have advertised for plans for the new building, and offered premiums of £.200 for the best, £.100 for the second, and £.50 for the third best designs, in the full confidence of being adequately assisted in their anxious desires to erect an hospital which may be at once a monument of a benevolent and enlightened age, and an honour to a great and distinguished nation.—The present intention of the Governors is to erect a building capable of containing four hundred patients, but not to confine themselves even to that enlarged number, if they shall be enabled, by the liberality of the public, to proceed further in their design.—The funds of the hospital, which are applicable to the purposes of a new building, amount, however, at this time, to little more than £.27,000, while the cost of a new hospital, upon the scale proposed, can hardly be estimated at a smaller sum than £.100,000.—To effect, therefore, so desirable a purpose as that in view, it will be obvious, that nothing short of a liberal subscription on the part of the public at large can suffice. The Governors have therefore published an address, most earnestly entreating all corporate bodies, as well as individuals, throughout the kingdom, to contribute, by their benevolence, more extensive means of relief and cure, than have ever yet been afforded, to the unfortunate subjects of the most afflicting malady with which it has pleased the Almighty in his wisdom to visit his creatures. Their appeal we are confident will not be in vain, in a country whose greatest characteristic is its noble and generous solicitude to alleviate the miseries, administer to the necessities, and heal the diseases of its people.

Subscriptions are received by Richard Clarke, Esq. Chamberlain of London, (the Treasurer of Bethlem Hospital), Bridge Street, Black-friars; and by most of the banking-houses in London.

MATHEM

It is well known to mathem
solid angles was left in a very i
has scarcely at all been advance
one of the latest commentators
having remarked that " we h
" *even in the simplest cases*, th
" bears to another." Dr. Gr
Academy, has recently invent
which is at once simple, satis
application. By means of this
nitudes of solid angles may be
they are of the same class,—as t
of three planes, those by the n
angles at the vertices of cone
class may be compared with th
to magnitude; and their mutu
processes as obvious and eleme
in Plane Trigonometry. He
solid angles of the regular tetr
dron, and of the right-angled
numbers 87·73611, 216·3518
spectively; the maximum limi
pressed by 1000.

Having been favoured with a
trait of Buchanan, by Titian,
engraved by Woolnoth in his
embellishments of the present N
as wish to possess proofs (of wh
off) of this admirable likeness
Publishers of the Magazine, at

Notice respecting the Preface
Encyclopædia

In writing the preface to th
some mistakes having occurred,
gaged in the publication, the c
leave to assure their subscribers
are wholly unintentional; as it
to detract, in any way, from
whom they employed. They
from Dr. Kirby, that the artic
mistake to another gentleman,
that the following articles,

Geology, *Materia Medica*, *Prescriptions*, *Russia*, *Amusements of Science*, and *Spain*, were also contributed by him.

This notice is to be printed separately, and may be had by the subscribers to the Encyclopædia, from the Publishers of that work in London and Edinburgh.

Rupture is so general a disease, and in its aggravated state so frequently and suddenly fatal, that every information which promises relief, particularly from the regular practitioner, ought to be universally known. We therefore give the following extract from a work lately published by Mr. Edward Geoghegan, in which an improvement in the treatment is suggested.—"I place the patient in a recumbent position, with his shoulders a little raised to relax the trunk, but the pelvis not raised, as that would put the fasciæ on the stretch. The knees are to be drawn up. If the parts have not been irritated by handling them, or the body disturbed by jolting it about, or by any such roughness, I proceed directly to apply cloths wet with cold water, expose the entire body naked to the air, the doors and windows being open. This practice usually succeeds within an hour[*]. If it does not, I surround the hernia with my hand or hands at about its middle, in the way that I would grasp a gum elastic bottle, to press out its air or other contents, by gently approximating its sides, always holding in view, that the tumour is to be *emptied*, and not *pushed up*. I never press the hernia in any direction, or at all towards the abdomen. When it is small, it may be done with the finger and thumb of one hand. Having applied the hands, I do not remove them for fifteen or twenty minutes, aware that reiterated impulses irritate, and that the effects of compression are lost each time that it is intermitted."—In cases of great pain and tension he omits this practice.—This practitioner differs from every other so far, as that the usual directions are to press the protruded bowel up towards the belly, which he takes great pains to show is improper, and insists that the contents should be merely squeezed out.

LECTURES.

Dr. Adams's Lectures on the Institutes and Practice of Medicine will commence on Monday the 8th Oct. next, at Eight o'clock, at Dr. Anderson's Lecture-rooms, 47, Frith-street, Soho.

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[*] In some cases where I could not immediately attend, I have directed that cold applications should be used until my arrival; and after an hour, they informed me that they were seized with a shivering, that they heard the wind rush out of the hernia, and that they were instantly relieved.

On the same day Dr. Ander
of Lectures on Practical Chem
Lectures on Materia Medica
Courses.

Further particulars may be k
Anderson, as above, or to Dr
street.

Dr. Clutterbuck will begir
Lectures on the Theory and F
Medica, and Chemistry, &c. o
at Ten o'clock in the mornin
No. 1, Crescent, New Bridge-
ticulars may be had; or at th
dersgate-street. The Lectures a
Practice, Mondays, Wednesda
Medica and Chemistry, on Tue
turdays, at the same hour.

George-street, Hanover-square,
On Saturday, Oct. 6, a Cou
and Chemistry will recommenc
usual morning hours: viz. T
Practice of Physic at Half afte
a Quarter after Nine, by Geo
Senior Physician to St. George'
of Physicians, &c.

Clinical Lectures are given, a
St. George's Hospital, every S
o'clock.

LIST OF PATENTS FOR

To Charles Williams, of Grav
wright, for a machine for grindir
beans, and any other kind of gra
cles.—Aug. 2, 1810.

To Marc Isambard Brumel, o
chinery for the purpose of maki
and boots.—Aug. 2.

To Thomas Collins, London,
proved mode of making ladder
different pieces, and capable of be
joints, will be found extremely
escalade, engineering, escapes fr
ings, and for all other purposes
description are necessary.—Aug

To William Whitmore, of Dudmarton, Salop, esq. for a magnetic toy to facilitate the teaching of children to spell, read and cypher, in any tongue, with ease to the teacher, pleasure to the children, and proportional expedition.—Aug. 14.

To Peter Durand, of Hoxton Square, merchant, in consequence of a communication made to him by a certain foreigner residing abroad, for a method for preserving animal food, vegetable food, and other perishable articles, a long time from perishing or becoming useless.—Aug. 25.

To James Walker, of Wapping, in the county of Middlesex, ship-chandler, for his machine or vessel for the safe conveyance of gunpowder, and for its preservation from injury by damp.—Sept. 7.

To James Weldon, of the county and city of Litchfield, engineer, for his further new improvements on a mill for grinding bark and other articles.—Sept. 7.

To Joseph C. Dyer, of Boston, State of Massachusetts, one of the United States, now residing in the city of Westminster, merchant, who, in consequence of a communication made to him by a certain foreigner residing abroad, is become possessed of a machine for cutting or removing all the various kinds of furs which are used in hat-making from the skins or pelts, and for cutting the said skins or pelts into strips or small pieces.—Sept. 7.

To David Mathews, of Rotherhithe, engineer, for his improved method of constructing and building locks with a groin or Gothic conic arch. Also an improved form of the gates, and an improved method of opening and shutting the same.—Sept. 7.

To Joseph Johnson, of the county of Surry, gentleman, for his new mode of communicating intelligence from one apartment of a house to another by means of machinery or apparatus, which he denominates a domestic telegraph.—Sept. 17.

To Jonathan Varty, of Liverpool, coach-maker, for his improvements in the axle-trees of carriages.—Sept. 17.

To Peter Brown, of Henrietta-Street, Covent-Garden, Middlesex, gentleman, for his new construction of buoys for ships or vessels, and for mooring-chains or similar purposes.—Sept. 26.

To Richard Seaton, of Berwick-Street, Middlesex, liquor-merchant; and Thomas Rice, of Whitecross-Street, Middlesex, spring roasting-jack-maker, for their new burner upon an improved construction, applicable to all kinds of lamps.—Sept. 26.

METEOROLOGICAL TABLE,

BY MR. CAREY, OF THE STRAND,

For September 1810.

Days of Month.	Thermometer.			Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
	8 o'Clock, Morning.	Noon.	11 o'Clock, Night.			
August 27	58	74°	62°	30·08	63	Fair
28	57	74	64	·13	74	Fair
29	58	71	67	·10	65	Fair
30	59	75	68	29·98	52	Fair
31	66	77	69	·90	53	Fair
Sept. 1	68	78	70	·85	61	Fair
2	70	80	69	·90	60	Fair
3	69	72	58	·80	58	Cloudy
4	58	62	54	·72	26	Cloudy
5	55	68	58	30·00	55	Fair
6	56	68	51	·00	45	Cloudy
7	50	64	49	·32	51	Fair
8	49	64	50	·12	55	Fair
9	51	68	56	·05	50	Fair
10	53	68	54	29·91	41	Fair
11	50	59	50	·70	0	Rain
12	58	58	45	·61	0	Rain
13	48	63	47	30·06	36	Fair
14	52	68	48	·20	42	Fair
15	47	61	51	·38	33	Cloudy
16	53	64	57	·28	42	Cloudy
17	57	67	58	·09	38	Fair
18	58	68	49	·05	30	Cloud
19	51	67	56	·10	22	Cloudy
20	56	63	59	·10	10	Foggy
21	58	66	58	·05	15	Foggy
22	56	68	56	29·95	32	Fair
23	57	62	52	·96	20	Showery
24	56	66	54	30·09	42	Fair
25	58	69	56	·11	80	Fair
26	57	67	55	·05	82	Fair

N.B. The Barometer's height is taken at one o'clock.

XLVI. *On the New Mountain Barometer. By Sir* HENRY C. ENGLEFIELD, *Bart. F.R.S. and F.S.A.*

To Mr. Tilloch.

SIR, THE experience of three years having ascertained the convenience and utility of the mountain barometers, made on the principles of which a description, drawn up by me, was inserted in your Journal, (vol. xxx. p. 46,) I am induced to address you again on the subject; both to inform the public of some improvements made in their construction since my former letter, and to propose some mode of collecting, for general benefit, the observations made by individuals.

The improvement in the construction is principally in the cistern. It had been found that when exposed to great motion in an unfavourable position, which in long journeys is not easily avoided, the agitation of the mercury had several times cracked the tube towards the top, in a fissure scarcely perceptible to the eye, yet sufficient to let in slowly a small portion of air. To remedy this inconvenience, the cistern has now a bottom of leather on which a screw presses in the usual mode, so as to force the mercury nearly to the top of the tube when packed for carriage. This screw is to be unscrewed as far as it can, when the barometer is prepared for use; and the leather bag is so adjusted, that there is no reason to fear that the capacity of the cistern thus unscrewed for use, will ever be sensibly different from itself at different times. It may be just mentioned, that when the barometer is carried by a careful person, it is by no means necessary to screw up the bag between every station; as, when unscrewed, the instrument is in precisely the same state that it always was, in those of the first construction*.

Mr. Jones, at the desire of several gentlemen, has endeavoured to add a gauge point and adjustment to keep the mercury in the cistern ever to the same height, as in other mountain barometers, but such addition has been found in practice productive of more inconvenience than advantage. He now, therefore, measures the content of every tube separately, and engraves on the mounting the correction to be made to the results, as stated in the former paper; and by this method it is presumed that all errors from the want of a gauge point must be prevented. Mr. Jones has

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* The screw which frees the cistern for use, is protected by an outer cap from being spoiled by idle curiosity, or injured by a blow, which often happened to those barometers where this screw was unprotected.

now sold above 150 baromet
these, it cannot be doubted th
been purchased by gentlemen
use them for the purpose of m
that a great number of valu
made with them in different
however, these observations
observers, the public is littl
doubt not that if it were g
for them was provided, all th
observations of altitudes wi
mountain barometers, would
and contribute their part to t
information which would be
either of the observations ther

For this purpose Mr. Jone
of Kenton-street, Brunswick
artist who made these barom
tion, has kindly consented, a
arrange all such observations
(post paid or franked) by th
them; and I shall be happy
puting them, but will readil
of them, either in the literary j
as may in process of time ap
the most desirable that the
be published with their obse
of authenticity to them: th
omitted, as the several contr

As it is to be hoped that
numerous, it will materially
rangement, if a general form
servations; and it is hoped t
will be found convenient to
well as to those whose provin

Extensive geological obse
out of their line; yet I ma
of the additional labour or
where each observation wa
specified. A[illegible] observa
of the barometer and therm
te[illegible] [illegible] [illegible] [illegible]rs at
[illegible] W[illegible] [illegible] 50 i
[illegible] [illegible] tha
s[illegible] [illegible] [illegible] s
[illegible] [illegible] the heat ar

It will, however, be best to make observations as often as possible both on the one and the other, as it has been ascertained, both by Mr. Cavendish and the late Dr. Hunter, that the temperature of the waters at any given place is a most accurate measure of its mean heat; a determination of which is not only an object of considerable curiosity in itself, but of very great consequence in an agricultural point of view.

The annexed form for registering the observations scarcely requires an explanation. The first column is for numbering the observations, which extremely facilitates the reference to them. The succeeding columns are fully explained by their titles. The last, called Results, is added, in order that those persons who choose it may place in one view the observations, and the altitudes deduced from them. Printed sheets in this form, ready for use, may be had of Mr. Jones. The back of each page is left blank, for the convenience of inserting any other notes or observations.

I am, sir,
Your humble servant,
H. C. ENGLEFIELD.

N°	Place of Observation.	Weather.	Wind.	Time	Barometer.	Th. A	Th. D	Results.
1	October 6 Steyne, Brighthelmston . . .	Sun	N W	2·15	30·268	63	63	1 and 2
2	Stand on Raceground.	Do.	Do.	3·0	29·870	62	62	400 feet.
3	Stand again	Do.	Do.	3·32	29·861	62	61	
4	Steyne again . . .	Do.	Do.	4·15	30·278	61	61	

XLVII. *On the Land Winds of Coromandel, and their Causes.* By WILLIAM ROXBURGH, *M.D.*[*]

THE land winds on the coast of Coromandel are those hot winds which blow at a particular season of the year, and hour of the day, from the western hills, commonly called the Ghauts, towards the Bay of Bengal. In the more inland countries, as above the Ghauts, they are not

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[*] From *Transactions of the Medical Society of London*, vol. i. part I. just published.

confined to any regularity, tl
with a great degree of severit

I understand also that in t
are sometimes experienced ver
the west or the northward, c
have not been able to ascerta
to prove the insufficiency o
signify little, although in c
more moment.

As they are generally su
country, and are felt during
should imagine their histor
fectly investigated and unde
neither the one nor the other
explained.

The most plausible reaso
accumulation of heat in the
which they prevail, and th
which they have to pass. T
true cause, it shall be my
which I will add an attempt
one, founded on known che

Respecting the theory I ha
found but few patrons in th
flatter myself may be ascribe
it has been proposed, than t
constructed.

In order to facilitate the
as well as to show that the l
attention from the philosop
phænomena accompanying
as well as the effects by whic

Could my pen equal my
paint their effects in the mos
years experience in a countr
for their intensity.

The land winds are prece
or in the beginning of April
eleven and twelve o'clock a
tions, mostly from west to e
called by the natives Peshash
times do a little mischief to

About the same time, o
of the whirlwinds, we may

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* Samulcotah in t

nished as it were with clouds, which become daily darker and heavier, until they discharge themselves with much thunder and lightning in a heavy shower of rain. After this marked phænomenon the land winds set in immediately with all the violence of which they are capable.

Their commencement is generally in the latter end of April, or beginning of May, and their reign lasts to the earlier days of June, during which period they generally exert their violence from ten or eleven o'clock in the morning until about three or four o'clock in the afternoon.

In this season the atmosphere is commonly hazy and thick, except that in the evenings and nights the sky is serene and clear, provided the land winds do not continue the whole day.

The rising sun which portends a land wind day appears of a fiery red, and as if involved in mist, which mist is changed afterwards into clouds that lie heavy on the Ghauts.

The land wind of each day is almost always preceded by a long calm, and immediately by a cloud of dust.

Their diurnal violence is terminated along the coast about two or three o'clock, by the setting in of the sea-breeze, which wafts delight and health as far as its influence extends, which is not more than ten or twelve miles inland. An abatement of their intensity from thence to the Ghauts is all that can be hoped for.

The sea-breeze regularly begins in the afternoon at one or two o'clock, blowing pretty steadily until sunset, when it dies away gradually, and at sunrise it is again perceptible, though weakly.

When I say its influence is only felt ten miles inland, I do not wish to be understood that it does not extend further: I mean only its powerful refreshing properties, which it loses in proportion to the distance from the sea, and in an inverse ratio to its strength, which is not great. In general it arrives at thirty miles distance from the sea in the evening, and is then only agreeable by the ventilation it effectuates.

In the country above the Ghauts, as in Mysore, the east wind prevails also in the afternoon, but from a period much earlier, or cotemporaneous with the sea-breeze on the coast, which renders it clear that this inland breeze either does not extend further than to the Ghauts, or really originates there; a point which deserves to be ascertained, as another phænomenon depends upon this circumstance.

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Should the sea-breeze fai
land wind decreases graduall
ginning of the night, which,
dismal to a degree: next mo
is again perceptible, but at
as strong and hot as the day b
hands upon is then distress-
be the case when the temper
that of the atmosphere. T
fortnight in the year 1799 i
the thermometer at eight
108°, and at noon at 112°.
very often crack and break
niture warps and shrinks s
out of doors and tables, &
however, I have never seen
than 115°, viz. in the coole
some say they have observed

The Ghauts, and the hills
are then seen lighted all n
often in a very picturesque
These illuminations appea
of the mountains, and seld
or bottom of them. They
hills on which the bamboo
probably led the natives to
rationally, by ascribing it
against each other.

Lieutenant Kater, of his
that the corky bark of the
spontaneously inflamed, as I
surveys, its bark converted
these trees burnt down to
not in the vicinity of any o

In Europe I know these
much discredited; and I d
sheets ever be published,
against what I have relate
state facts only, which a lu
painted in more striking c
stricter adherence to truth.

The land winds are not
generally produce on the fa
that of the animal creation.
felt in the eyelids, which

stiff and painful. This is owing to the immediate volatilization of all humids that irrigate our organs, and which, in this particular one, probably gives rise to inflammations of the eyes, so frequent at this time of the year[*].

The continuance of this wind causes pain in the bones, and a general lassitude, in all that live; and, in some, paralytic or hemiplectic affections. Its sudden approach has, besides, the dreadful effect of destroying men and animals instantaneously.

It is not very uncommon to see large kites or crows, as they fly, drop down dead; and smaller birds I have known to die, or take refuge in houses, in such numbers, that a very numerous family has used nothing else for their daily meals than these victims of the inclemency of the season and their inhospitality. In populous places it is also not very uncommon to hear, that four or five people[†] have died in the streets in the course of a day, in consequence of being taken unprepared. This happens especially at the first setting in of those winds.

The natives use no other means of securing themselves against this wind but shutting up their houses, and bathing in the morning and evening; Europeans cool it through wetted yats[‡] made of straw or grass, sometimes of the roots of the wattie[§], which, wetted, exhale a pleasant but faint smell. It will be incredible to those that have never witnessed it, but the evaporation is really so great, that several people must be kept constantly throwing water upon the tats (eight feet by four) in order to have the desired effect of cooling a small room.

It would be scarcely necessary to observe, if it were not in contradiction to public opinion, that the cold produced is not a peculiar property of the wind, but depends upon the general principle, that all liquids passing into an aëriform state absorb heat, and cause immediately around them

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[*] The eye flies, so often supposed to occasion it, produce a transient and sharp pain in the eye, but never, I believe, a lasting inflammation.

It is generally thought infectious, and may be so by the interference of the eye flies carrying the contagious matter from an affected eye to a sound one.

[†] Four people dropped down dead at Yanam, in the year 1797, an hour after my arrival there from Masulipatam: and at Samulcotah four or five died the same day on the short road between that place and Peddapore: the number of inhabitants of either of these places does not exceed, I believe, five thousand.

[‡] The frame of them is made of bamboos in the form of the opening in the house to be tatted, let it be door or window, which is then covered with straw in the manner every one, thinks best suited to retain the water longest.

[§] *Andropogon muricatum.*

a diminution of it, and con
On the same principle depe
and water, in the land win
earthen vessels which allow a
their pores, and the former i
of cloth or in straw, which m
ened.

The great violence of thes
by frequent showers of rain,
and by the greater quantity c
the inland countries, which
formation of clouds along t
clearer, and visible at a great
at any other period of the yea

After the enumeration of
stances, I am naturally led to
that produce them. Before
must prove, according to pr
philosophers is founded in er

They ascribe, as already ob
which distinguishes these wi
absorption of caloric in the
tract of country, at a time w
fully in our latitudes.

According to this theory,
proportion to the space over
it should be hotter on the co
country inland, or, which is
by degrees from the eastern t
sula. Experience, however,
it is hottest near the Ghauts, a
those ranges of hills, than a
the heat of those winds decre
Bay of Bengal, and in a direc
sea: accordingly, it is at Am
and at this place again than a
Madras, where the land wind
gree of severity.

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* A place situated in the most wes
at the foot of the steepest ascent into

† Lies in a spacious valley nearly a
and has the advantage of an open co
the north-east.

‡ A large city, the capital of the na
of hills called the Ghauts.

§ [illegible] miles east of the latter place
lous place. I have chosen this tract
not the hottest; for Ellore, Rajahmun
Circars, are by far more exposed to t

Time is another measure applicable to the acquisition of heat, as it increases to the greatest pitch which a body is capable of receiving in proportion to its continuance: the land winds should therefore be cooler when they set in at ten or eleven o'clock, and hottest at their termination in the afternoon; they should be so at least at noon, when the sun is nearly vertical, and has the greatest influence on the substances from which heat is to be attracted. The contrary, however, comes nearest to the truth; for it is known that these winds set in with their greatest violence and heat at once, which rather abate than increase, as might be expected.

We should, on this principle, further suppose the heat would increase gradually with the return of the sun to our latitudes, from its southern declination, and stand always in proportion to its position. We find, however, that experience also contradicts this point of the theory under discussion; for after the sun has passed our zenith *, the land winds set in at once with all their intensity, in the manner before described, and they cease as abruptly before its return again †.

A material change in the temperature of this climate is certainly effected by the approach of the sun from the south; but the heat which is thus caused, and which increases by imperceptible degrees, is never so great, and is only felt by those who expose themselves to it unprotected; for the air remains proportionally cool, and our houses afford, in this season, a pleasant retreat. We find it far otherwise in a land wind; for this penetrates our inmost recesses, and renders life miserable every where.

I have before observed, that winds equally hot with those of periodical duration are felt in all parts of the country, and at different seasons; a circumstance alone sufficient, if proved, to overthrow the groundwork of the old theory.

For a confirmation of this, I will appeal to the general observation, that immediately before a long rain the weather is sultry, and that a single shower is always preceded by a warm disagreeable wind.

We are very particularly reminded of the approaching great monsoon in October by the oppressive heat we have in the calm evenings of that month, which, I am persuaded, would equal that of the land winds in May, if the atmosphere

---

* The sun is in the zenith at Madras about the 26th of April.

† The sun is again in our zenith on its southern declination about the 19th of August.

sphere was not cooled in the
breezes that have wafted over e

I can refer, secondly, to n
according to which, the 4th of
a place not far from Bengalore,
short time to 104° just before a
at a time when heavy clouds d
sphere.

Further, in the months of I
had often at Bengalore, in the
wind from the eastward, which
land winds, and were really as
derate land winds are in the Ca
tiplied instances of this kind, b
fact so much known it would b

The last refuge of the defe
valleys of the Ghauts, in whic
generated by the concentrated
sun.

I will not deny but the heat
may contribute much to raise t
but the sudden appearance of the
and abrupt disappearance, all m
tion as a principal cause.

The heat of these winds shou
words more on the preceding
from the point where it is great
both sides, as is the case on the
the contrary, we find that, in
ascended the Ghauts, or on the t
the clouds, we have escaped th
hereby remarkable, that the dire
to appearance nearly the same
for example, the wind is, in t
during the greater part of the
from the east, and commonly w

This, together with what ha
hope, be thought sufficient to

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* Major Lambton, at the top of Carn
the Carnatic, about three thousand two h
sea, found, in the middle of the land wind
80° in the mornings, and, at noon, 82° ar
and more.

This observation may be the more depe
for a considerable time on the top of thi
accurate survey in the course of which
well as to all other points that could influ

lative to what *can not* be the cause of the heat in the land winds.

It remains now to point out a theory, supported on a firmer basis, which I shall endeavour to do in the following pages. It is founded on a chemical principle, and will explain, I think, the heat of these winds in a satisfactory manner.

The principle itself needs no demonstration, as it is admitted as a general law; viz. that "all bodies, when they become more dense, suffer heat to escape; or, what is the same, they give out heat." For example, when gases or aëriform substances become vapours, they discharge as much heat as was necessary to keep them in their former gaseous state: further, vapours in condensing into fluids are known to do the same, as also fluids, acquiring solidity.

I am sorry that the quantity of heat set free in the condensation of vapours required for a pound of water has escaped my memory; but I recollect it was very considerable. We know, however, that a great deal of it is required for the evaporation of the same measure, and it is but reasonable to admit that the same quantity with which it has combined should be discharged on its returning to its former state of fluidity.

In order to apply this principle to explain the presence of heat in our land winds, I must first observe, that the atmosphere in January, February, and March, is perfectly clear and serene; and then I will call to mind what has been said of the phænomena of those winds, that they are preceded by clouds on and among the Ghauts, and that a heavy shower of rain from that quarter announces their arrival; that during their continuance clouds are observed to lie on the Ghauts; and that the atmosphere, even in the low country, is hazy and thick. I must add also, that the countries west of the Ghauts are at this season frequently visited by heavy showers of rain, accompanied with much thunder and lightning, and sometimes with hail. Here in the Mysore country I have found the heaviest showers of this kind to come from the north-west*, which is exactly in the direction of the countries remarkable for the great heat of the land winds in this season. At times, we have also showers from the east and south east, and my attention shall not be wanting to ascertain whether it is not at the time when the land winds blow hottest in the Carnatic,

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* The hottest land winds in this season (1804) at Madras were, I understand, from the north-west; which corresponds with the direction from which the rains came in Mysore at that period.

By this we see, that the cloud
charged with water and electricit
to investigate), are drawn to the
which, during the formation of
sarily be discharged, is carried t
parts of the coast, and causes th
land winds are so remarkable.

I have acknowledged already,
by the power of the sun in this
aggregate of it in the wind; but
it acts only as a secondary cause
venting its absorption and dimin
variety of substances, particularl
would combine, if they had not
or incapacitated.

In colder climates, this absorpt
degree, as substances are abund
produced by the formation of ra
come imperceptible*. It is, ho
marked, that the heat of the su
powerful than at any other tin
ascribed to the reflection of the
clouds; but I opine it is often th
mation of water in the clouds,
that moment.

It has been observed, that the
not felt on the top of high hills,
considerable perpendicular heigh
rages most violently; as for exa
Ghauts, which is only about five
the valleys immediately below.
weighty objection against my th
in the light of an elastic fluid, ex
and from whatever cause it procee
to extend even further where it
as from the air in higher region
lighter and more penetrable than

But the reverse takes place;
above the clouds no other heat
might be owing to the nature of

This circumstance may be ac
nished density of the air in the l

---

* Earl Dundonald's Treatise, p. 20. "
gree of heat and cold in the atmosphere ar
nate disengagement and fixation of heat b
the effects of the solar rays."

produced by the heat of the season, which would naturally cause the wind to rush thither, with all its contents, and with greater impetuosity. The coolness of the atmosphere on elevated situations may be ascribed also to the evaporation of the uppermost strata of the clouds, which accompany the land winds.

Many arguments I have dispensed with, which might have been produced to elucidate and to establish my theory, as they were chiefly such as could be collected from simple inference, and from affirmative application of doctrines advanced before.

I will only add, that both the sirocco and samiel may be owing to similar causes as those which appear to be productive of the pernicious, or rather disagreeable, effects of our land winds.

XLVIII. *Hints respecting a New Theory on the Orbits of Comets.* By *Mr.* W. Crane, *of Edinburgh.*

To Mr. Tilloch.

Sir, The following theory, for any thing I know, is original: should it be deemed worthy of a place in the Philosophical Magazine, its insertion will much oblige

Your humble servant,
W. Crane,
Student of Medicine, Edinburgh.

Sept. 27, 1810.

> "Hast thou ne'er seen the comet's flaming flight?
> The illustrious stranger passing, doubles wide
> Heaven's mighty cape, and then revisits Earth."—*Young.*

The difficulties with which this intricate branch of astronomy is surrounded, the short part of an orbit of a comet that is visible to us, and the rarity of their appearance, have given rise to innumerable theories, many of which have no sooner been advanced than they were immediately abandoned as erroneous.

The school of Peripatetics assigned comets no place in our planetary system, they only considered them as sublunary things made up of the exhalations in the terrestrial regions; which was the opinion of many, until Tycho Brahe and Kepler proved by observation that they were beyond the moon, and consequently not composed of terrestrial vapours: this was further confirmed by the observations made by Cassini, of that seen in the year 1665, and of another that appeared in April 1680. Cartesius thought them to be permanent bodies, like the planets, and to be constantly

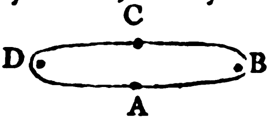
carried from one vortex to a
Cassini supposed from his obser
circles very eccentric, and contai
them; and from hence was led
and 1681 was the same as app
of this and some others he had
he determined that comets mo
tions Antinous, Pegasus, And
Scorpio, and the bow of Sagit
zodiac of comets. That this is
tions have proved. The comet
1808 was first seen in Serpentari
the right shoulder of Hercules, t
the tail of the Swan, which is a
the zodiac laid down by Cassini
System of Comets, published in
satellites moving about a prima
around the sun, at a distance eq
of the Magnus orbis, in four ye
Saturn, who is 258 times neare
tion in about 30 years. This
never see, on account of its s
stance, and these comets or sat
they descend towards us in pe
have a greater body revolving
smaller; which is contrary to w
to the other planets and their sa
sider this as one of those theori
from so great a man, would ha
oblivion?

Sir Isaac Newton, Dr. David
others, imagine them to move
having the centre of the sun in c
in their calculations have substit
having the same vertex and focu
true trajectory, if it never retur
leads us from one difficulty to
ask, By what means did it con
the sun, and from whence? A
from one fixed star to another
which is the theory adopted b
All the celestial phænomena wit
are obedient to certain laws of a
in circles or ellipses, but none i
tioned.

But to return to the former

move in very eccentric ellipses, in one of the foci of which is placed our sun; for, as La Place says in his System of the World, analogy leads us to imagine that comets move in orbits, which, instead of being nearly circular, like those of the other planets, are very eccentric, and the sun extremely near that part in which they are visible to us; and to observe the same law as the other planets.

Hence is it not probable that they revolve about two fixed stars, placed in the two foci of their orbits? This opinion, I think, is strengthened by the amazing eccentricity of their orbits, which, as was observed above, approaches very near to a parabola, no comet has yet been seen that would answer to an hyperbola: of this amazing distance, the exceeding small part we see before a comet approaches the sun, and when it leaves him, would not differ much from a right line. Again, as the two foci of the ellipse in which it moves are so very distant, is it not probable there are two attracting powers? that, is one in each focus; and as the attraction of one body begins at the point where the other ends, let us conceive the comet to be put in motion a little beyond that point, as at A, and by the time it arrived at B, its centrifugal force becomes great enough to throw it within the attraction of the focus D, which we will suppose at C: it is now acted upon by the attractive power at D, and acquires in moving from C to D a velocity great enough to bring it again to A; and thus it will revolve about the two fixed stars B D, in a very eccentric ellipse. This will also account for their appearance from every part of the heavens: and it is supposed that more than 450 have been seen in different directions; for about the same fixed star many may revolve, yet only one about the same two fixed stars.



Ferguson, in his Astronomy, estimates the nearest fixed star at about 32,000,000,000,000 miles distance from the earth, consequently it is 32,000,082,000,000 miles from the sun; and Adams, in his Astronomical Essays, says that the comet seen by Brydone at Palermo in 1770 moved at the rate of 60,000,000 miles an hour. Now admitting this to be its average rate, and that it performed a revolution once in 129 years, which is the period assigned to that which appeared in 1661, we shall have 67,802,400,000,000 miles for the length of its orbit; and it is not improbable that this would be the perimeter of an eccentric ellipse whose foci were the distance above mentioned.

All Nature is held together b
getable kingdom is joined to t
plant; birds and fishes by the b
joins beast to men; and the su
the worlds together that form
our views a little further, and
comet uniting the systems of c
the links of that chain by whic

XLIX. *Description of a M attempting Depredations wi Limbs. By Mr.* Robert S

Sir, I beg leave to submit t
mantrap, which I hope will n
To those who live in the coun
the frequency of petty depreda
orchards, &c. and which are so
persons would like to endang
predator by setting the commo
sumed there are but few who
offender. The instrument wh
mit to the Society is for the pu
the person without injury.
at Woburn last summer, an
milar purpose was produced b
consisted of a wood box, co
barrels, and two chains passing
this was set, the chains were
barrels, and extended to a cer
kept the trap from closing. Th
with thin iron plates; so that
those plates his leg dropped i
closed round it and held the l
three feet square and a foot
should at setting be let into t
work of considerable labour,
difficult to dispose of the stuff
the trap; and as the whole a
expensive, it appeared to me
practice.

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* From *Transactions of the Society*
factures, and Commerce, vol. xxvii.—
voted to Mr. Salmon for this commu
reserved in the Society's repository for

I think it right to give this explanation in justice to Sir Theophilus Biddulph, from whom my idea of the utility of something of the kind arose, as also to show the difference between his invention and the trap I have made, which is so very simple as hardly to require explanation. When set, it only requires that the two keys be withdrawn, and that the trap be covered with a few loose leaves or mould. To the trap I have attached a piece of chain and a screw to be screwed into the ground, so as to prevent its being carried away; but against any person that may be caught such a precaution is perhaps unnecessary, for any person who is caught will find the jaws of the trap close so fast on the leg that he cannot drag the trap far without great pain, and will consequently be glad to stand still and to call out for relief. For the convenience of explanation I have applied mufflers to the jaws of the trap, so that any person may put in his leg without the least inconvenience. I have even tried it without, yet, though void of danger, the sensation is not pleasant. The muffle will of course be omitted when set for use, as it is not then necessary to guard against a little inconvenience, otherwise the springs might be made weaker. I remain, sir,

Your most obedient humble servant,

Woburn, Feb. 12, 1809. ROBERT SALMON.

To C. TAYLOR, M.D. Sec.

P. S.—Permit me strongly to recommend to the notice of the Society the earth screw attached to the trap, as excellent for the purpose of fixing any thing steadily in the earth. This screw is far superior to the common way of driving an iron point or stake therein.

I have employed it for several years in fixing cross-staffs and other surveying instruments with great advantage. The very act of driving a spiked instrument into the earth leaves it loose with some play or movement, which prevents it from being easily secured; but with a screw of this kind at the bottom of the instrument it is firmly fixed in the ground, and a turn of the screw will again fix it, if it should by any means be moved or loosened. It may also be screwed into the ground with any instrument upon it, which would be spoiled by the act of driving it in.

Description of Mr. Salmon's Man-Trap, which detains the Offender, without injuring or maiming him. See Plate VI. Fig. 1.

The principal figure in the fore-ground of Plate VI. is a

perspective view of this mac
of wrought iron, about 18 ir
jecting from it to receive a
which is fastened to an iron
screwed into the earth by tl
is about 14 inches long,
ground, will hold so firmly,
being drawn out, even by tw
small square end, it cannot
handle E; so that an offend
cult to remove the trap: *e e*
on centres in the frame AB
tendency to close together,
fixed in the frame AB, and
from the upright sides of tl
two small iron rods jointed
frame *g*, and passing throu
other frame *f*. These locks
by springs into the teeth, as
so as to prevent the two ba
when they have been clos
The internal mechanism of
2, 3, on a larger scale at L
of the lock is supposed to b
parts, where *k* represents tl
which is cut into teeth, *r*
teeth of the rack, and prev
lock: the click is pressed
a spring, which is plainly
attached to the ends of the
the bar passing through tl
riveted on it is confined i
be got out. But as it is ne
draw the clicks back from
mon has contrived two di
this object. Figure 3. M
del left at the Society's R
is put down through a he
received into a hole lapp
turning the screw it lifts
rack; so that the movin
from each other, till they
iron cross *m* is then put be
S of the two locks are to
the trap is set for use. I
within the square of the

cross *m*; and having thus removed the obstruction, the two frames *e e f g* are closed together by the springs *p p*, so that the bars *f g* inclose his leg, and the clicks in the locks prevent the bars being opened without the screws S. In some of the machines which Mr. Salmon has made since the model was deposited with the Society, the locks are made like figure 2, L, where a common key is to be introduced, and, when turned round, catches the tail of the click; it may have wards to prevent the using of a false key, though no wards are shown in the plate. Part of the screw D for securing the trap from being carried away by depredators, is shown on a larger scale at N, in order that the peculiar form of its threads may be better seen, which fix it firmly in the earth. Such screws would be very serviceable in fastening horses at grass, &c.

L. *An Account of a New Method of increasing the charging Capacity of coated Electrical Jars, discovered by* JOHN WINGFIELD, *Esq. of Shrewsbury*[*]. *Communicated by Mr.* JOHN CUTHBERTSON, *Philosophical Instrument-Maker, Poland Street, Soho; with some Experiments by himself on that Subject.*

IN my treatise entitled *Practical Electricity and Galvanism*, page 103, I have said that breathing into coated electrical jars increased their charging capacity to such an astonishing degree, that their discharge would fuse four times the length of wire more than they could in ordinary circumstances; which I proved by experiments 147 and 156. Since that publication, large electrical batteries are become more general, and the number of jars increased; so that batteries containing thirty, sixty, and even a hundred jars are frequently met with; and, when so numerous, breathing into each jar is very disagreeable; and not only that, but in very dry states of the atmosphere, when most wanted, is even ineffectual, as those jars first breathed into lose that property which was produced in them by breathing, before the last can have obtained it: so that various other means have been tried; such as wetting the inside of the jars, and putting wet sponges into them, or by greasing and oiling the uncoated part in the inside; all of which gave very uncertain results, till John Wingfield, esq. communicated to me,

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[*] A gentleman who has lately very much distinguished himself, not only in the electrical science, but in all other branches of experimental philosophy.

he had discovered, that pastin
outside of the jars above the c
venting the jars from explodin
believed that their charging
thereby.

I embraced the first opport
discovery with single jars.

Experiment I.—I took a ve
used to show the phænomena
out breaking) twelve inches
inches, containing in the who
was applied to the conductor
and six turns of the plate ca
the coating: the state of the
it required eight and twelve t
a third explosion: a fourth co
cleaned and dried as before,
discharge.

Experiment II.—A slip
taken, of sufficient length to
jar when the two ends were
ped on to its outside to about
the uncoated part being rubb
to the machine, eleven turns
tary discharge to the outside

Experiment III.—The pap
to touch the coating, and the
voluntary discharge could b
charged in the common way
increased,—to prove which,

Experiment IV.—The co
ter (which is always fixed to
was used, to try to what di
made to pass from the kno
of the electrometer; which
five-eighths.

Experiment V.—A piece
inch in diameter and one in
electrometer, through which
to pass, and the wire was bl

Experiment VI.—The pa
breathed into twice; the dis
distance of two inches, and

Experiment VII.—The j
dry, and a piece of the sa
length was hung to the ele

as before, and it appeared that the greatest charge it could take had not the least effect upon the wire: thus it appears that a paper ring so applied does not increase the charging capacity of jars in the same degree as breathing.

Experiment VIII.—The jar was highly charged, and examined in the dark: the paper ring appeared luminous all round the uppermost edge.

Experiment IX.—The ring was taken off, and pasted on in the inside close to the coating: 23 turns caused a voluntary explosion through the ring to the outside coating.

Experiment X.—A second ring three quarters of an inch broad was pasted on close to the other: the same number of turns produced a voluntary explosion, and the paper was torn by the discharge, which was repaired and left to dry.

Experiment XI.—When dry, no voluntary explosion could be obtained.

Experiment XII.—Its greatest power was then tried, and was found to be exactly the same as in Experiment VI.(when it was breathed into): it discharged at two inches distance, and the same length of wire was fused into balls.

Experiment XIII.—A second jar was taken of a larger size, being 13 inches high, and its coating seven inches; in the whole it contained about 190 square inches: after being rubbed clean and dry, it was applied to the conductor of the machine: twelve turns of the plate produced a voluntary explosion to the outside coating.

Experiment XIV.—A paper ring was put round the uncoated part on the outside at about 1¼ inch distant from the coating: eleven turns of the plate produced a voluntary explosion to the outside coating: the paper ring was then pushed down to the coating, after which no voluntary explosion to the coating could be obtained; but it discharged itself to the electrometer ball standing at the distance of 2¼ inches from the knob of the conductor.

Experiment XV.—The same sort of wire, two inches long, as used in Experiment VI, was hung to the electrometer, and the discharge made it blue with several bendings,—a proof that it had been nearly red hot.

Experiment XVI.—A ring of common writing-paper one inch broad was pasted on the inside close to the coating, and when dry no voluntary explosion to the coating could be obtained; but it discharged itself to the electrometer ball standing at the distance of 2¼ inches, and the wire was fused into balls.

Experiment XVII.—The paper rings were now taken off, and the uncoated part made clean and dry: 19 turns pro-

inches above the ground; upo
are placed, and bolted togeth
shown at G, are placed betwee
form the ends of the mould; th
the two bolts *a a*, which are s
of the moulds, and are held fa
is now to be rammed in betwee
with an iron head X. When
and well rammed down, the ke
bolts, and the bolts drawn out;
and put together again, a lengt
bolts at the end being put thro
only one of the end boards is n
proceeds as before: in this m
built of any length; and when
then the mould may be taken
again upon that course, the lo
put through the bolt holes wh
the wall at the first operation,
of the wall is in the same plac
When a wall is to be built th
wood must be placed under th
diminish the space between th

When the angle walls of bu
apparatus is put together, as sh
planks are put together to for
end of each of the planks F an
bolts, the other ends have each
as shown separately at *b d*; th
moulds, so as to form a hinge
so as to be perpendicular to e
K, hooked into eyebolts fixed
planks of the mould are joine
ner, see fig. 5, that of one fr
the other, and has two pair o
receive the bolts *t t*, which a
other shorter plank, and the
of the bolts, to secure the pla
P is occasionally placed betw
and the side of the other, to i
planks, to make a thicker wa
the plank being received int
wood, and these bolts are th
of the long plank. In buildi
sary that the vertical joint
should not be over one anot

manner as the joints of brickwork: this is accomplished by making the lower course of wall upon the brickwork only half the length of the mould, which is done by placing the end board G of the mould in the middle of it. The next course over this is to be made the whole length of the mould, the next one only half, and so on, as shown in the figure.

Improved Moulds and Description of making Earth Walls, by Mr. R. SALMON, *of Woburn, Bedfordshire.*

The model of the frame in possession of the Society is made to a scale of an inch to a foot, the frame at large is made of 1¼ inch deal, ploughed and tongued together. The bolts and pins or keys of iron, as are also the plates on the holes in the sides of the frame. These plates are put to prevent the keys from cutting into the wood, and the holes from gulling and wearing.

This sort of mould is calculated for making walls, either fourteen or sixteen inches thick, and the model (or perspective view of it in the distance of Plate VI.) shows how the mould is to be applied for making the corner of a building of the sixteen inch wall; the same moulds may be applied for a fourteen inch wall. *ff* being the outer sides. FH the inner sides. When employed for straight walls, or making good between the corners of buildings, the two returns of the frames are used in pairs, *ff* and FH make two sets of frames. The board marked G must be of width equal to the thickness of walls to be made, and are for the purpose of stopping the earth, and making ends or jaumbs to doors or windows, or wherever wanted. The piece of wood P is two inches thick, and is for the purpose of making out the external sides of the moulds, from a fourteen inch to a sixteen inch wall: by introducing this piece between the two sides *ff*, and putting the fixed iron pins in the outer holes ZZ, and taking away the blocks under the heads of the outer bolts, the sides of the frame will then be sixteen inches, as under, and thereby adapted for a sixteen inch wall. Fig. E are pieces of wood about 1¼ inch square, and cut to the length of the thickness of the wall, and are for gauges to be applied on top of the bolt, to keep the keys from drawing the sides too close together.

In beginning the wall, some of them are necessary at the bottom, the more firmly to support the frame on the brick or stone work. They are then worked into the wall, and, after the frame is taken down, drove out. After the first course, they are only necessary to the top irons, aud may be taken out as soon as the earth is rammed up near them,

so that no holes are left in the
more than the bolt holes.

When these frames are used, o
direction, that the front or end m
away, and then by means of the
other return is sure to stand at
Care should then be taken, in t
sides level: that being done, the
the nature of the frames, and m
of course come upright and lev
care, and a wall being properly beg
After the first course of a buil
should be moved to another, an
are up; and as the top holes of
come the bottom holes in the suc
will be found in fixing the moul
properly done.

Fig. 6. shows the iron pin an
ternal angle of the frame togethe
to set the returns at right angl
where other means of setting the
be obtained.

Having described the frame,
generally, it may be necessary to
ticulars in the process. Having
the building, it frequently happe
comes too dry to attach to the ne
therefore it is adviseable that, as
the succeeding course, a small qu
posed of $\frac{1}{5}$ lime, and $\frac{4}{5}$ earth, b
course, immediately before the fi
A very small quantity is sufficie
the strength of the work, by ce
together at the joints. The w
the corner of his rammer, in ram
joints, cut down a little of that p
he works; this will make the up
and unite in a solid manner. H
got up the walls, the next thin
holes, with mortar made $\frac{1}{4}$ lime a
wall.

The earth proper for this work
clay, but partaking of both. Cla
able, as is also chalk, or calcareo
is also not proper, unless accom
quality: the bolder and coarser t

When used, it should retain no more moisture than just to make it adhere together, under the pressure of the thumb and finger. Notwithstanding earths bordering on sand appear to make the strongest work, nevertheless good earths may often be found in parts that do not abound with sand. Those that abound with a mixture of grit or fine gravel are generally the best. Having provided proper earth, as much should be put in each layer as to form about an inch and a half when compressed by ramming.

The rammer X should not be more than half an inch wide on the edge, in order that it may more forcibly compress every part of the earth, which a flat rammer would not do so well.

In making the walls, about three inches in thickness of loose earth should be put in each course, which done, the same, by means of a trowel made for the purpose, is drawn back and cleared from the face of the wall, and the space then filled up with the facing composition, forming on an average about one inch in thickness; the whole then is firmly rammed, (in which, and properly preparing the facing stuff, much depends the perfection of the work) till it is quite hard, when it will be compressed to about one inch and a half in thickness. The common facing stuff is composed of lime one part, and earth, the same sort as used for walling, three parts. The lime and earth mixed and slacked together, the same as for mortar. The more it is slacked and wetted the better, provided time can be allowed for it again to dry and pulverize, so as to be fit for ramming. The better sort of facing stuff may have a small quantity more of lime in it.

The foundation should be of brick or stone, carried up nine inches above the ground; and if a plinth is to be shown, then one course above the same should be of brick or stone, to prevent the water that might lodge on the plinth from damaging the earth wall.

The proper season for performing this work is any time that the earth is to be procured sufficiently dry for the purpose; the more early in the season the better, in order to give it time to dry before finishing, or if late it would be adviseable not to finish till the year after it is built.

Windows and doors may be left in the walls wherever wanted, by fixing the head of the moulds and carrying up quoins to form the same: in erecting which some bond timber should be laid in coarse mortar and rammed in with the earth. Lintels may also be laid at the proper height. This method is cheapest, where only one window or door of a

size is wanted; but if many, the
make some rough frames of boa
ness of walls, and place them in t
and doors. When done, the ea
laying bond timber at the sides a
both cases the windows and doo
their places and fastened to the
is up. The bond timber, lintel,
as thin as possible, in order to pr
tween the earth and timber in
the same. The bond timber ab
wall plates 6 inches by 2; lintel
it may be worthy of notice that a
be used, the earth being sure to
in place.

For common cottages, when
up and covered in, the holes s
coarse mortar made the same
wetter, and the wall then lime-
sharp sand, which should be n
and used while hot. This may
knob of lime and sand a little at

For better kind of cottages th
may be used, and then, as befo
or if it be required to make the
sible, the following is the best n
brush thoroughly wet and soak
or three yards in superficie at a t
the said wetting, should be con
about with a hand float, till su
smooth and even, by which the
wash up as to become a pleasant re
and hard when dry, and not lia
plastering would do. This fini
by a small quantity of lime bein
soaking the face, and if, after
rubbed, as above mentioned, th
a brush some of the lime and sa
whiting,) and that also worked
then become as perfect and hard

Having explained the frames
performing earth walling, as also
I beg leave to lay before the S
these, compared with the origin
ner of performing the same, as
of Communications to the Boar

The sides of the frames, as formerly constructed, were supported on joists or cross pieces of timber, which pieces were cut into the top of each course of walling. The sides were then kept together by upright timbers framed into the cross pieces or joists, and the tops of the upright pieces were wristed and held together by ropes going across the frame from one side to the other. In consequence of this construction, by experience I found much labour was lost in cutting the channels to lay the cross pieces in. These channels, after the buildings were up, took labour and materials to fill them in, and rendered the walls less strong. Also the difficulty of getting the frame rightly placed every time it was moved, and the elasticity of the rope across the top, made the whole very imperfect, so much so that all work done in that manner was untrue and unsound; as the rope, however tight it might be strained, would yield to a certain degree. The labour of moving was great, and when the frames were set, the cross ropes and uprights above the sides were much in the way of the workmen.

On examining the model I have the honour to send, it may be seen that these frames being once set true, they require very little care afterwards: being kept together by iron bolts, no elasticity can occur, and the earth will be as firmly compressed as if rammed between two walls. No cutting away for cross pieces is required, nor any holes but the small bolt holes to make good; and as nothing sticks up above the frames, the workman cannot be impeded. In consequence of these alterations the work may be more cheaply and truly executed than with the old sort of frame.

Previously to entering into the expense of this sort of work, on my conceptions as to its advantage, it may be necessary briefly to state from whence such is collected.

About sixteen or eighteen years ago, the late Duke of Bedford directed a foreigner, who was then making some walls in Lancashire, to come and make some specimens here, and wishing to know how far it might be usefully introduced, I was directed to give attention and every aid to the man employed. Accordingly frames of the old sort were made, exactly like those before described, and with them some specimens being made, the man returned. These specimens I considered were very bad walling, and in attending to the execution thereof, seeing sufficient room for improvement, I was directed further to practise it. Frames were then constructed like the model, and several walls erected, among which were some cottages now standing,

and lastly, the house
about twelve years, an
the practice: the hou
the walls, as if built o

With regard to the
labour is the principal
places labour is dearer
mating it at different p
man should do in a da
yard superficial, in the

At this place the ex

Labour to making fa
and ramming to a 1
is at hand (labourer
per yard superficial .
Value of lime used in
into the face of a y
8d. per bushel) . . . .
Lime and labour to ru
outside face of the w

Total finishe
If a wall to a garden or
faced on both sides,

Total for wal

At this place the wa
than ten shillings, of
bricks being 48s. per
sequently the economy
same difference will be
and bricks bear the sa
found at hand. But as
it is not applicable, or
failure, may prevent its
ful, I shall endeavour
have struck me, and ev
against it.

Many persons have
that almost any earth w
tuous; for proper ear
it being difficult to de
found, it seems advisea

siderable work, that the experimentalist should first do a small piece, and let it stand with the top only covered for a winter at least.

It has before been observed that the excellence of the work depends on its having due compression, as well as being of proper soil. If the compression be not perfect, although the soil be good, the walls will be unsound; and unfortunately it so happens, that when a wall is built and badly rammed, its imperfection cannot readily be observed, and further, the defect is likely only to be found but by its failure: and hence arises the greatest bar to its general introduction; for, as it requires considerable labour to build a wall, it requires exertion to do it in proper season; and if the labourer be employed to do the work by task, it becomes his interest to get on and do it slightly, and if done by day, it will not advance so rapidly: consequently, in either way, it will require great attention from a careful overlooker.

From the foregoing comparative statement of *pisé* against brickwork, persons unacquainted with building are inclined to suppose that the whole expense of the building will be in proportion thereto: contrary to this, it only affects the walling,—the roof, floor, &c. remaining the same as before, excepting as it may reduce the quantity of bond timber and lime used in plastering the inside; this latter is less than when plastered on brickwork, the face of the wall being so much truer than brickwork.

A working drawing, on a scale of one inch to a foot, is left with the Society, for the inspection of any person inclined to construct the apparatus.

LII. *Memoir on the Alterations which the Light of the Sun undergoes on passing through the Atmosphere. By M.* HASSENFRATZ. *Read to the Class of Mathematics and Physics of the French Institute, 20th October* 1806.[*]

THE sun presents different colours to our eyes: its disk appears white, yellow, orange, or red, according to the purity of the air, the height of the orb in the horizon, the latitude of the places where we observe it, and their elevation above the level of the sea.

In the torrid zone, the disk of the sun is always white, when the air is pure, and when it is at the zenith of the place.

In the frigid zone, the disk of the rising or setting sun is always red in the shortest days of the year.

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[*] *Annales de Chimie*, tome lxvi. p. 54.

On high mountains, at an
zon, the disk of the sun is con
valleys and plains.

In general, the white, yellow
the disk depends (if the purity
the thickness of the strata of
through before reaching the eye
ner the strata which are passed
disk: and *vice versâ*; the disk
orange, and afterwards red.
exhalations, when it contains s
suspension, and when its purity
of the sun is sometimes coloure
intensity of its light diminish
white*.

The cause of the colouring
those problems which ought t
philosophers, and which interes
tion to its influence in the phæn

It has been strongly asserted,
by rays subtracted from the fas
through the air; but we are ent
number and species of the mol
the colours of the disk may re
one or more coloured molecul
ought to be fulfilled, is, that the
subtracted rays should be com
are perceived.

The azure colour in which th
has induced some persons to
subtraction of some blue mol
that the colour of the disk w
thought that the purple, violet,
more refrangible and more refle
separated from them in passin
and that the red, orange and
them, which had not been refl
junction the colour of the disk
sumed that the violet and gree
by the air at the same time that
green molecules were refracted.

Although the colour of the
nounce an action of the air on t

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* *Hist. de l'Acad. des Sciences*, 1721.
of the sun having appeared the whole d
white, and its lustre as usual, but withou

although it ought to be presumed that the solar spectrum should undergo variations flowing from this action; as no astronomer, to my knowledge, has announced, that in the experiments which have been repeated at various times, and in different places, variations have been observed in the colour of the spectrum, and as all are silent as to a phænomenon so singular; it might be supposed that the cause of the colouring of the disk depended on an order of alteration with which the spectrum was not affected.

Those hypotheses which attribute to the molecules of air properties so different were presented under an aspect more or less seducing; all of them could be discussed, defended, and adopted, if the reasonings and authorities of authors had been sufficient: but as none of those who have proposed them have supported them by any positive facts, I appeal to experience.

The most natural experiment, and that which every author ought to make before proposing his hypothesis, is the analysis of the rays of light when the disk of the sun is presented under colours so various. This analysis has been effected, and I now present the results to the class.

I fixed upon some fine days in the summer of 1799, when the sky was pure and the disk of the sun white, which always happens towards noon, when the sun is at its greatest elevation.

With this view I introduced a solar ray into a dark room through an aperture of the size of 25 decimillimetres: I received it on the surface of a prism, which I turned so as to make the angles formed by the ray refracted, and the two faces of the prism equal. I observed at one and the same time both the series of the colours of the spectrum and its length at 30 decimetres distance from the prism. I remarked that all the colours were perfectly distinguished from the purple to the red, and that on the evening of the same day, at sunset, when its disk appeared yellow, the spectrums formed by the solar rays were not so long: the purple no longer existed, and a greater or less portion of the violet was wanting, and sometimes it was even entirely wanting.

The experiments thus made at noon and evening were recommenced in the fine weather of the following years, and gave the same result.

I have the honour to present to the class three spectrums obtained with the same prism on the 13th of January 1801. They are remarkable for their length, and for the colours which form them. At half past ten o'clock A.M., the spectrum, fig. 1, (Plate VII.) was 145 millimetres long;

at noon, fig. 2, 185; at four o'cl
at ten minutes past four, fig. 4,

When the spectrum was lon
between the green and the o
violet disappeared, the yellow
was coloured red, and partook

I repeated these observatior
times of the year, and always
spectrum increased or diminisl
the colour of the disk was whi

Finally, on the 15th of Ja
observing the decomposition of
that when the disk of that orb
of the spectrum was diminish
was no more than 70 millimet
day it was 183; and in the seri
prism, we could only distinguis

M. Girard, draftsman to th
been present on the above occ
solar spectrum in question.
drawing, and fig. 6 presents t
was received on a white card in
deep orange, approaching the

The subtraction of one or
fasciculus which the sun send
orange, or red, may be easily re
at different hours of the day,
which they present, or in the
I have several times verified th
[illegible] an experiment wh

[illegible]

colour

are subtracted by the air, and that in this case the spectra and the irises ought to present a single colour only, which is the red.

LIII. *On the Application of the Barometer for indicating the Weather, and for measuring of Heights in the Atmosphere. By* Richard Walker, *Esq.*

To Mr. Tilloch.

Sir, In order to prognosticate the weather by means of the barometer, one general rule should be premised, viz. that, previously to observing the barometer, the state of the weather at the time should be accurately noticed in every particular. Hence, to speak figuratively, we might affix this motto to the barometer, " Tell me what the weather is, and I will tell you what it will be."

The circumstances to be collected previously to inspecting the barometer are, 1st, The state of the atmosphere, respecting its degree of clearness or cloudiness: 2dly, The direction of the wind, together with its steadiness or variableness: and 3dly, The altitude and density of the clouds.

Signs of Fair Weather.

1. The barometer *rising* may be considered as a general indication that the weather, comparatively with the state of it at the time of observation, is becoming clearer.

2. The atmosphere apparently becoming clearer, and the barometer above RAIN, and rising, show a *disposition* in the air for fair weather.

3. The atmosphere becoming clear, and the barometer above CHANGEABLE, and rising, indicate fair weather.

4. The atmosphere clear, and the barometer near FAIR, and rising, denote *continued* fair weather.

5. Our prognostic of the weather is to be guided, relatively, thus: If, notwithstanding the sinking of the barometer, little or no rain follow, and it afterwards rise, we may expect continued dry weather.

6. If, during a series of cloudy rainy weather, the barometer rise gradually, though yet below RAIN, especially if the wind change from the south or west towards the north or east points, clear and dry weather may be expected.

7. The weather for a short period, viz. from morning until evening, may commonly be foretold with a considerable degree of certainty. If the barometer has risen during the night and is still rising, the clouds are high and apparently dispersing, and the wind calm, especially if it be in or about the north or east points, a dry day may be con-

fidently expected :—the same ru
weather from evening till morn

8. During the *increase* of th
a greater disposition or effort in
than in the *wane:* but this di
commence till about three or fo
and ceases about three or four d

9. The barometer should be
in the day, or oftener when the
order to notice whether the me
or sinking; for from this circu
direction of the wind and the ap
th ion to be collect
s n change of the
, Observe always
n the scale in each
g ogressive its motion s
t tion: likewise, The me
, or east points, the g
r fair weather.
ations of *rainy w*
rse of those rules wh
indicated in winter
weather, the wind l
nts, and the thermo
of snow seldom come
uuration, and is indicatec
, especially if the merc
and the thermometer at or near

When the temperature of th
rain sometimes fall together; a
35° it seldom snows, or rains a

Thunder is presaged by the s
accompanied by sultry heat: the

Storms, hurricanes, and hig
barometer falling *suddenly*, c
MUCH RAIN.

The barometer is known to
mercury having either a conv
the perceptible rise or descent
of observation the barometer

If at any time the weather

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* A barometer, conveniently porta
taining whether the atmosphere is bec
der commotion. I should apprehend, not

† The best index for these observat
far as the surface of the column of
passing one across the centre of the p

indications of the barometer, it may be presumed, as is sometimes known to happen, that a particular spot is affected by local circumstances.

After a long continued series of wet weather, we may, when the weather becomes fine, expect an uninterrupted continuance of dry weather.

If, after a long series of wet weather, the barometer rise above CHANGEABLE, and the wind veer steady to the north or east points, a continued duration of fair weather may be expected.

Slow and progressive variations in the barometer, with a fixed and steady state of the wind, indicate permanency with the change.

The barometer standing at or above FAIR, denotes *generally* fair weather, although the atmosphere wear at the time an unfavourable aspect.

Lastly, The greater coincidence there is of the circumstances enumerated in the rules above mentioned, the stronger may our confidence be in the expectation of fair weather, and in the continuance of it when present, by the barometer whilst high, remaining stationary, or varying but little, and the state of the atmosphere, and direction of the wind, disposed to be settled.

In this variable climate, there is no reliance, I think, to be placed on any rules, beyond those above mentioned, for indicating the weather for any length of time together, or for any distant period.

Many of these rules, perhaps, may appear trite, and as if collected from the observations of others; but, unconscious of retaining those of any other person in my mind, I give these as the result of my own experience.

A Summary Method for ascertaining without the Use of Logarithms, *the various Degrees of Elevation in the Atmosphere, by means of the Barometer, which, by the Examples that follow, will be found to give a tolerably close Approximation to the geometrical Measurement.*

The following Tables are applied thus:

1st. Note down the height of the barometer at the *lower station*, in inches, tenths, and hundredths*; and the temperature of the air, as indicated by the thermometer.

2dly. In a similar manner note down the height of the barometer at the *upper station;* and the temperature of the air (if this differ from the former observation).

3dly. Subtract the quantity shown by the barometer in the *upper station,* from that which is shown in the *lower station.*

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* I have judged it unnecessary to descend below *hundredths* on the barometer.

4thly. Seek

4thly. Seek in the column o
the temperature observed at th
of the two, if they differ,) and
feet, tenths, and hundredths, w
that temperature in the adjoin

5thly. Make corrections (if

6thly. Multiply the number
by the difference between the
the lower and upper stations.

7thly. Add to the product
Table III., which finishes the

Table I. exhibits the number
dredths, in perpendicular altitu
of the mercury in the barom
of an inch; which descent v
perature of the air, at the rate
column. This table is constru
the barometer at the *lower stat*

N. B. Although this calcula
dredth part of an inch of variat
be used for variations of *one ten*
altering the denomination of the

The variation of 100th part of
perature 90°, indicates altitu

The variation of a 10th part o

The variation of an inch ...

Table II. shows the alterat
the numbers taken from Tab
not at 30 inches in the *lower* s
30 inches, the number of *feet*,
the barometer's height in this
feet of altitude corresponding
cury in the barometer for *one i*
inches, *subtract* instead of add

N. B. If the variation of t
as an inch, but only some
figures in the second colum
tenths of feet to be added or s

Table III. shows the num
added to each 100 feet of al
from 1 to 46, according as th
tion stands between the altitu

N. B. A table for the corre
the barometer, when the te
differs at the upper and lower
by the adjustment of the tabl

TABLE I.

Temperature.	Altitudes for the Variation of every 100th of an Inch on the Barometer. Feet. Tenth. Hund.
90	9,9,5
88	9,9,0
86	9,8,5
84	9,8,0
82	9,7,5
80	9,7,0
78	9,6,5
76	9,6,0
74	9,5,5
72	9,5,0
70	9,4,6
68	9,4,2
66	9 3,8
64	9,3,4
62	9,3,0
60	9,2,6
58	9,2,2
56	9,1,8
54	9,1,4
52	9 1,0
50	9,0,6
48	9,0,2
46	8,9,8
44	8,9,4
42	8,9,0
40	8,8,6
38	8,8,2
36	8,7,8
34	8,7,4
32	8,7,0
30	8,6,6

TABLE II.

Height of Barometer. Inches. Tenths.		Feet.
5	*Subtract.*	15
4		12
3		9
2		6
1		3
30,0		0
9	*Add.*	3
8		6
7		9
6		12
5		15
4		18
3		21
2		24
1		27
29,0		30
9		33
8		36
7		39
6		42
28,5		45

TABLE III.

Increasing Ratio.

Inches. Tenths.	Feet.
31,0	1
30,0	1
29,0	2
28,0	3
27,0	4
26,0	5
25,0	6
5	7
24,0	8
5	9
23,0	10
5	11
22,0	12
7	13
3	14
21,0	15
7	16
3	17
20,0	18
7	19
3	20
19,0	21
7	22
3	23
18,0	24
7	25
5	26
3	27
17,0	28
8	29
6	30
4	31
2	32
16,0	33
8	34
6	35
4	36
2	37
1	38
15,0	39
*14,0	46

Example

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* On the top of Chimborazo, one of the Andes, in South America, and the

Examp

Lower station . . . .
Upper station . . . .

D

Temperature of air 60° . . . . . .

For 29 inches at lower station,

Increased ratio, 3 to each 100, T

Estimate

Examp

Lower station . . . .
Upper station . . . .

I

Temperature of air 60° . . . . . .
(Lower station 30 inches, no
Multiply 92,6 feet by 48 tentl
Add for increasing ratio 1½ pe

Estimated

Height by geometrical measur

Exam

Lower station . . . .
Upper station . . . .

Dil

Mean temperature of air 54° .
For 29,9 at lower station add

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the highest mountain in the world,
low as 14 inches, its summit being 20
The greatest height to be relied on, to
a balloon, is that at which the baron
is equal to 6,027 feet; though some a

Feet. Tenth.
Multiply 917 by 3,6,9 .... = 3383,7
Add, increasing ratio 5 per 100.... = 169,

Estimated height.. 3552,7

Height by geometrical measure ...... 3558 feet.

Example 4.

Inches.Tenth Hund.
Lower station 29,9,3
Upper station 16,8,1

Difference 13,1,2

Mean temperature of air 64° .... = 934 feet to each
For lower station 29,9 add.......... 3 inch of variation.

937

Feet. Tenth.
Multiply 937 by 13,1,2 .... = 12.293,4
Add increasing ratio 29 per 100.. = 3,565,

Estimated height.. 15,858,4

Height by geometrical measure ..... 15,833 feet.

N. B. In the three last examples, the heights of the barometer at the different stations, and the geometrical heights corresponding thereto, are extracted from different works of unquestionable authority. I am, sir,

Your obedient servant,
Richard Walker.

Queen-street, Oxford,
Oct. 13, 1810.

LIV. *Œconomical Process for the Preparation of the sublimed Muriate of Mercury (Calomel): to which is subjoined an easy Method of purifying the Calomel used in Commerce. By M.* Planche *.

Anxious to avoid the inconvenience which results from the employment of corrosive sublimate in the preparation of calomel; convinced by experience that the various methods proposed with this view by Van Mons and Brugnatelli were insufficient; and considering the discrepancy which prevails in most of the pharmacopœias of Europe, as to the

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* *Annales de Chimie*, tome lxvi. p. 168.

doses of corrosive sublimate, ar
ought to be used in this prepar
merous trials, has ascertained
to obtain calomel, to sublime a
cury at the *minimum*, and of

Preparation of the Sulphate
a stone retort placed in a rever
crude mercury, and one part
at 66° of Baumé's areometer.
lated receiver to the retort, w
municate either with distilled
flasks, if we wish to collect t
the external air, if the situatio
gas being set at liberty. Grad
acid boils, and keep up the fi
disengaged in abundance, taki
the end of the operation, *i. e.*
which passes from the retort
each other slowly, and when
white vapours. After this oper
hours, the retort may be brok
rate by means of tongs the su
easily detached.

The acid sulphate of mercur
and very friable; it passes to
dition of the most trifling qua
der to carry this salt to the stat
the author combines it with
manner.

He takes 18 parts of the abo
and eleven parts of mercury.
in a mortar, or in a porcelain
six parts of cold water.

The first portions of water
yellow colour, which soon disa
is developed. The matter as
lour. After a few minutes tri
quantity of water to give to th
thick broth; and he continue
has become of a dirty white,
disappeared, which lasts for fiv
is considerable. He afterwar
stove at a temperature of 30° t

M. Planche is of opinion, th
results from this operation is i
minimum, and he proves it by

1. It is soluble in distilled water, and its solution does not alter the tincture of turnsole or the syrup of violets.

2. It is precipitated black by lime water, and gray by ammonia.

Preparation of the mild Muriate of Mercury. To convert the sulphate of mercury into calomel, mix intimately, on a porphyry stone, equal parts in weight of the sulphate of mercury as above designated at the *minimum*, and of sea salt purified and dried: introduce the mixture into matrasses with flat bottoms two-thirds of which are left empty, and proceed to sublimation in the usual way. After the operation, which lasts five or six hours, there will be found in the arch of the subliming vessel a loaf of calomel of the weight of about 30 ounces, if four pounds of mixture are operated upon. This salt is as white as that of commerce, and purer than that which we commonly meet with as coming from the laboratories of Switzerland.

In order to add to its purity, particularly when the heat has not been well managed, the author of the memoir proposes the following method, which perfectly succeeded with him, and which has the advantage of having no action on the mercurial salt.

Purification of Calomel. Pulverize the calomel in a mortar of marble or of hard stone. Pass it through a fine hair sieve in order to obtain a homogeneous powder tolerably fine. Introduce the pulverized salt into matrasses of the same form as in the foregoing operation: afterwards cover it with a layer about two lines thick of fine sand, previously washed with water slightly sharpened with muriatic acid, in order to free it from the carbonate of lime and oxide of iron which are mixed with it, and sublime as before directed. The calomel purified by this process is very pure. M. Planche has presented to the Institution a loaf of it very regularly crystallized, and in whiteness equal to that of corrosive sublimate.

LV. *Description of a Process by means of which we may metallize Potash and Soda without the Assistance of Iron.*

By M. Curaudau[*].

The decomposition of the alkalis, which I have never regarded as simple bodies, having been long an object of my inquiries, I became anxious to repeat the experiment according to which Messrs. Thenard and Gay Lussac have

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[*] *Annales de Chimie*, tome lxvi. p. 97.

announced that potash and s
metal by means of iron; bu
more satisfactory than those
repeated the same experimer
which appeared to me to be th
as Mr. Davy had thrown so r
nomena which I had observe
count for.

In short; if, according to t
English chemist, potash and s
it not more than probable tha
nothing else than the combin
coal? Such at least was my
seen how far it was well fo
metallizing potash and soda,
these two alkalis with charc
shall find, enters into the prus

The metallization of pota
with one or other of the two r
out, and succeeding equally w
pipes, we may employ the f
criminately. As to the nat
iron one, because it is more
less subject to fuse than ston
is penetrated with alkali; an i
us from bringing the operatio
so frequently happen with th

First Process. Mix exactl
animal charcoal with three pa
in the fire without having bee
with a sufficient quantity of
make a paste.

Second Process. Take tw
intimately with one part of c
in the foregoing experiment:
quantity of linseed oil, but so
ing in a pulverulent state.

Whatever be the kind of
the substance in question,
cond mixture be used, we m
gradually: but as soon as th
increase the fire until we see
iron pipe a fine celestial blue
greenish. To this light ther
dant vapour, which obscures
and which is the metal extric

The fire must no longer be increased, for at this temperature the retort begins to melt; and if the iron resists better, it is because the alkali penetrates it less speedily than it does the stone; and also, because the heat which it receives is sooner transmitted to the matter.

In order to collect the metal as fast as it is formed, introduce into the vessel a piece of iron well scoured; and as we must not give it time to become red, it must be withdrawn in four or five seconds: it is then entirely covered with metal, which may be removed by suddenly plunging the iron into a glass cucurbit filled with spirits of turpentine. This cucurbit ought to be dipped in a bucket of water, in order to prevent the spirits of turpentine from boiling. Still, however, in spite of this precaution, it is sometimes so heated as to take fire when the pieces of iron are introduced into it.

Requisites for the Operation. It requires three persons to perform the operation well. One must work the bellows, and take care of the fire: the most expert of the attendants collects the metal as it is produced, and plunges with the utmost celerity the pieces of iron into the turpentine: the third assistant removes the metal which adheres to the iron, and afterwards dips it into the water, as well to cool it as to remove the alkali which has escaped metallization, and that which is formed by the combustion of the metal before its immersion into the turpentine. He takes care also to clean the pieces of iron well before using them.

This operation requires the most dexterous manipulation while the metal is forming. The bellows must also be carefully managed; for, if the fire be suddenly slackened, the metal ceases to be set free, and the pieces of iron are covered with pure alkali only: if, on the other hand, the fire is hastily increased at this instant, the vessel melts, and the experiment is fruitless. This proves, therefore, that the temperature ought to be uniform and steady. I have observed that it is always at the heat of melting iron that the metal is produced. It rarely happens that an iron pipe serves twice, and the retorts melt long before the whole of the metal is obtained which the alkali can produce.

I purpose subsequently to make known any observations which I may happen to make on this metallic produce: in the mean time I think I may infer from my experiments, that the production of the metal is not owing, as has been said, to the deoxygenation of the alkali, but is on the contrary a new compound, into which hydrogen seems to have

entered in combination, and
be in a very condensed state in

To conclude:—During the
extricated, alkali not metallize
have collected this last produc
tities.

These results tend, therefor
gen is one of the constituent
disengagement of which is fa
that the charcoal itself is a cc
is one of the principles. We
other of these hypotheses.

LVI. *Reflections on some M*
R. CHENEVIX, *Esq. F.R.S.*
lated entire from the Fren
lator†.

FUNDAMENTAL PRINCIPLES A
OF THE WERNE

NATURE has given to all bod
immediately or mediately sens
systems of mineralogy. One
ing useful is by establishing
may be referred, and which
wholly or in part. We seek
and least variable possible, to
determination of these uniti
agreed to call the latter by the
getable and animal kingdom
individuals fecund and similar
a species.

This principle of specificati
ful; but it is not applicable tc
there exist any other? If we
mineralogy, we are tempted
wanting. In more modern ti
seeking and noting in minerals

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* The opinion expressed in this p
the French chemists.—EDIT.

† As the learned author has not tho
and scientific reflections in his native l
regretted, the translator has, for the sa
classing them under different heads, ac
has also ventured occasionally to intr
synonyms of the various minerals m
enable the English reader to form mor
stances, and feel the force and justice c

strikes the senses; and he has succeeded in uniting the principles which enable him to class and distinguish minerals into a body of doctrine. The colour, brilliancy, fracture, and other properties, have been examined in a point of view the best calculated to attain this object: the advantages which a knowledge of the latent qualities may offer have not been neglected;—these labours have procured the author the approbation of the learned world. In this manner a great step has been made; and if it does not conduct us entirely to our object, it at least demonstrates the difficulty of attaining it. M. Werner has said (Memoir of Daubuisson, *Journal de Physique*, Frimaire, 16th year), "that all the minerals which have the same constituent parts, both with respect to quality and quantity, form only one species; and that all those which differ essentially belong to different species. If, in the same species," he adds, "divers minerals having the same characters (one only excepted) differ from others in two or three characters (a greater number would induce a difference of species) from those which we have designated, they form a particular subspecies. Finally, When an individual in a species, or subspecies, presents but one different character, it forms a variety."

To the word *essentially* I have two objections. In the first place, it does not excite the same idea in all minds, and we can have no precise notion respecting it, while it has no fixed signification. In the second place, the chemical means which could enable us to pronounce with some certainty on what belongs essentially or accidentally to the composition of a mineral, are entirely omitted. But let us suppose, for a moment, that we have acquired the necessary knowledge. *The chemical composition is therefore the true basis of specification in this system*[*]. We also learn from the above quotation, that when two minerals of the same species differ in one

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[*] To this the Wernerians object, that it is degrading to mineralogy to be dependent on chemistry; that it is possible for a man to be a very good mineralogist without being previously a chemist; and that they are two distint and independent sciences. In support of these positions, they sometimes appeal to the increasing number of botanical nomenclaturists who are not vegetable physiologists: but the allusion only tends to place mechanism before science; the former are to the latter what sculptors and lapidaries are to scientific mineralogists. The Wernerians, therefore, when they reject chemical science, and build solely on their external characters, place themselves on precisely the same basis as the lapidaries and sculptors; they become artists, but not men of science properly so called. They may indeed be most acute observers, very accurate reporters of their observations, and even pioneers in the fields of mineralogical science; but they ought not to aspire to be world-makers, or attempt to raise any superstructure without the aid of chemistry; while mineralogy and particularly geology are not less sciences of deduction than of observation.—TRANS.

character, they are varieties ; if
subspecies. If the number of
exceed three, the minerals thu
to different species. Here, th
species which has just present
with the first, we have only to
more than three specific charac
quence of an essential difference
of minerals.

We cannot be perfectly se
should be that which charact
mical composition, and we a
has been taken at random. It
a motive. The species is sub
variety ; the variety, therefor
the species ; and to determine
the variation of two or of th
therefore, necessary to carry b
species ; that is to say, (acco
quoted,) the essential difference
I have said that there was a ve
the number three ; but I have
ble of being acknowledged by

In establishing the difference
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we render it independent of
the value of the characters, o
shades of these characters di
matters not, that two minerals
racters so different that they f
extremes of the series of charac
not discover a fourth which is
nerals must belong to the same
whatever of minerals that I ca
a difference in three specific ch
first portion be opake, ductile,
the nail ; let those of the secon
and so hard as to resist the fil
be entirely similar in every
which compose these two port
since the number of differen
three. Moreover, let us sup
brownish red colour, except on
dish brown. Here we must
in which the single shade betw
brown, (for M. Werner places

among the colours) effects the translation of a mineral from one species to that of another, as it is that which, superadded to the first, forms the fourth characteristic difference, while the other minerals always remain in the same species as before.—It is the last pound under which the camel succumbs.

Here is a second example of the very serious inconveniences arising from making the species depend on any fixed number of characters. Form is a character, crystallization is comprehended in forms. M. Werner admits the prism only where there exist certain proportions between its height and its breadth; if we diminish the former, the prism becomes a table. The prism and table are considered as making two different species of primitive forms. Take a crystal of calcareous spar in a hexahedral prism, and another crystal of the same in a hexahedral table; here is a different primary specific character in these two specimens of calcareous spar. Suppose the table very near becoming a prism, and the prism approaching very near to the table; and suppose the one translucid and the other opake; add two specific characters not less insignificant than these two, and behold a new species at very little expense. But, had the causes which determine crystallization added some molecules of carbonate of lime to the calcareous spar, in the direction of the axis of the prism which was considered a table, it would have been saved from this forced separation from its equals.

In establishing a subspecies, if the liberty of choosing two or three for the number of characters by which the observer decides, leave any influence to the particular value of each character, it is necessary that this value should rest on a solid basis; otherwise we risk the danger of making arbitrary dispositions, and the same mineral may be found belonging to as many different species as there will be persons who shall examine it. M. Werner, indeed, has distinguished some characters by the order of importance in the determination of the species, as well as in the diagnosis of minerals. He gives sometimes to the specific gravity, sometimes to the colour, more value than to the greater part of the other characters. I have seized every opportunity to acquire clear ideas on this subject, either in consulting M. Werner himself, or addressing those who had profited most by his instructions; and all that I have been able to learn amounts only to this, That the value of a character varies from one species to another: thus, then, to decide on it, it is necessary to know the species, that is to say, in

algebraic language, for to fin
commence with knowing it.

WERNERIAN DIVISION OF TH

M. Werner* has divided th
neric characters, specific char
rieties which influence the sys
nerals under analogous denom
and specific gravity undergo
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the colours. The shades of t
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don- or sea-green is a variety o
is another; sky-blue is a vari
is one of yellow. These dist
as soon as we understand to p
specific colour according to
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than between sky-blue and
between gosling-green and s
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than two species differ. T
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The division of external c
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new difficulty; for we here se
tion relative to minerals. W
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unless that the consistence of these principles be not so necessarily united as to prevent their separation. For example; if a certain colour was an inevitable consequence of the presence of a certain constituent part, we might adopt the colour as a principle of classification, at the same time with the presence or absence of this constituent part. But, in doing this, we would admit at bottom but one single principle as the basis of the system, since the existence of the one would necessarily imply that of the other. As M. Werner has admitted external characters to form the basis of his system, at the same time that he explicitly declares, that "all minerals which have essentially the same constituent parts both with respect to quality and quantity form the same species," we must suppose that he has discovered certain connexions which exist between these characters and the essential chemical composition of the same mineral. The results of chemical analysis, nevertheless, do not correspond with this supposition; and the science which unfolds the composition of minerals pronounces it in a manner that does not agree with our received ideas of the external characters. At the first glance over the classification of M. Werner, we may perceive the difficulty in which this contradiction involves us; for the desire of reconciling two things dissimilar in themselves, has introduced an uncertainty which prevails over all its parts. If we wish that this celebrated author should remain faithful to his principles, I see no other mode than to suppose that he takes the testimony of external characters as the index of the chemical composition, rather than the results of chemistry itself.

Other authors, who have published works according to the principles of M. Werner, tell us, that although this philosopher considers all minerals which correspond in external character and chemical composition, as belonging to the same species, he does not pretend that his arrangement should agree with the experiments of the chemist. This is to speak at hazard, and to avow frankly that he regards theoretical assertions as superior to experience, and the system which he has adopted as preferable to the principles of science. It would therefore only be when the chemical results agree with the external resemblances of minerals, that they could occupy a place in this system. We see sensible characters combined with chemical composition to determine a species; but if they do not agree with the results of chemistry, this science can be of no utility.

Such

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things are to be known, viz

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2dly. If all those which
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3dly. If all the minerals
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4thly. If all those which
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5thly. If the minerals are
cies, and varieties, accordin
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from each other, than those
different varieties, &c.

The relative condition in
most striking manner by W
divided into three species,
gives the same results. In
we have two species* with
it is the same in appatite,
Gypsum and fraueneis (sel
carbonated lime presents
ing number of 14 species,
and six varieties.

In the 3d Art. we ha
earth, and siliceous beryl
has instead of it fluoric ac

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* [illegible]

affinity in their chemical compositions, belong to the same species. (June 1805*.)

I shall not stop to give examples proving that the 3d rule has been abused: it has been violated at almost every step. In the greater part of the minerals which are but varieties of the same species, if we examine them closely, we shall find more than three specific characters which are dissimilar. The division of characters into generic, specific, and characters of variety, and the little precision which exists in all that has been said on the number and importance of characters, render this examination irksome.

As to the 4th Art. I asked the celebrated author of the system of external characters, if there existed a sufficient disparity between the properties of sulphated barytes and sulphated strontian to constitute them two species; and he answered No. Here chemistry makes two genera where the external characters would not have two species.

For the 5th Art. there is not less difference between the garnet and pyrope, quartz and *eisenkiesel* [iron flint, Jameson], beryl and emerald, than between the common or compact feldspar and *hohlspath* (*macle* of the French), potter's clay and *schieferthon* [slate clay of Jameson, and *argile feuilletée* of Brogniart], mountain cork [*asbeste tressé* of Haüy, or *A. suberiforme* of Brogniart], and amianth, calcareous spar, pisolite [peastone of Jameson, and *chaux carbonatée concretionée* of Haüy] and compact, common and fibrous limestone.

I have chosen only a few examples; but they are sufficient to prove that there is not one of the rules proposed to serve as a basis to the system which has not been infringed; sometimes one prevails, sometimes another; and we can only refer the consequent instability to the insufficiency of the principles.

PHILOSOPHERS AND PHILOSOPHY OF FREYBERG.

During a residence of 18 months at Freyberg, where I had every day occasion to admire the precision and accuracy with which the learned professor recognized minerals at the first view, and where I was more than ever convinced by the example of others of the difference which exists between the institution of species and the knowledge of individuals,

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* Professor Jameson has even gone further, and divided, after Werner, beryl into two subspecies, calling the one "*first* sub-species, precious beryl" (beryl of Kirwan), and the other "*second* subspecies schorlous beryl," the pycnite of Haüy, and shorlite of Kirwan; thus indicating a relation in numerical order which has no existence in nature.—Trans.

I neglected no means of forming to myself a distinct idea of the former. Sometimes they spoke to me of the chemical composition; but when I cited the zircon and hyacinth, I was answered, that the external characters made the difference. If heavy spar and celestine were the subject, they again referred to chemistry. Often they spoke to me of approximate characters (*caractères des rapprochemens*), or characters of agreement and disagreement, of which no mention has been made in the enumeration; and they quoted to me, as a reason for placing potter's clay (*glaise*) and schistose argil in the same species, that both are disunited in water. To justify the separation of chalk from mineral agaric [rock milk, Jameson; and spongy carbonated lime, Haüy], of foaming earth [or schaum earth of Jameson, silvery chalk of Kirwan, talcous pearly carbonated lime of Haüy] from *schiefferspath* [slate spar of Jameson, argentine of Kirwan], they relied on the external characters; and to prove that *bitterspath* [muricalcite, Kirwan; *chaux carbonatée magnesifere*, Haüy; *chaux carbonatée lente picrite*, Brogniart] justly makes a species different from calcareous spar, they turned about to chemistry without daring openly to claim its support. Sometimes the colours were but shades or accidents; sometimes they offered characters of the highest importance. At other moments they confessed to me that *they made species by instinct*: and when I complained of not being satisfied with some conclusions indicated by this guide, they answered, "*One is not always in his instinct.*" Finally, after being detected in every manner, they referred the specification to the *tact* of the observer *. But, in this respect, who should venture to make species if not M. Werner alone?

If I have spoken of these details which I often collected in conversation, and if I have quoted the words of other persons as well as M. Werner, it is to prove that in the *system of external characters there are no principles of specification which could serve as the basis of any science*; for, if there had been any, it is more than probable that some one would have been able to show me them; and, until that I receive a clear and distinct answer on this head, I shall be pardoned for believing and saying that there are none.

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* True philosophers are deeply indebted to Mr. C. for this clear and manly exposition of a system not of science but of delusion worthy only of the lowest religious jugglers and fanatics. The "mineralogical instinct" is certainly a new faculty discovered in the human mind by the philosophers of Freyberg, whose ardent zeal in propagating their opinions furnishes a better proof of their passions than of their logic or reasoning powers. It may, perhaps, be laid down as a general truism, applicable in every branch of natural philosophy, that all schemes or systems of natural knowledge may be esteemed scientific or dogmatic just in proportion as their followers embrace them by reason or by passion. Science is properly a creature o reason, and modestly retires whenever the passions or affections appear opinions, being originally suggested by the feelings, are naturally supporte and propagated by the passions, while science can only be maintained ar disseminated by close abstract reasoning. Hence it is not difficult to co ceive why some of the more imprudent Wernerians have expressed the selves with so much violence against the volume containing the above sta ment of facts and reflections.—TRANS.

BASIS OF A SCIENTIFIC SYSTEM OF MINERALOGICAL SPECIFICATION.

If in the multitude of properties which distinguish bodies we are fortunate enough to find those which lead to a more certain and exact determination, let us hope that when properly unfolded they may be converted into principles, and that a science shall spring up from the whole. The precision of the terms which it shall employ will be the measure of its accuracy, and the definitions become its language.

The knowledge which we have hitherto acquired, furnishes us with two means of appreciating in bodies those qualities which escape the cognizance of our senses. These means are physical and chemical; they unite, to the advantage of being able to appreciate with more precision the properties which on the first view are but imperfectly discovered, that still greater, of developing the new properties which are only manifested by indirect means. Having seen the little success attending the system of immediately sensible properties, and the little hope which remains of improving it, since M. Werner has not been able to make it better, let us have recourse to the succour of these two sciences to establish mineralogical species.

Physics and chemistry furnish us two modes of attaining the final results of the division of bodies. Without entering into useless metaphysical discussions on infinity, we may suppose any substance whatever reduced to the finest and most imperceptible particles which the mind can imagine. This is the last point of physical division, and one of these grains presents us with the physical element of a body. Yet this element may be still very compound in another point of view, and undergo another species of division by means which are properly the province of chemistry. When the latter is also carried to its ultimate point, we obtain the chemical element. By physical element we understand that which occupies the smallest portion of space which we can conceive; chemical element supposes the least possible number of component principles.

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MECHANICO-CHEMICAL OR CR
OF HAUY, AND HIS DEFI
SPECIES.

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and has defined the species. "
says M. Haüy, " is a collecti

molecules are similar, and composed of the same elements united in the same proportion." It is the assemblage of all the minerals which agree, with respect to the final results of division, in their physical and chemical molecules, in the true expression of nature reduced to its greatest simplicity*.

This definition of the mineralogical species is rigorous, and leaves nothing to be desired; but it requires a knowledge of the integral molecules. In the first place, it requires us to ascertain what its form is in all cases similar to itself: in the second place, we must be able to determine the nature and relations of its chemical elements. The first problem consists in finding the planes which terminate the small solid called the integral molecule, or, what amounts to the same thing, a solid which may resemble it; for it is not the absolute but the relative dimensions of this molecule which are required. But, the planes which terminate this solid can be but those which are parallel to the different directions in which a mineral is divisible without breaking, or what has been denominated the direction of the cleavage.

It is otherwise with the problem respecting the chemical element. We know that there exist vacuums between the molecules of bodies, and that even these vacuities are very considerable: hence it is that foreign matters have so often interposed themselves, and altered the sensible characters of a group of molecules or of a mass. Suppose that all the directions of the cleavage parallel to the planes which terminate the physical molecule are ascertained. Whatever may be the dimensions of the piece in which these directions are found, we have the representative of the molecule; but as these dimensions necessarily exceed those of the molecule itself, it follows that the piece contains more than one molecule, and hence foreign matters may deposit themselves in the interstices. Hitherto chemistry possesses only the means of distinguishing the simple parts which compose the physical elements from those which are interposed. Hence a source of uncertainty in the results of chemical analysis; and, in order that it may enjoy all the

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* Most assuredly this conception, even were it devoid of basis, would do honour to the human intellect. Man is placed in the middle of the universe, as if to contemplate the infinite space which surrounds him. On whatever side he looks,—whether he contemplates those worlds whose volumes and remote distances are to him without measure, or whether he considers the atoms which form them and the laws by which these atoms are united,—every thing is to him infinite, and begets in his mind that sentiment of sublimity originating in a grandeur for which we have no expression.

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of each species might unite to
dimensions, and afterwards con
mass, in which each species

ble; or, they might be so blended together in their origin, that in the mass which would be produced, it would be impossible to discover one simple species whose molecules had contributed to form it.

TRUE PRINCIPLES OF FORMING MINERALOGICAL SPECIES.

In the mineral kingdom, therefore, we must admit the following four conditions, arising from circumstances which have presided at the formation of minerals:

1st. Simple minerals whose molecule we are able to discover:

2d. Simple minerals whose molecule eludes our researches:

3d. Compound minerals in which the simple component minerals are discernible:

4th. Compound minerals in which we cannot distinguish the simple components.

Of these four conditions there is but the first which gives the species with strictness, and which truly appertains to science; but the others belong to nature, and must not be excluded from the method of classification. If, then, we find in any mineral, characters sufficiently marked to establish a well-founded opinion that it is of the same species with some one of the first section, we refer it by analogy to this, and consider it as belonging to the same species.

What is carbonated lime?—It is a mineral composed of 0·55 lime, and 0·45 carbonic acid, and which has for its molecule an obtuse rhomboid, whose great angle is 101° 32′ 13″. Here is carbonated lime defined; and it is evident that in our principles a mineral which has these properties necessarily belongs to this species. What is compact carbonated lime?—It is a mineral whose chemical composition makes us presume with the utmost likelihood that it is of the same species as crystallized carbonated lime, and that it differs only in the circumstances under which it has been formed, not having permitted the symmetrical arrangement of its molecules, so that one might extract from the mass the solid, which it represents. Here the type is but presumed; and it is only after strong proof from analogy that we resolve to class in the species of carbonated lime, a substance which cannot be proved strictly to belong to it.

In granite, gneiss, and porphyry we distinguish the pieces of simple minerals of which they are composed. They appear to have enjoyed in their formation all the circumstances which could favour the union of the molecules of the same

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agate, jasper, hornstone, and the

are found in the argillaceous or clay genus of Werner, of which we are ignorant, and perhaps for ever shall be ignorant, to how many simple species they owe their origin. What do we know of the family of argillaceous schist, of serpentine, of *pierre ollaire* [potstone, Jameson; *talc ollaire*, Haüy, and *serpentine ollaire*, Brogniart], of pipe-clay, and of fuller's earth; except that we do not conceive why they have been made strictly species? Whenever a mineral gives no true representative of the species, and that we do not there find other physical or chemical properties to refer it with sufficient certainty to any whatever, in which it may be strictly admissible, it is better to make it a species of convention, in order to complete the outline which nature has traced.

DISTINCTION BETWEEN MINERALOGICAL SPECIES.

In this point of view, the species of the mineral kingdom should be divided into four sections, corresponding with the four conditions of which I have already spoken. The first should contain the species strictly so called (*espèces de rigueur*); the second, those by analogy; the third, those which I call geognostic; and the fourth, those of convention. All belong to nature; the first only appertains to science, if we wish to preserve to this word that idea of rigour which it necessarily carries with it.

The principle of M. Haüy embraces all those which are known in the first section, therefore this system embraces all the mineralogy which is capable of becoming a science.

The method of M. Werner extends to all the mineral kingdom. " Who embraces too much, badly binds," it is said: thus we know not what is a species, because all are species. We have a measure without unity.

I pretend not that the system of mineralogy should be subjected to the division indicated by these four sections; but if we wish to consider the bodies which compose this kingdom with respect to the rank which they ought strictly to occupy, we can no longer divide them otherwise; and even in classing them according to more essential principles, it would not be useless to mention this, in order that each individual may be estimated at its just value.

The advantages that mineralogy has derived from the philosophic spirit which directed the researches into the true type of the species, and the happy application of an exact method of determinating it, have been immense. All at once it is become a science; it is supported by fixed

principles susceptible of demo
problems, enlightened futurity,
of analysis.

SYSTEM OF HAUY AND W

By a happy anticipation of
confirmed by experiment, we o
properties the union of beryl w
staurolite, as well as the separa
cime [both are denominated c
his disciple Jameson], stylbite
from mesotype [radiated zeolite,
t llite. It has lef
as united zircon
ope, quartz and
mineralogical s
g to Werner, w
green earth, of p
[actinolite, Jam
e, Brogniart].
n two different
renouncing prej
omposed of alu
it is hard. It
e diamond am
mus, where the su
has not made 32
a mine, nor two in sul
phated barytes; and above all,
the earthy fossils. It has not
year to the side of a species fro
the preceding, and which some
from the side of its new neigh
elsewhere. It has not made dif
the other, traverse the whole list
able to find where to fix themse
guests, who go every where an
Its principles are fixed; and
time to pronounce, it virtually
poses more surely and leaves no
makes distinctions without differ
prochemens] without analogies
racters.

Notwithstanding, it will not p
persevere in retaining a rank wh
nor will it deem the circumsta

science varies in gaining new means of improving itself. Since the publication of his work, M. Haüy has already made considerable changes in his system, but all of them were foreseen and previously indicated, with the single exception of sphene, which he then knew only by some indistinct crystals. When one has laid down certain principles, the path of science is then found circumscribed, but its march is direct: if it change, it is only for a rational melioration, and it proceeds in advancing. If delivered to the current of opinions, or of hypotheses, it is discussed in every sense, and fortunate if it does not retrograde. For it there is no more surety, each one buffeting it at his pleasure. To reform is a great art, and to retouch without defacing requires great ability. Principles produce improvements, arbitrariness induces revolutions.

M. Haüy had formerly determined the molecule of *spargelstein* [asparagus-stone, the *chaux phosphatée chrysolithe* of Haüy and Brogniart] as well as that of appatite; but at an interval of several years, as the idea of comparing his results did not occur till M. Vauquelin had discovered that *spargelstein* is a phosphate of lime. Here chemistry was found to agree with crystallography.

A mineral was discovered, which some thought to be of the calcareous genus, which dissolved in acids without either effervescing or emitting fluoric acid, but which nevertheless gave traces of a combination with an acid; and M. Werner pronounced it a phosphate of lime. He was not deceived; but the difference which there is between reasoning and divining, is, to set out on a principle, or to start at hazard. Yet, even in setting out on a principle, we are not always sure of reasoning. Phosphorus in burning is phosphorescent and odorous; quartz when rubbed has the same properties; therefore silica is composed of phosphoric acid and lime. It is thus that a very celebrated German professor spoke, and he pretended to reason: his pretensions were so much the greater, that the combination of phoshoric acid and lime is neither phosphorescent nor odorous.

[To be continued.]

LVII. *On the Decomposition of Water by Charcoal.* By M. Tordeux, *Student of Chemistry in the Polytechnic School**.

In the note at the end of the observations of M. Figuier, on the sulphurets contained in the soda of commerce, in a

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* *Annales de Chimie*, tome lxvi. p. 318.

preceding Number of the Ann
example of the explosions wh
soap-works, which he ascribes
with atmospheric air, existi
vat, above the caustic lixiviu
mation of this gas, by suppos
crude soda contains, set free
ceeding that which is necessa
hydrogenated sulphuret, whe
with water.

We know that when an alk
water, the latter is partly d
formed, and the hydrogen set
the remains of the sulphur and
hydrogenated sulphuret. We
periment there is no extricati
low temperature.

Hence it is evident that the
over the soap-maker's ley, do
composition of the water by th

I have been led to ascribe t
the charcoal always met with i
a remark which I made sever
served that potash purified by
contact with vegetable substan
coloured by the charred substa
from them, when fused in a cr
of gas which took fire spontan
was red hot, its combustion
gas.

It appeared to me on reading t
the hydrogen of which he sp
duced by a nearly similar cause.
on this subject, and the object
results.

The potash, on which I ma
sides charred substances, also
the more considerable, as it h
desiccation; and all circumstan
peared to me that the carbonic
in this case by the resulting at
gen, and of potash for this acid
must have been extricated pure

In order to ascertain if this w
in a stone retort, potash similar
the crucible: in an instant the

off water from the potash, and a gas began to be set free, which issued incessantly during parts of the operation. This gas was insoluble in water; it had a feeble empyreumatic smell; it did not disturb lime water, and it was inflammable, burning like a mixture of hydrogen gas and carburetted hydrogen gas: it made lime water turbid after its combustion; when mixed with oxygen in Volta's eudiometer, it detonated by the electric spark.

The disengagement was kept up a long time at a weak heat; nevertheless I increased the fire until the bottom of the retort was red hot: I always obtained the same product, only the hydrogen became purer.

After some time the disengagement of gas slackened. I increased the fire: and when the retort was very red it began again; but the gas which I obtained this time was entirely absorbed by the water, and by the lime water which it rendered turbid. It was no longer inflammable, and proved to be pure carbonic acid. At the end of the operation, however, it left a combustible residue, when it was shaken with lime water: this residue was probably gazeous oxide of carbon. The potash had become almost white, and the retort was attacked.

It appears to me that we may explain this operation in the following manner: The water in presence of the charcoal and of the potash, acts in the same way as when it is in contact with an alkaline sulphuret or phosphuret. Carbonic acid and a carbonate are formed; since the potash purified by lime may contain at this temperature a greater quantity of carbonic acid than that which it contains already; and if when the retort is red hot it is extricated from this acid, this perhaps is merely owing to the combination of the potash with the earths of the retort, a combination which does not admit of the presence of carbonic acid. Lastly, the gaseous oxide of carbon certainly proceeds from the decomposition of a little acid by a residuum of charcoal.

I confirmed this experiment on potash very much charred and carbonated, obtained in the following manner:—I evaporated to dryness alcohol containing a great quantity of potash in solution; not effervescing with the acids, but very high coloured, although transparent. The evaporation was effected in a silver vessel in order to obtain pure potash: in proportion as the operation advanced, the potash became very black; and towards the end it swelled up, giving out an inflammable gas: finally, it became dry and spongy. It was treated with water, and evaporated to dryness without filtering; it was black like charcoal, and ef-

fervesced

fervesced a little. It was in
to distillation in a stone reto
to the lime potash. The res
solutely similar; and when I
retort, it was white, and exhil

I should in all probability
with soda purified by lime, if
experiments, considering the
between these two alkalis.

In order to assimilate my
which are performed on a g
it remained to form the causti
and to observe what took pl
this view I made a paste wit
alicant soda, and 250 gramn
diluted it in water, and left i
perature from 10° to 15° of F
Some bubbles of azotic gas
the result of this last experin
factory, I am not less inclin
gas, whether pure or carburett
works, is owing, as I have
position of water by charc
doubted that the circumstan
tremely different from those
manufactories where large
where the soda employed is
tion, either from containing
in more minute division. In
cumstances which must nece

LVIII. *Cases illustrating t*
in expelling t

C

By John Coakley Lettsc
Medical

Early in September 1809
about thirty-five years of ag
in the abdomen, with dysp
originate from tænia, or tap
had occasionally been evacu

I prescribed a course of

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* *From Transactions of the Med*

cathartics, as recommended by Madame Nouflet. In this plan he persevered for the space of three months, in which period he discharged, at two different times, about eight yards of the tænia. In April 1810, he again applied to me, in consequence of labouring under his former complaints; adding, that he imagined, from the long use of the plant I had recommended, his pains, and particularly the dyspepsia and general debility, had increased.

At this time I ordered the oleum terebinthinæ rectificatum, in a dose of nine drachms by weight, and after it a little honey to remove the heat and unpleasant taste it might occasion. In a week after taking the oil, he called upon me agreeably to my request, and gave me the following information: That, as far as he could judge, in swallowing this medicine, it occasioned less heat than would have resulted from taking the same quantity of brandy, or other spirit; and that the taste and heat it produced were soon removed by the honey.

In about three hours after taking this dose, a laxative motion was produced, without any discharge of tænia: but soon afterwards, with the second stool, more than four yards of the worm were discharged, and also a quantity of matter, resembling, as the patient expressed it, the substance and skins of the tænia. On the surface of this evacuation he noticed the oil floating, together with the tænia and the substance mentioned. It produced little or no pain, and certainly much less than the purgative he had taken after the use of the male fern. The subsequent motions contained no tænia, nor any of the substance before noticed. He experienced no pain or heat in the urinary passages, though the urine continued to impart a terebinthinate smell for three or four successive days.

My patient has since remained in perfect health, enjoying a degree of comfort to which he had been a stranger for the preceding half year.

From this and other instances, I am induced to conclude, that the best method of taking the oil is without any admixture; and that the dose of nine drachms occasions very little inconvenience: and further, that this quantity, perhaps owing to its quick purgative effect, excites no irritation in the urinary passages, although it imparts its peculiar smell to the urine.

I do not recollect that it has been heretofore observed, that the oil has been evacuated in its original state. It might hence be inferred, that it is most efficacious when

exhibited uncombined, in whi
with any particular inconvenie

It is well known that tænia
of the system; and that henc
curately ascertained by any
actual discharge of portions
times, indeed, there is felt a he
attended with dyspepsia and e
pathognomonic symptoms, as
causes.

[To be con

LIX. *Intelligence and*

NATIONAL VACCINE

The Board appointed by Hi
regulate the affairs of this est
the following description of
structions relative to vaccina
sented by the director, be st
nating surgeons.

Description of the reg

When vaccination succeeds
ble on the third day, the da
being reckoned the first. If
tion is felt; and if examined
little tumour appears surroun
cence.

The spot gradually enlarge
sixth day a circular vesicle ap
cine vesicle is elevated, the
is at first of a light pink, som
changes by degrees to a pearl
what darker than the other pa

The vesicle is hard to the t

In its internal structure it is
with transparent lymph.

The vesicle commonly augn
day.

In the early stages, there
inflamed ring; or this takes
day: towards the ninth it s
tenth forms an areola of about

This areola is of the usual
hard, and accompanied with s

It continues out for a day or two, and then begins to fade; sometimes forming two or three concentric circles.

After the areola is formed, the vesicle begins to decline; the centre first turns brown, and the whole gradually changes into a hard smooth scab, of a very dark mahogany colour. This dry crust usually drops off about the end of the third week, leaving a permanent cicatrix.

Varieties in the Progress and Appearance of the Vaccine Vesicle, not preventing the Success of Vaccination.

The first appearance is seldom earlier, but often later than has been described. In some rare instances the vesicle commences even a fortnight or three weeks after vaccination; but if the process is then regular, it is equally efficacious.

When the vesicle is slightly ruptured before the sixth day, if it resume its proper form, and the process continue quite regular, success is not prevented: nor is it, when the crust of a regular vesicle is rubbed off in the decline of the disease.

The irregular and imperfect Vesicle and Pustule, which are not to be depended upon.

In these deviations there is usually a premature itching, irritation, inflammation, vesication, or suppuration. Or the progress of the vesicle is too rapid, its texture soft, its edge not well defined, its centre elevated, and the contents discoloured or purulent. Or instead of a proper areola, a premature efflorescence of a dusky purple hue takes place, and the scab is of a light brown or amber colour.

The irregular vesicle or pustule is more liable to be broken than the other, both from its more pointed form and softer texture, and also from its being usually so irritable as to provoke scratching. When broken, or even without this happening, ulceration often ensues.

A vesicle, apparently regular at first, sometimes does not augment to the proper size, but dies away without completing the regular process. This usually leaves no cicatrix, or one which is almost imperceptible.

When those, or any other considerable deviation from the regular course of the disease take place, no dependence can be placed upon the operation. In such cases vaccination should be repeated.

Probable Causes of irregular Vesicles and Pustules.

These accidents may be occasioned by matter or lymph being taken from an irregular vesicle or pustule at any period, or from a regular vesicle at too late a period; or by

lymph, though originally pure,
long keeping, by heat, or otherw
by performing the operation wit
or in a rude manner, or by inju
stage, and thereby exciting too
terrupting the regular progress
eruptions, and other cutaneous
supposed the cause of these irre
to prevent the vaccine lymph ha

The Methods of taking Vaccir

The lymph of a regular vesi
time it is secreted, till the ar
may therefore commonly be tak
not after the areola is formed.

The lymph is to be taken by
made in the vesicle with the poi
be allowed for the liquid to e
pellucid drops. When requisit
be cautiously applied with the
Great delicacy is requisite in th
sicle is rudely treated, or too n
and ulceration may ensue.

Lymph intended to be used
days, may be received on a lanc
instrument for preserving it lo
rusts the lancet, and it is then
injurious. Quills and toothpic
of ivory shaped like the tootl
pointed, are the most convenie
der them more certain, they sh

In order to preserve lymph, h
method is by two bits of squ
be received on the centre of on
a punctured vesicle. When th
be covered with another bit of
wrapped up in paper or in gold

In whichever way the lymp
lowed to dry without heat in
dry and cool place. When inc
is not taken, it may be injure
wax in sealing the packet.

The Mode of I

Liquid lymph is better than
and the operation is more lig
Therefore, in every instance w

tient from whom the lymph is to be taken should be present, and the lymph should be transferred from the one to the other.

Vaccination is generally performed in the arm near the insertion of the deltoid muscle; but in order to hide the scar, and in adults who are likely to use the arm much, it may be adviseable to vaccinate the outside of the leg, a little above or below the knee.

The skin being stretched, a lancet charged with vaccine lymph should be held with its flat surface to the skin; and the point insinuated slantingly through the cuticle till it touches the cutis. It should be retained there for a few seconds.

The lancet should be dipped in water and wiped after each operation, even when several successive inoculations are to be performed.

Dry lymph on glass may be moistened with a very little cold or tepid water on the point of a lancet, allowing it some time to dissolve, and blending it by a little friction with the lancet. It must not be much diluted, but of a thick consistence: it is to be inserted in the same manner as the recent fluid.

When quills, ivory lancets, or toothpicks charged with dry lymph are used, the lymph should not be diluted; but a puncture having been first made with a common lancet, the point of the instrument is to be inserted, and held in the puncture half a minute or more, that the lymph may gradually dissolve and remain in the wound. If the part of the instrument which is charged be afterwards wiped repeatedly upon the edges of the puncture, it will tend still further to ensure success.

Vaccinated patients must be cautious not to wear tight sleeves, nor to injure the vesicle by pressure, friction, or any other violence; lest considerable inflammation or ulceration should ensue.

One perfect vaccine vesicle is sufficient; but for various reasons it is always prudent to make two or three punctures, especially when the danger of receiving the small-pox is imminent, the lymph dry, or the patient's residence distant. Besides, greater security is obtained against a chance of failure from the derangement or destruction of one vesicle by accidental injury, or by the taking of lymph for vaccination. When two punctures are to be made in one limb, they should be at least two inches asunder, on account of the irritation they may occasion.

One vesicle should be always permitted to go through its course without being punctured.

Lancets for vaccination should be kept clean and bright.

Constitutional

Constitutional symptoms son
eriod, but more commonly fr
enth day. These are drowsin
ıcceeded by heat, thirst, hea
brile affection. Now and the
lace, especially in infants.

The constitutional symptom
transient.

In a great proportion of case
disposition; nevertheless, the
h the future infe
ess of the vesicle

be taken not t
with those produ

Medical T

o medicine is re
nptoms happen
e remedies are t
ıy other cause.
ory medicines are
no cathartics n
local inflammati
ıappens, unless
may soon be che
compresses of linen
Plumbi Acetatis dilutus, or in
Plumbi Superacetas in a pint
applied cold.

If the scab be rubbed off pre
place, cooling and astringent a
as a drop of liquor Plumbi Acet
to dry on the part, and then
dipped in water, or in either
above-mentioned, and frequen

When ulceration is deep or
bread and milk, or of bread wi
lead may be applied, as the
must never be applied till they

In such foul and obstinate
applications, the Unguentum
with an equal quantity of Ung
milar applications, may someti
tage. And at other times thes
Ceratum Plumbi Superacetatis

The irregular vesicles and pustules are frequently followed by ulceration at an early period, which is to be treated in the same manner as if it proceeded from the regular vesicle.

When the patient has been previously exposed to the infection of small-pox, this disease will be either superseded or not, according to the time which has elapsed before vaccination.

Medical gentlemen in all parts of the empire may be supplied with vaccine lymph, without any expense, from the National Vaccine Establishment.

Applications for lymph, letters, and communications respecting vaccination, will meet with proper attention: they should be addressed to Dr. Hervey, register, Leicester-square; and when from a distance, put under a cover, directed to The Right Hon. the Secretary of State for the Home Department.

Board Room, 21, Leicester Square, Feb. 22, 1810.

MOUNT VESUVIUS.

Naples, Sept. 24.

The recent eruption will make the year 1810 an epoch in the annals of Vesuvius, on account of the manner in which it began, and the disasters it has produced. It is considered as a very extraordinary circumstance that this eruption was not preceded by the usual indications; every convulsion of Vesuvius being previously announced by the drying up of the wells of Naples. This phænomenon did not take place on this occasion, and, to the great surprise of the inhabitants, Vesuvius began to emit flames on the night of the 10th of September.

On the morning of the 11th the flames became more intense, and the lava began to flow from the east and south-east sides of the mountain. Towards evening the conflagration increased, and about twilight two grand streams of fire were seen to flow down the ridge of the volcano; night produced no change in this state of things.

On the morning of the 12th a hollow sound was heard, which continued increasing; the fire and smoke also augmented in intensity, and towards evening the horizon was obscured. The breeze, usual in these parts, having blown from the south-east, dissipated the accumulated clouds. The mountain continued to vomit lava and a dense smoke, which even at a distance was strongly sulphureous; the hollow noise in the sides of the mountain continued to increase.

Curious to witness as near
astonishing phænomena of nat
fortune of Pliny, I set out fro
evening I reached Portici. Fr
the mountain the road is lon
way there is a hermitage,
refuge and shelter to the tra
there fixed his residence, an
a moderate sum, refreshments,
veller are worth their weight
this hermitage produce the fa
Christi.—From the hermitage
is a long quarter of a league
in order to reach from thence
climb a mountain of cinders,
up to the mid-leg. It took m
our guides, two hours to mak
ready midnight when we reac

The fire of the volcano serv
had totally ceased for two hour
derably decreased:—these circu
curity, and supplied us with
traversing such dangerous gr
near as the heat would permit,
of our guides in the lava, wh
hollows from the crater. The s
nearly resembles metal in a sta
it carries a kind of scum, wh
then forms masses of scoria, w
and roll all on fire, with noise,
Strong fumes of sulphuric acid
these scoria, and by their caus
render respiration difficult.

We seemed to be pretty secu
far from thinking of retiring,
which launched into the air fr
the distance of more than 10
danger to which we were expo
a moment in embracing a ret
cleared in our descent a spac
taken two hours to climb.

We had not reached the he
frightful than ever was heard,
fury, began to launch a mass
loads of stones and fragment
projectile force which it would
the projection was vertical, al

ing mass fell back again into the mouth of the volcano, which vomited it forth anew to receive it again, with the exception of some fragments which flew off, to fall at a distance, and alarm the inquisitive spectator.

The 13th commenced with nearly the same appearances as those of the preceding day. The volcano was tranquil, and the lava ran slowly in the channels which it had formed during the night; but at four in the afternoon a frightful and continued noise, accompanied with frequent explosions, announced a new eruption: the shocks of the volcano were so violent, that at Fort de L'Œuf, built upon a rock, where I then was, at the distance of near four leagues, I felt oscillations similar to those produced by an earthquake.

About five o'clock the eruption commenced, and continued during the greater part of the night. This time the burning matter flowed down all the sides of the mountain, with a force hitherto unprecedented; all Vesuvius seemed on fire. The lava has caused the greatest losses: houses and whole estates have been overwhelmed; and at this day families in tears and reduced to despair, search in vain for the inheritance of their ancestors, buried under the destroying lava.

At ten at night the hermitage was no longer accessible; a river of fire had obstructed the road. The districts situated on the south-east quarter of the mountain had still more to suffer. Mount Vesuvius presented the appearance of one vast flame, and the seaman at a great distance might contemplate at his leisure this terrific illumination of nature.

SERPENTS IN AMERICA.

The following is translated from the Reading (Pennsylvania) Eagle.—A daughter of Mr. Daniel Strohecker, near Orwigsburg, Berks county, Pennsylvania, about three years of age, had been observed for a number of days to go to a considerable distance from the house with a piece of bread which she obtained from her mother. The circumstance attracted the attention of the mother, who desired Mr. S. to follow the child and observe what she did with it. On coming to the child he found her engaged in feeding several snakes, called yellow heads or bastard rattle-snakes. He immediately took it away, and proceeded to the house for his gun, and killed two of them at one shot, and another a few days after.—The child called these reptiles in the manner of calling chickens; and when its father observed, if it continued the practice they would bite her—the child replied—

" No, father, they won't bit
give them."

NATIVE M

Although magnesia enters i
mineral substances, yet its exi
dom, in an uncombined state
years, been unknown.

At Hoboken, in New Jersey
Stevens, is found a mineral wh
ments of Professor Bruce, of
hundred parts,

Magnesia ....
Water of crystallizati

SHOWER OF METEORIC STO

Raleigh,

On Tuesday, the 30th of J
P.M. there was a fall of meteo
Their descent was seen for a
and two reports distinctly hear
of 30 miles.—A fragment we
quarters struck a tree in the n
near where some woodcutters w
hending the fate of Sodom and
out once looking behind them.
a woman, whose curiosity was
returned with her to the place,
which was still hot. We underst
of the Mississippi territory, no
sending it to the Chemical S
analysed: it is, he informs us
porous, and probably contains i

COFFE

A foreign journal announces
manufacturer in the departmen
has succeeded in growing cof
some Mocha coffee, and obtai
pounds of beans possessing th
Perhaps the most important c
experiment was his neither em
glass frames, nor any unusual sl
the soil with some care.

DE LUC'S ELECTRIC COLUMN.

We have again to notice the ringing of the small bells by means of De Luc's electric column. They were ringing on the evening of the 24th of August, and had been so doing, without being observed to have stopped, for a period of 152 days and a half. This long continuance renders it not improbable that (as was suggested in our Magazine for March) "the weight of the clapper may be so adapted to the power of the apparatus, as to cause small bells to continue ringing for several years without intermission."

If any of our mechanical readers can suggest to us an easy method, by which a pendulum vibrating can give motion to wheel-work, they are requested to communicate such contrivance to us. It is much wished that an instrument may be made, which by the motion of an index hand and dial-plate may show the number of vibrations in a given time, as it would be very interesting to observe what alterations may take place in different states of the atmosphere.

LECTURES.

Middlesex Hospital.

Medical Lectures, 1810-11, by Richard Patrick Satterley, M.D. Fellow of the Royal College of Physicians, Physician to this Hospital, and to the Foundling Hospital; and Thomas Young, M.D. F.R.S. Fellow of the Royal College of Physicians.

Dr. Satterley's Course of Clinical Instruction will begin the first week in November: the attendance on the Patients will be continued daily, and Lectures will be given once a week, or oftener when it may be necessary, at Eleven o'Clock.—Mr. Cartwright, Surgeon to the Hospital, will undertake such occasional demonstrations of morbid anatomy as may be required for the illustration of the respective cases. The objects of the Course will also be extended to such remarkable peculiarities in the diseases of Children, as may occur in the Foundling Hospital. Terms of admission, to Pupils of the Hospital, Five Guineas.

Dr. Young will begin, in February, a Course of Lectures on Physiology, and on the most important parts of the Practice of Physic; in particular the Nature and Treatment of Febrile Diseases: he will deliver them on Tuesdays and Fridays, at Seven o'Clock in the Evening. Admission, Two Guineas: to former Pupils, One Guinea.

Those who are desirous of attending either of these Courses, are requested to leave their names with the Apothecary at the Hospital, from whom further particulars may be known.

Electrical and Elect

Mr. George Singer will co
on Electrical Phænomena, co
coveries, and illustrated by nu
Early in the ensuing season
Instruction may be had of
street; or of Mr. Singer, at the
Cavendish-square.

Surry In

The Annual Courses of P
Institution, Blackfriars Bridge
and will continue every succe
Evening, at Seven o'Clock,
lowing Gentlemen have been
partments, viz.

Zoology ....
Music ....
Zoonomy ....
The Chemistry of the Arts
Natural Philosophy and Astronomy ....

LIST OF PATENTS FO

To Thomas Norris, late of
for his new mode of sheathing
ships or vessels with certain
be a substitute for copper.—S

To Samuel Hobday, of Wo
of Aston, near Birmingham,
by the application of which al
rack, he can make snuffers t
tember 26.

To Marck Isambard Brunel,
sequence of a communication
reigner residing abroad, is bec
for giving motion to machine
plicable to hydraulic and pneu

To Benjamin Milne, of Bridli
collector of the customs, for a
alarm.—Oct. 1.

To Joseph C. Dyer, of Bos
one of the United States, nov
chant, in consequence of a c
by a certain foreigner residing
sessed of certain improveme

method of using plates and presses, and for combining various species of work in the same plate for the kind of printing usually called plate printing, designed for the objects of detecting counterfeits, for multiplying impressions, and saving labour.—Oct. 1.

To George Miller, of Panton-street, near the Haymarket, musical instrument-maker, for his method of making wind instruments commonly called military fifes, of substances never before used for that purpose.—Oct. 1.

To John Towill Rutt, of Goswell-street, in the county of Middlesex; John Webb, of Hoxton, in the said county; and John Tretton, of the city of London, card manufacturers, for their improved apparatus to machines for making fillet, sheet, and hard cards, such as are used for carding wool, cotton, flax, silk, and all substances capable of being carded.—Oct. 8.

To Ebenezer Parker, of Highfield, in the parish of Sheffield, in the county of York, silver-plater; and Francis Cleeley, of Sheffield aforesaid, surgeons' instrument-manufacturer, for their method or plan of making an adjusting bedstead on a double frame with a four-fold method, for the relief of sick, lame, infirm and aged persons.—Oct. 8.

To John Hazledine, of Bridgenorth, in the county of Salop, engineer, for his manifest improvements in the construction of a plough for the cultivation of land.—Oct. 8.

To George Hodson, of the city of Edinburgh, North Britain, ash-manufacturer, for his improved method of separating the alkaline salt from the acid as it exists in the following substances, viz. kelp, black ashes, soaper's salts, spent leys, sosa natrose, rock salt, common salt, brine, sea water, caput mortuum of aqua-fortis, caput mortuum of oil of vitriol, and caput mortuum of salt used by bleachers, being on a principle entirely new.—Oct. 8.

To Charles Francis, of Phœnix Wharf, Nine Elms, in the parish of Battersea, Surry, temper lime-burner; and William Waters, of Princes-street, in the parish of St. Mary, Lambeth, Surry, potter, for their improved method of joining pipes.—Oct. 8.

To Henry Stubbs, of Piccadilly, in the county of Middlesex, blind-maker, for his new grand imperial aulæum, from three to 18 or 20 feet wide, without seam, and to any length or colour, for decorating the most superb or useful room, for such as drapery, curtains, and fringes, chairs, sofas, tables, &c. or finished on one side only for ornamental hangings, borders, and every other species of decoration.—Oct. 8.

METEOROLO

By Mr. Carey,

For Oct

Days of Month.	Thermometer.			H the I
	8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	
Sept. 27	53	68°	54°	2
28	55	68	56	
29	51	66	56	3
30	56	64	57	2
October 1	58	64	51	3
2	55	63	52	
3	50	64	51	
4	52	64	52	
5	51	64	52	
6	52	61	50	2
7	48	66	55	
8	53	65	56	
9	54	61	55	
10	55	59	54	
11	52	59	48	
12	47	57	44	
13	40	57	43	3
14	46	56	46	
15	46	57	47	
16	42	56	55	2
17	57	62	56	
18	56	64	55	
19	50	59	58	
20	56	64	49	
21	50	59	56	
22	57	61	48	
23	49	55	44	
24	46	52	41	
25	42	50	40	30
26	39	49	44	

N. B. The Barometer's hei

ERRA

Page 231, line 26 from the top, Hernia,)—for *turned* read *traced*.

LX. *Description of a Camp Telegraph, invented by* Knight Spencer, *Esq. Secretary to the Surry Institution.*

To Mr. Tilloch.

Sir, The important advantages resulting to the naval service from the introduction of the telegraph by Sir Home Popham, now universally adopted, are too well known to be here insisted upon.

That telegraphic signals have been productive of great advantages to land armies, for more than 3000 years, is very easily proved.

That *the most important advantages have resulted* to the French arms, from the use of the telegraph, in the present age, is too well authenticated to be doubted.

That commanders of British armies have felt the absolute necessity of adopting some mode of telegraphic communication, is proved by the late campaign in Sicily, and, the present campaign in Spain.

That many attempts have been made to introduce the telegraph into our land-service universally, cannot be questioned.

To what cause, then, is it to be attributed, that to the present moment this powerful instrument remains to British armies (generally speaking) nearly a useless invention?

The only *rational* answer to this question seems to be, that, hitherto, no practicable system has been offered, and the attempts to introduce it must, probably, have failed;—either, from the intricacy of the machines, or, the difficulty of transporting them into situations where they could be used.

Whatever cause may have hitherto retarded its introduction, it will hardly for a moment be contended, that, were a telegraph produced as certain in its operations as the present fixed telegraph, and at the same time so simple and portable as to require no separate establishment, either for its transport or management, it would not be a most important acquisition in the field.

With this conviction on my mind, I have endeavoured to obviate the supposed difficulties; and the result, which I call my Camp Telegraph, I request permission to lay before the public through the medium of your respectable Magazine;—indulging the hope, that it may meet the attention of those who have sufficient influence to bring the subject fairly under the consideration of his majesty's government,

vernment. Perhaps it may
my invention has already b
bation of several general an
forming correct opinions on
frequently asked a question
miles, and have received an
Any officer of ordinary capac
application, to direct a stati
the duty of a signal-man at
the apparatus not being mo
pike, there seems no necessi
blishment to manage it.

EXPLA

To work the Camp Teleg
director of each station mus
or others, to be called signa
furnished with a staff 13 or
be mounted two flexible ball
described below:—this is
other two signal-men must
ten feet high, mounted with
The signals must be mad
men taking an ordered num
of the centre-point; in th
takes his stand, during the ti
All signals must be made
station, who must give the
of paces. These are to be
double-quick time, carrying th
they have arrived at the poin
must be instantly elevated.

All signals must be repea
tion: and when the directo
communication, observes th
"*Down*," and his signal-m
quick time to the rear of th
balls at the trail. The wor
given by the director of the
tion, the instant he observe
sponding station begin to re

A. (Plate VIII.) Is the
made by placing one of the
right, and the other at 20 p
point.

B. Is the signal of a *point or period*, and is to be made at the close of a number, as 275, by placing one signal-man three paces to the right, and the other three paces to the left, of the centre-point.

C. Is the signal of *error*, and is to be made when your correspondent has mistaken your last signal:—Suppose you had made the signal No. 2, which is 20 paces to the right, and your correspondent answers with 20 paces to the left, which is the signal No. 7. Then make the signal of error, by placing one signal-man three paces to the left, and the other 10 paces to the right of the centre-point; and when your correspondent has repeated this signal, thereby convincing you he is sensible of his error, *repeat the signal that had been mistaken*, and, if rightly answered, proceed as before.

D. Is the *repeating* signal, and is to be made if the last communication is not understood. It is made by placing one signal-man three paces, and the other 20 paces, to the left.

NUMERALS.

No. 1. Is made by placing one signal-man three paces to the *right* of the centre-point.

2. By placing one signal-man 20 paces to the right.

3. By placing one signal-man 10 paces, and one 20 paces, to the right.

4. By placing one signal-man at three, and one at five paces, to the right.

5. By placing one signal-man at 18, and one at 20, paces to the right.

6. By placing one signal-man three paces to the *left* of the centre-point.

7. By placing one signal-man 20 paces to the left.

8. By placing one signal-man 10, and one 20, paces to the left.

9. By placing one signal-man at three, and one at five, paces to the left.

0. By placing one signal-man at 18, and one at 20, paces to the left.

The flexible ball is cons
ner:

Take an ash or deal staff
substance of a stout pike.
feet six inches long, and fi
top of the staff, in the way t
fixed:—fix the lower ends o
slide (like the slide of an un
be 18 inches long, and pr
this pipe, stretchers 18 inc
also to the middle of each w
an umbrella, to keep the bal
then be a strong umbrella-s
feet from the upper fastenin
the ball, so that, when the
bones will form a sphere of

The skeleton of the ball
covered with glazed linen,
vided *vertically*. Letter G
the ball, but only showin
twelve. When the balls ar
sprung, and covered with st

SIGNALS

To make these, it will
inches square and 12 inches
the other, at the distance of
tre-point: and one lamp
on the top of the ball-staff.

Each lamp must have two
diameter, filled with differen
(say pale green and pale red)
from common lights. They
put through a strong iron fr
sign which is fixed upon an
the staff is raised they wil
when they are carried at the
pendicular position.

The reservoir for the oil
agitable lamps; the wicks
broad.

E. is a front view of the

F. is a side view of the

A code of numerical sign

applicable to the land service, arranged upon the plan of Sir Home Popham's for the naval service, will be necessary.

When a tent or any other object is fixed upon as a centre-point, it is then generally unnecessary to use the double ball.

When stations are taken *below the horizon*, the white sides of the balls are to be turned to your correspondent, and it is advantageous to have the men in white or fatigue dresses.

When stations are taken *above* the horizon, the black sides are to be turned towards your correspondent, and then it is advantageous to have the men in uniform.

I am, sir,
Your obedient servant,
KNIGHT SPENCER.

Surry Institution,
Nov. 6, 1810.

LXI. *On the Penetration of Balls into uniform resisting Substances.* By W. MOORE, *Esq.*

To Mr. Tilloch.

SIR, SHOULD the following paper on the destruction of an enemy's vessel at sea by artillery be thought deserving a place in your excellent Magazine, you are at liberty to make use of it accordingly.

I am, sir,
Your most obedient servant,
W. MOORE.

Royal Military Academy,
Woolwich, November 10, 1810.

LEMMA I.

If two spheres of different diameters and different specific gravities impinge perpendicularly on two uniform resisting fixed obstacles, and penetrate into them; the forces which retard *the progress of the spheres will be as the absolute resisting forces or strengths of the fibres of the substances directly, and the diameters and specific gravities of the spheres inversely.*

Let R and r denote the absolute resisting forces of the two substances; F and f the retardive forces; D, d the diameters of the spheres; Q, q their quantities of matter, and N, n their respective specific quantities. Then the whole resistances to the spheres, being by mechanics proportional to the quantities of motion destroyed in a given time, will be as the absolute resisting forces of the sub-

stances and quantities of resi
the resisting forces of the s
diameters of the impinging sp

But in general $\frac{M}{m} = \frac{F}{f} \times$
two values of the whole resis
$\frac{Q}{q} = \frac{R}{r} \times \frac{D^3}{d^3}$, and $\frac{F}{f} =$
$\frac{q}{Q} = \frac{d^3}{D^3} \times \frac{n}{N}$, it is $\frac{F}{f} =$
$\frac{d}{D} \times \frac{n}{N}$: that is, the forces
uniform resisting substances
of the fibres of the substance
and specific gravities of the s

LEM

The whole spaces or depth
on different resisting substanc
of the initial velocities, the di
of the spheres directly, and t
sisting substances inversely ;

For by mechanics we hav
preceding lemma $\frac{f}{F} = \frac{r}{R}$:
in the above it becomes $\frac{S}{s} =$

These being premised, I n
lowing most important

PROP

To find a general formula
tity of charge for any given p
greatest destruction possible t
being supposed of oak substan
distance not affecting the init

By Lemma 2, we have, ge
$\frac{R}{r}$. Also the charges of po

velocity and weight of ball jointly*. Hence, since it has been determined from experiment that a charge of half a pound impelled a shot weighing one pound with a velocity of 1600 feet per second; we shall, considering V the velocity of any ball impinging on the side of the vessel, have for the expression of the charge impelling it through the space $S = \frac{SdnRv^2w}{2DsNr \times \overline{1600}|^2}$.

Now to apply this in the present instance, it is first necessary that a case be known concerning the penetration of a given shot into oak. Such a case is presented at page 273 of Dr. Hutton's Robins's New Principles of Gunnery. It is there asserted that an 18-pounder cast-iron ball penetrated a block of well-seasoned oak, such as ships of war are generally built with, to the depth of 3½ inches when fired with a velocity of 400 feet per second. Making, therefore, this the standard of comparison for all cases where the object is of oak substance, we shall have for the charge *generally* $\frac{400^2 \times \cdot 42}{2 \times 1600^2 \times \frac{7}{24}} \times \frac{SnRw}{DNr}$; or, because the balls are of the same specific gravity, and the substance the same, or $R = r$, and $N = n$; it will be $\frac{400^2 \times \cdot 42}{2 \times 1600^2 \times \frac{7}{24}} \times \frac{8w}{D} = \cdot 045 \times \frac{Sw}{D}$; that is the charge varies as the space to be penetrated and weight of ball directly, and diameter of the ball inversely.

But the charge by the question being to produce the greatest effect possible in the destruction of the vessel; S in the above formula must always be put equal to the given thickness of its side *plus the radius of the ball;* since it is well ascertained that, for a shot to produce the most damage to any splintering object, such as oak; it must lose all its motion just as it quits the superior or further surface of it.

Hence the charge in question is $= \cdot 045 \times \frac{(S' + \frac{1}{2}D)w}{D}$. S' being the thicknes of the side of the vessel, w the weight of the ball, and D its diameter.

We have supposed, that the resistance opposed to the ball's motion is uniform throughout the entire penetration;

---

* This law of variation of the charges does not exactly obtain in practice after a certain charge, on account of the definite lengths of the guns: but it is presumed the deviation from it, if known, would not materially affect our results.

which is not strictl
partly on the quant
tinually varies until
its radius; when it c
further surface of th
commences its variat
are about sufficient to
of the charges before
falling somewhat sh
certain charge.

An enemy's ship i
42-pounder guns to
as possible when th
other: the side of th
1¼ foot thick of oak

The diameter of a

foot; we get ·045

14ozs. for the weigh

Containing the vario
36- and 42-poun
effect in all cases
of oak materials,
dius of the ball
increasing by 1 in

Nature of Ordnance.	Thic
	12 Inc
Pounder 12	lbs. 1·4
18	1·928
24	2·330
32	2·830
36	3·061
42	3·393

Nature of Ordnance.	Thickness of the Side of the Vessel, plus the Radius of the Ball.			
	16 Inches.	17 Inches	18 Inches.	19 Inches.
Pounder	lbs.	lbs.	lbs.	lbs.
12	1·918987	2·838926	2·158863	2·278800
18	2·571428	2·732142	2·892856	3·053571
24	3·115533	3·310254	3·504975	3·699696
32	3·773960	4·009833	4·245705	4·481578
36	4·082173	4·337310	4·592445	4·847581
42	4·524240	4·806905	5·089770	5·372535

	20 Inches.	21 Inches.	22 Inches.	23 Inches.
	lbs.	lbs.	lbs.	lbs.
12	2·398737	2·518674	2·638612	2·758547
18	3·214285	3·374999	3·535714	3·696428
24	3·894417	4·089137	4·283859	4·478580
32	4·717350	4·953323	5·189195	5·425068
36	5·102717	5·357853	5·612988	5·868124
42	5·655300	5·938065	6·220830	6·670202

	24 Inches.	25 Inches.	26 Inches.	27 Inches.
	lbs.	lbs.	lbs.	lbs.
12	2·878484	2·998420	3·118358	3·238292
18	3·857142	4·017856	4·178570	4·339284
24	4·673300	4·868021	5·062741	5·257463
32	5·660940	5·896813	6·132685	6·368559
36	6·123260	6·378396	6·633531	6·888668
42	6·786360	7·069125	7·351890	7·634655

Nature of Ordnance.	Thickness of the	
	28 Inches.	29 I
Pounder 12	lbs. 3·358228	l 3·4
18	4·521340	4·6
24	5·452184	5·6
32	6·504432	5·8
36	7·143804	7·3
42	7·917420	8·2

	32 Inches.	33 I
12	lbs 3·837972	l 3·9
18	5·164196	5·3
24	6·231068	6·4
32	7·547924	7·7
36	8·164348	8·4
42	9·048480	9·3

	36 Inches.	37 I
12	lbs. 4·317716	l 4·4
18	5·807052	5·9
24	7·009952	7·2
32	8·491410	8·7
36	9·184892	9·4
42	10·179540	10·4

Nature of Ordnance.	Thickness of the Side of the Vessel, plus the Radius of the Ball.											
	40 Inches.	41 Inches.	42 Inches.	43 Inches.	44 Inches.	45 Inches.	46 Inches.	47 Inches.	48 Inches.	49 Inches.	50 Inches	51 Inches.
Pounder	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs	lbs.	lbs.
12	4·797460	4·917396	5·037332	5·157268	5·277204	5·397140	5·517076	5·637012	5·756948	5·876884	5·996820	6·116756
18	6·449908	6·610622	6·771336	6·932050	7·092764	7·253478	7·414192	7·574906	7·735620	7·896334	8·057048	8·217762
24	7·788836	7·983557	8·178278	8 372999	8·567720	8·762441	8·957162	9·151883	9·346604	9·541325	9 736046	9·930767
32	9·434908	9·670781	9·906654	10·142527	10·378400	10·614273	10·850146	11·086019	11·321892	11·557765	11·793638	12·029511
36	10·204436	10·460572	10·715708	10·970844	11·225980	11·481116	11·736252	11·991388	12·246524	12·501660	12·756796	13·011932
42	11·310600	11·593365	11·876130	12·156895	12·441660	12·724425	13·007190	13·289955	13·572720	13·855485	14·138250	14·421015

Nature of Ordnance	Thickness of the
	52 Inches.
Pounder 12	lbs. 6·236692
18	8·378476
24	10·125488
32	12·265384
36	13·267068
42	14·703780

	55 Inches.
12	lbs. 6·596500
18	8·860618
24	10·709651
32	12·973003
36	14·032476
42	15·552070

	58 Inches.
12	lbs. 6·956308
18	9·342760
24	11·2938 4
32	10·680622
36	14·797884
42	16·400370

In this Table the first column contains the nature of the ordnance, and the numbers in the other columns are their respective charges of gunpowder in pounds, when the thickness of the object to be destroyed is as specified at the top of the columns. If the thickness be given in inches, and parts of inches, take such parts of the difference between the charge for the given number of inches and the next greater; and add them to the charge first found for the given number of inches for the charge required.

The value of the decimal part of each will be had by multiplying it by 16, the number of ounces in a pound, and pointing off in the product from the right hand towards the left, as many places for decimals as are contained in the given decimal, and retaining the number on the left of the point for the ounces, increasing it by ¼, ½, ¾, or 1, when the first figure of the decimal is 2, 5, 7 or 8 respectively. This hint is merely given for those practitioners who may not be very conversant in decimals.

SCHOLIUM.

This question is not only of the utmost importance and practically useful in naval engagements, but in several instances also of military operations; as the bursting open gates of besieged cities with promptitude and effect, and breaking up all fortifications composed of wooden materials; especially those of a splintering nature, to which the above charges apply most correctly. In the case of a naval action where the object to be penetrated is of oak substance, the ball by having a small motion when it quits the ship's side tears and splinters it excessively, breaking away large pieces before it, which are not so easily supplied in the reparation: whereas on the other hand, if the shot had any considerable velocity when it quitted the side, the effect produced would be merely a hole, which would be stopped instantly by the mechanic employed for that purpose; and indeed in a great measure by the springiness of the wood itself; for I have seen in his majesty's dock-yard at Woolwich, captured vessels having a number of shot-holes in them, almost entirely closed by the wood's own efforts; and that required nothing more than a small wooden peg or a piece of cork to stop them up perfectly: all the damage, therefore, the shot can do under such circumstances of swift celerity is merely killing those men who may chance to stand in the way of their motion.

If any object to be destroyed be so thick that it cannot be completely pierced by any common engine; or if it be

of a very brittle nature, such
charge is to be used which will
the shot to produce the maximu
of bombardment this charge is l
for though the effect produce
greater, yet in any considerable
be less than that from a smalle
count of the extreme heat it wo
a few discharges; and in conseq
would be required for cooling t
further service.

EXAMPL

Required the charge for a 2
the gates of a city with the gre
ing of elm *one foot thick.*

Here the object to be penet

letters in the general formula $\frac{}{21}$
must be made to denote the se
periment made in the penetration
therefore, the experiment of I
5th problem of his elegant Ex
$d = \frac{1}{6}$ ft. $v = 1500$, and $S = $

$S = 1$ ft. $D = {\cdot}46$, and $w = 24$

$$= \frac{1{\cdot}23 \times \frac{1}{6} \times 1500^2 \times 24}{2 \times {\cdot}46 \times \frac{13}{12} \times 1600^2} = \frac{830{\cdot}25}{191{\cdot}36}$$

$5\frac{1}{2}$ ozs. nearly the weight of the c

Retaining the experiment of I
all cases where the substance to

shall have by reduction $\frac{(S' + \frac{1}{2}D)}{2Ds \times 1}$
the charge for any piece the di
and weight w; S' being the thic
fore.

It is not unworthy of remark,
place, or any like things, might
by the gun itself, charged only v
close to the gates with its mu
mentum of recoil being general
objects completely.

LXII. *Cases illustrating the Effects of Oil of Turpentine in expelling the Tape-worm**.

Case II.

By Thomas Hancock, *M.D.F.M.S., Physician to the Finsbury Dispensary.*

[Concluded from p. 308.]

I have used the ol. terebinth. in only one case of tænia. Jane Woodward, a poor woman, about 45 years of age, first applied to me at the London Electrical Dispensary, some months ago. She had been for more than seven years afflicted in a very distressing manner with this complaint, and was four times a patient in different hospitals; where, by the use of active remedies, she obtained temporary relief from pain, and frequently voided large portions of the tænia per anum. So soon as she had recovered a little strength, by indulging her appetite, after the violent operation of purgatives in these hospitals, her abdomen began to increase considerably in size, and small detached portions of tænia, about an inch or more in length, apparently endowed with life, continued to pass at times through the rectum; so that she was prevented from earning her bread, by this very distressing circumstance. She had generally recourse to purgatives on these occasions, and their operation had regularly the effect of reducing the size of the abdomen; but her disease continued. I may also observe, that, after these courses of medicine, she had less of rumbling in the intestines, and felt less pain, than when she freely indulged her appetite; for then, to use her own expression, "the worms appeared to gain strength," according to the increase of her own strength.

About two weeks after the application of electrical sparks to the abdomen, she discharged a portion of tænia, seven yards in length, without any appearance of head, which lived in cold water nearly three hours after its expulsion. Mr. Chamberlaine informs me he has known the tænia live nearly as long in water which was much above the temperature of the human body; a sufficient proof of the extraordinary tenacity of life in this animal.

Electricity was continued for some weeks longer; but as her pains also continued, and no more of the worm came away, my friend Mr. Chamberlaine kindly offered to try the

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* From *Transactions of the Medical Society of London*, vol. I. part I. just published.

effect of his electuary of the
tient took this at first witho
scobs stanni in large quantitie
but though, as she asserted, th
she had ever taken, relieved
no discharge of tænia.

I now heard of the ol. ter
in this complaint, and resolv
cially as my patient was herse
medy that promised the slig
I may premise, that her abdom
her stools slimy, and, in shor
that she had still large portio
I ordered her at first small dose
twice a day, mixed with treac
produced no other effect than
easiness, and particularly on g
the rectum. The dose was no
at longer intervals. The first
she took without treacle, pro
confusion of ideas, and afterwa
complained of no uneasiness
gans. After these doses, she pa
mucus, with such relief in al
she earnestly entreated I woul
dose. The quantity of an oun
ways produced a great degree c
toxicated, which came on shor
tinued for an hour or more, ur
which followed, removed it.

Although this medicine was
every two or three days for a fe
particular desire, there was nc
stools. I could not, however,
which was discharged so abu
the ol. tereb. sometimes exhil
films, as if the substance of
down. She took the very larg
two or three times, after the
effect, with results similar to t
short, by her own account, vic
thing that relieved her; and a
medies, as well as nourishin
her sufferings, so that she ab
appetite craved it, in order to a

I have since heard that she we

and had again taken the ol. tereb.; for I strongly advised her to discontinue its use some time before she left the Finsbury Dispensary, having lost all hopes of its ultimately curing her.

CASE III.

By SAMUEL FOTHERGILL, *M.D.F.M.S., Physician to the Western Dispensary.*

A soldier, aged about 40, applied to the Western Dispensary, the 28th of October 1809. He stated that he had been subject to tape-worm during the last four years, previously to which he had served with the army in Egypt, and attributed the origin of his complaint to the badness of the water which he drank in that country. He is now a private in the Middlesex militia. He complains of gnawing pains in the abdomen, irregular appetite, debility, and anxiety. He is somewhat emaciated, and his complexion is rather sallow. Whilst with his regiment, he had occasionally taken, by order of the surgeon, a variety of worm-medicines, and small pieces of tape-worm were passed at times; they even sometimes came away alive, without medicine having been taken, and without a stool.

I directed him to take pulv. scammon. cum calomel. ℈j. every third morning. Two doses operated freely, but only a few very short pieces of tape-worm were brought away. I now directed him to take half an ounce of the oil of turpentine. He took it as ordered, November 9, in a little tea, sweetened with honey. In a quarter of an hour he was seized with retching, and in the course of the day passed four copious stools, in one of which was a tape-worm of several yards in length. The portion which the patient brought me I found measured four yards; he threw the smaller pieces away; but thought that altogether the length might be ten yards. The worm was dead, and had a livid appearance: the patient remarked that the pieces which formerly passed from him were of a whiter colour and brighter aspect.

The dose of the medicine was increased to six drachms, and was repeated twice a week for the space of a month. During the first fortnight small pieces of worm continued to pass away, both after taking the medicine and at other times; but in the second fortnight the stools were natural, and contained no vestige of tænia. The remedy was consequently discontinued; and the man called some weeks afterwards to acquaint me that he had remained entirely free from all symptoms of his complaint, and had regained his strength and cheerfulness.

He was generally a little sick after taking the medicine, and for a day or two was affected with a severe pain in the back part of his head, but complained of no other unpleasant effects from its use.

CASES IV. and V.

By GEORGE BIRKBECK, *M.D.F.M.S., Physician to the General Dispensary.*

Dr. Birkbeck stated to the Society that he had administered the oil of turpentine, successfully, to two middle-aged females who had long been troubled with the tape-worm.

In the first case half an ounce was given: no unpleasant sensation occurred whilst it was swallowed, but considerable confusion of ideas and vertigo, with a slight degree of nausea, were soon produced. In a short time a discharge from the bowels took place; this was quickly followed by another, with which more than four yards of the worm were evacuated. The patient, in consequence of the frequent spontaneous escape of small portions of tænia, and the expulsion of a larger quantity about twelve months before by an active purgative medicine, had an opportunity of comparing the ordinary appearance of the worm with that which it now presented. Instead of being whitish, smooth, full, and in motion, she represented it to be dark-coloured, shrivelled, filmy, and lifeless. A second dose of the oil did not expel any more of the worm, nor, when he last saw her, about three months afterwards, had it again appeared. In that interval she had not been disturbed by any of the unpleasant feelings to which she was before subjected.

Considerable derangement of the general health and great pain in the pit of the stomach were produced by the tape-worm, in the second case in which the oil of turpentine was employed. Although one tea-spoonful only was introduced, sickness and acute pain followed: this dose was repeated several successive mornings, always with the same immediate effects; but occasionally it was succeeded by the expulsion of large portions of the worm. The worm was represented to have the appearance before noticed. The patient had sufficient resolution and confidence to continue for some time the use of the medicine, and at length became free, not only from any further appearance of tænia in the stools, but likewise from all those sensations which had so long denoted its presence in the intestines.

Case VI.

By James Saner, *Surgeon, F.M.S.*

A woman, about 40 years of age, came to me in May last, very much agitated, having just voided about six or eight feet of tape-worm. She told me, that pieces had come away for the last seven years whenever she took a dose of jalap, which she had done that morning. She never found any thing to relieve her so much as the jalap, though she had taken a great deal of medicine from respectable practitioners, and had also been under the care of a noted empiric for two years.

I thought this a good opportunity for trying the ol. tereb. rectificat. I therefore gave her one ounce with an equal quantity of syrup of saffron. In less than two hours she returned to me with about eight feet of the worm, with the head attached. She was very much gratified by this, as she had been told to look for the small black head. The medicine did not produce any unpleasant sensation; merely a slight degree of nausea, a giddiness as if intoxicated, and a frequent desire to void urine, though without pain.

The day after, she complained of a feeling of emptiness in the stomach. I gave her the decoct. cinchonæ for a few days, which completely removed the sensation, and she has remained perfectly well ever since.

P.S.—The woman informed me she used to eat raw meat formerly, as it seemed to ease her stomach more than any thing else; but since she voided the worm, she has had no craving for it.

Case VII.

By the same.

Since communicating the above, I am sorry to say I have had a case of tænia, where the ol. tereb. rect. has not so completely answered my expectation.

Being very sanguine in my opinion of it, in consequence of my former success, I mentioned the case to a relation of mine, who informed me he knew a labouring mechanic (a Russian) who had voided large pieces of tape-worm for a number of years. He persuaded this man to visit me, and I gave him the same dose I had given my former patient. It brought away a very large quantity, but so very soft that I could not measure it. As I could not perceive any thing like the head of the worm, I advised him to repeat the dose in a few days, which he very readily complied with, as he had suffered very little from the first.

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Aug. 27, 1810.

LXIII. *A short*
made in determ
T. S. Evans.

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sketch, the various endeavours of these illustrious men, is the humble attempt of this short essay; which, it is hoped, will have the desired effect of stimulating others, who possess the means, to the consideration of the subject, that we may shortly be enabled to discover its quantity with the greatest accuracy, at all altitudes, and under all changes of the atmosphere.

There appears to be but little doubt that the astronomic refraction was known to the ancients, since it is expressly mentioned by Ptolomy, although not made use of in his calculations*. He says, near the end of the eighth book of the Almagest, that in the rising and setting of the heavenly bodies there are changes which depend upon the atmosphere: and he mentioned it more at length in a work on optics which unfortunately has not been handed down to us†.

Alhazen, an Arabian writer, who is generally supposed to have lived about the year 1100, and to have taken the greater part of his optics from the works of Ptolomy, speaks also decidedly of it, and shows the manner of convincing ourselves of its existence by experiment ‡.

"Take," he says, "an armillary sphere which turns round its poles, and measure the distance of a star from the pole of the world when it passes near the zenith in the meridian, and when it is rising or setting near the horizon, and you will find the distance from the pole less in the latter case." He then demonstrates that this must arise in consequence of the refraction, but he does not state its quantity.

In the collection of observations made by Bernard Walter, published by Willebrord Snell, in the year 1618, it is stated, the observations were so exact that they pointed out to Walter the quantity by which the altitudes of the stars and planets were increased on account of the refraction.

Tycho Brahe §, however, appears to have been the first who asserted, with any degree of accuracy, that the refraction elevates the heavenly bodies rather more than half a degree when in the horizon. But either his instruments or his observations were not sufficiently correct to determine it with certainty for all degrees from the zenith to the horizon: and accordingly where these failed the rest was supplied by conjecture. He believed that the sun's refraction was 34′ in the horizon, and that it became insensible at 45° of altitude. For the stars, however, he assumed an en-

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* La Lande's Astronomy, 2163, 3d edit. Encyclop. Yverd. art. *Refraction.* Encycl. Meth. do.

† La Lande's Astronomy, as above. Smith's Optics, p. 58. Remarks. Priestley's Hist. Opt. 4to, p. 18.

‡ Encyclop. Yverd. art. *Refraction.* § Progymn. p. 15.

tirely different quantity, viz.
according to him, terminated

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solstices, and found it four m
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but he thought the moon h
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and the stars 30′ 37″.

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* Mem. de l'Acad. av. s. renouv. tor
p 251, where a comparison is given c
Flamsteed. † Encyc

‡ The greater part of these very e
given in his *Astronomiæ instauratæ*
1598. This work is now become ext
in a few of the great public libraries
the plates engraved again upon a redu
moirs of the Academy of Sciences of F

§ Progymn. p. 79. 104. Street's A
p. 254. Maria Cunitia *Urania Propiti*

‖ Astr. Reformat. Astr. ref. Tabu

It was not till after the year 1672, that a tolerably near table of refraction made its appearance, when the elder Cassini took the subject into consideration[*]. What led to this was the voyage of Richer to Cayenne in that year, upon the utility of which some very excellent remarks were made by Cassini, showing how far observations made in a situation so near the equator tended to confirm or disprove certain theories derived from observations made in Europe[†]. Several very useful deductions were drawn from a comparison of those made both at Paris and Cayenne; among others the refraction was settled upon more accurate elements than heretofore[‡], and a new Table computed, for the first time, of its quantity for all degrees, up to the zenith; an abridgement of which is given in the margin.

Table continued.

Alt.	Refract.
0°	30′ 0″
6	9·0
7	8·15
8	6·45
9	6·0
10	5·30
11	5·0
12	4·30
13	4·0
14	3·30
15	3·0
16	2·30
17	2·0
18	1·15
19	0·30
20	0·0

Alt.	Refract.
0	32·20
1	27·56
2	21· 4
3	16· 6
4	12·48
5	10·32
10	5·28
15	3·36
20	2·39
30	1·42
40	1·10
45	0 59
50	0·50
60	0·34
70	0·21
80	0·10
90	0· 0

From the relation of his grandson, it appears, however, that Cassini had at one time computed three tables of refraction for all altitudes: one for winter, another for summer, and a third for spring and autumn: but several doubts having been suggested to him respecting this arrangement, although in appearance conformable to nature, and principally the observations of Richer at Cayenne, where the refraction was found little different from that at Paris, he changed his opinion; and, judging that since the great difference of heat of the torrid zone, from that of the temperate, which we inhabit, does not cause sensible differences in the refraction; therefore, the greatest heat or cold of our climate could not change it much; and he then fixed upon one table, which was that used by the astronomers of the Royal Observatory of Paris, up to the year 1745[§].

It was always thought, before the time of Cassini, that the refraction did not extend its influence higher

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[*] Mem. de l'Acad. avant son renouv. tom. v. p. 81.

[†] In the observations of Picard made in various parts of France, in the year 1674, there are several for ascertaining the refraction; and a table is given from them for each degree of altitude up to 23°.—*Mem. de l'Acad. av. son renouv.* tom. iv. p. 111.

[‡] Mem. de l'Acad. avant son renouv. tom. v. p. 105.

[§] Ibid. 1745.

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the first who prov
zenith[*]. He als
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[*] Mem. de l'Acad. 170
[†] " Refractio solis inocc
in Lapland were made by
[‡] In two papers of rem
the Mem. de l'Acad. 1700,
[§] Smith's Optics, p. 6
Geography, vol. i. p. 441.
[¶] Voy. dans les Mem. d

on the subject; and, perhaps, bring others to light of as great or greater importance*.

The refraction of the north being so considerable, is very useful to the inhabitants, who are deprived of the sun's light during many months; as it makes the sun rise much earlier, and set much later to them, than it otherwise would.

About the year 1725, Mr. Flamsteed, the English Astronomer Royal, published his table† computed from his own observations: and this was the one commonly used in England for many years afterwards.

Sir Isaac Newton also constructed one‡ from theory, which was first published by Dr. Halley in the Philosophical Transactions, No. 368, for 1721. He made the horizontal refraction 33′ 45″; whereas Mr. Flamsteed's was only 33′ 0″.

But although the refraction might be determined within a few seconds at all altitudes by observation; yet, the law of its increase from the zenith to the horizon was a subject that occupied the principal mathematicians and astronomers for more than a century§. Newton having discovered the general principles of attraction, found that the refraction was a consequence of this law of nature; and that it arose from the attraction of the atmosphere on the particles of light. On this principle the curve which a ray of light describes might be determined; since it is successively attracted by different layers of the atmosphere, increasing in density as they approach the earth, and, consequently, bending the ray more and more from the right line which it described in the vacuum previous to its reaching the atmosphere. There are many authors who have endeavoured to find from theory the curve described by this ray in its course, by the assumption of various hypotheses: but perfection and our attempts to arrive at it, as is well observed by the elder Cassini in discoursing on this subject, are like the progress of certain curves and their asymptotes. The principal of these writers on the subject are, Bernouilli[1], Boscovich[2], Bouguer[3], Cassini[4], Des Cartes[5],

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* Encyclop. Meth. art. *Refraction*.

† Hist. Célest. vol. i. p. 396; also Hodgson's Math. vol. i. p. 367. Long's Astronomy, p. 254. ‡ Long's Astr. p. 254.

§ In 1714, Cassini published in Mem. de l'Acad. for that year, some methods of finding the refraction by observation, and of determining its quantity by theory. He has also given a table of it for the first 30° of altitude, computed, first, according to a rectilinear, and, secondly, according to a circular hypothesis which he there assumes.

[1] Hydrodyn. 1738, p. 221. [2] Oper. tom. ii.

[3] Prix de 1729. Mémoires, 1739, p. 407; 1749, p. 75.

[4] Epist. ad Montanari, 1665. Refrassioni è Parallasse, &c. 1671. Mem. for 1714, and his Astr. vol. i. p. 11. Paris, 1740, in 2 vols. 4to.

[5] Dioptrique, 4to. Paris, 1637.

De la Grange[6], Euler[7], Greg
Kramp[11], Lambert[12], Laplace[13],
Thomas Simpson[17], Brook T
Mayer[20], La Hire[21], d'Alembe

It was conjectured by many
refraction was subject to var
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astronomic instruments were
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in winter than in summer. H
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had scarcely risen, when it wa
tions being raised by the sun's
between Paris and Mount Vale
whereas, before the sun rose, the
than that of Mount Valerian †.

The density of the atmosphere
of the refraction, it was very nat
decrease as this density becam
which diminished its weight, or
by heat: and, indeed, astronome

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[6] Nouveaux Mémoires de Berlin, vol. iii.
[8] Astronomy, vol. i. p. 358. edit. of 171
[9] Mathematics, vol. i. p. 367. Fluxions.
[10] Traité de la Lumiere, p. 44. Dioptri
[11] Analyse des Refract. Astr. et Terres.
[12] Les Proprietés Remarquables de la R
1759. Another edition in German, 1773.
[13] Mécanique Céleste, vol. iv. p. 231.
[14] Tables, 1770. [15] Principia,
[16] Ephem. de Milan, 1788. [17] Mat
[18] Methodus Incrementorum, 4to. Lond.
[19] Dissertatio de Computo refractionum A
[20] De Refractionibus Astronomicis, 4to.
[21] Mem. de l'Acad. pour 1702, p. 52.
[22] Opuscules Mathématiques, tom. viii. p
† Mem. de l'Acad. av. s. ren. tom. i.

before they discovered that very sensible differences were occasioned by these circumstances.

But all the honour of introducing corrections on account of the variation of density in the atmosphere, as indicated by the barometer and thermometer, is due to Messrs. Lowthorpe and Hauksbee; the former of whom, in 1698, proved by a very simple experiment, in the presence of the Royal Society, that the refractive power of air is directly proportional to its density*: and the latter, by repeating and extending the same course of experiments in the year 1708, with the machinery pointed out by the former, found that the variations of refraction, depending on the barometer, are proportional to the alteration of height of the mercury in the tube: and by a series of these experiments, he furnished us with a table of the corrections which it is necessary to make on account of the changes of heat indicated by the thermometer. These experiments, although not quite conclusive on the subject, were yet made with as much accuracy and care as the nature of the machinery, and the state of experimental philosophy of that time, would admit. An example is also given, towards the end of his paper, on the mode of applying them to correct the refraction. By these, Hauksbee found that a volume of air expressed by unity, when the thermometer was at 130° above zero, became, at 50° below, one-eighth more dense: or, which is the same thing, that the air lost one-eighth of its density, for an elevation of 180 degrees of Fahrenheit's thermometer; which is exactly the difference of heat between melting ice and boiling water. But although this one-eighth, as will be shown hereafter, was too small; yet it laid the foundation for other experiments, since made by several philosophers, by which the quantity of expansion has been determined more accurately.

We have already shown that the refraction near the pole is greater than in our climate †; the degree of cold being more intense. It was also found to be less in the torrid zone, where the heat is greater than in Europe. Bouguer made a variety of observations at Peru ‡, the result of which he has given us. In 1740, he came down into an island situated in the river of Emeralds, called Isle of Inca, where he determined the refraction from 1° to 7° of altitude: and the table which he computed therefrom, shows the refrac-

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* Hauksbee's Exper. 4to, 1709, p. 175.

† It was, however, found by Capt. Phipps, in his voyage to the North, in 1773, that the refraction in latitude 80° was the same as in England. But this was in summer.

‡ Vide Mem. Ac. 1739, and his Fig. de la Terre.

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horizontal refraction he
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a table † for Quito, wh
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cording to the choice

It is indeed rather
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tions with each other,
mometer only, withou
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He concludes his
without being able t
does it clearly appea

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* This Table is in the me
† Mem. 1749. Conn. des
tom i. § Mem. p. 17

mentioned experiments made by Hauksbee till about the year 1749[*]. It is also worthy of remark, that although the necessity of introducing corrections on account of the alterations of the barometer and thermometer were likewise shown to be absolutely necessary by Dr. Halley[†], and the circumstance mentioned, and in some degree admitted by Le Monnier[‡], yet it does not appear that he followed the advice of his illustrious contemporary, but merely endeavoured, as Cassini did, to reconcile his observations with the state of the thermometer at the time of making these observations, without taking the barometer into account[§].

[To be continued.]

LXIV. *Some Particulars respecting the Thunder-storm at London, and in its Vicinity, on the* 31*st of August* 1810. *By Sir* H. C. Englefield, *Bart. F.R.S. and F.S.A.*

To Mr. Tilloch.

Sir, As the stroke of thunder, which was felt in London at about half after two o'clock in the morning of the 31st of August last, was, perhaps, the most violent and awful ever experienced in this country, you may not think the following account of it from an eye-witness, and who was very near the spot where it fell and did mischief, unworthy of insertion in your Journal.

I was with three friends in a coach standing at a house where we had supped. The house-door was still open, and there was a strong light from a large lustre in the hall, full on the coach, and two very bright lamps at the door of the house. This circumstance was in favour of our seeing the nature of the light distinctly; for, had we been in the dark, its excessive brightness would have so dazzled our eyes as to prevent all distinct vision. As we got into the coach there was a small mizzling rain, and a very strong flash of distant lightning in the N.E., but no thunder that we could hear. The servants at the door said there had been much distant lightning for an hour or two.

The sky over head appeared very dark, but the lights prevented accurate observation of it. We were just seated in

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[*] Mem. de l'Acad. 1749, p. 106.—Probably this was on account of some reflections made by him on the French philosophers who repeated his experiments before the Royal Academy of Paris, and failed in their results.—Vide his book, p. 196.

[†] Philosophical Transactions 1720, No. 364.

[‡] Hist. Céleste, 4to. Paris, 1741.

[§] See the whole of his *Discours prelim.* prefixed to the work before cited.

the carriage, and my
window nearly towar
which however I coul
were looking out of t
from which we were d
once enveloped by an
of more than instanta
explode into sparks m
My friends saw them
and their motion was
the porch appear to vi
the effect of what in a
which as it bursts th
squibs in all direction
sparks an astonishing
took place, similar in s
cannon directly at us;

The explosion seem
companied by a sens
vast weight had fallen
close by us. The sou
for a very long time m

Astonishment and t
we then agreed to qu
house, the door of w
drops of rain then fell
the servants standing
to them to threaten t
heavy rain now came
We were all in fear
but nothing followed
As we passed the gate
sington, we stopped, a
and felt. He told us t
for that he was dazzl
and was scarcely him
seemed to him that a
In our way to town v
ning to the N. W. w
time we arrived in to
stars very bright.

The succeeding day
son extremely hot; th
and free from reflect
severe thunder-storm
cool the air, for both

hot as Friday, and the nights uncommonly hot, though very bright star-light. Having been informed that mischief was done at Kensington Palace, by the tremendous flash I had witnessed, I went to view the spot. A large elm in the outer Palace-yard, near the Guard-house, and about 120 yards from the spot where our carriage stood, was struck in a manner rather uncommon. A main root about the size of a man's thigh was blown out of the ground to the length of twelve feet from the trunk of the tree, and was broken into three pieces. The trunk of the tree was barked at intervals, not in a continued line, and this injury quitted the main stem at the lowest large branch, and followed that branch up to a fork where some decay appeared in the wood. Beyond that, no injury appeared, nor was the main stem or any other branch higher up affected. The whole appearance of the tree, as well as the sensation I felt from the explosion, lead me to think that the shock was from the earth to the passing cloud.

The part of the Palace directly opposite to the tree is a long building with large arched windows. In these 48 panes of glass were broken by the concussion. This building is about 50 yards from the tree.

The sentinel at the Duke of Sussex's door was knocked down by the shock, and remained, as he said, senseless for some minutes.

Another carriage had just quitted the door where we were, and which was perhaps still nearer the tree than we were. The horses stopped short, and remained motionless. The gentleman in the carriage, when he recovered from his surprise, spoke to his coachman, who as well as the footman declared themselves stunned and blinded. After a pause of a few minutes they however recovered, and felt no further ill effects.

I have been several times as near mischief in storms as I now was; but I am certain that I never saw or heard any lightning or thunder which could be at all compared in tremendous severity to this: indeed it was of a different kind from any other, as the sound was not sharp and crackling as thunder very near usually is, but deep and heavy. Two of the gentlemen who were with me have been often in the southern parts of Europe and the Mediterranean, where storms are much more severe than is usual in England; but they agreed with me that they never had witnessed any thing at all like this. Its effect in London, though the nearest part of the town is full two miles from the explosion, was very singular. Almost every body was waked account for the experi[illegible]

[illegible], Nov. 2, 18[illegible]

LXXV. Researches on [illegible]
Combinations; and [illegible]
With some Experi[illegible]
made in the Labor[illegible]
H. Davy, Esq. Se[illegible]

The Chemists [illegible]
[illegible]
[illegible]
[illegible]
[illegible]

M. [illegible]
[illegible]
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Dr. [illegible]
[illegible]
[illegible]
[illegible]

In the [illegible]
[illegible]
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In the second [illegible]

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Gay Lussac and Thenard have detailed an extensive series of facts upon muriatic acid and oxymuriatic acid. Some of their experiments are similar to those I have detailed in the paper just referred to; others are peculiarly their own, and of a very curious kind: their general conclusion is, that muriatic acid gas contains about one quarter of its weight of water; and that oxymuriatic acid is not decomposable by any substances but hydrogen, or such as can form triple combinations with it.

One of the most singular facts that I have observed on this subject, and which I have before referred to, is, that charcoal, even when ignited to whiteness in oxymuriatic or muriatic acid gases, by the Voltaic battery, effects no change in them; if it has been previously freed from hydrogen and moisture by intense ignition in vacuo.

This experiment, which I have several times repeated, led me to doubt of the existence of oxygen in that substance, which has been supposed to contain it above all others in a loose and active state; and to make a more rigorous investigation than had been hitherto attempted for its detection.

If oxymuriatic acid gas be introduced into a vessel exhausted of air, containing tin; and the tin be gently heated, and the gas in sufficient quantity, the tin and the gas disappear, and a limpid fluid, precisely the same as Libavius's liquor, is formed:—it occurred to me, that if this substance is a combination of muriatic acid and oxide of tin, oxide of tin ought to be separated from it by means of ammonia. I admitted ammoniacal gas over mercury to a small quantity of the liquor of Libavius; it was absorbed with great heat, and no gas was generated; a solid result was obtained, which was of a dull white colour; some of it was heated, to ascertain if it contained oxide of tin; but the whole volatilized, producing dense pungent fumes.

Another experiment of the same kind, made with great care, and in which the ammonia was used in great excess, proved that the liquor of Libavius cannot be decompounded by ammonia; but that it forms a new combination with this substance.

I have described, on a former occasion, the nature of the operation of phosphorus on oxymuriatic acid, and I have stated that two compounds, one fluid and the other solid, are formed in the process of combustion, of which the first, on the generally received theory of the nature of oxymuriatic acid, must be considered as a compound of muriatic acid and phosphorous acid. It occurred to me, that

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exclude moisture. The results were new combinations; that from the phosphuretted liquor was a white solid, from which a part of the phosphorus was separated by heat; but which seemed no further decomposable, even by ignition. That from the sulphuretted liquor was likewise solid, and had various shades of colour, from a bright purple to a golden yellow, according as it was more or less saturated with ammonia; but as these compounds did not present the same uniform and interesting properties as that from the phosphoric sublimate, I did not examine them minutely: I contented myself by ascertaining that no substance known to contain oxygen could be procured from oxymuriatic acid, in this mode of operation.

It has been said, and taken for granted by many chemists, that when oxymnriatic acid and ammonia act upon each other, water is formed; I have several times made the experiment, and I am convinced that this is not the case. When about 15 or 16 parts of oxymuriatic acid gas are mixed with from 40 to 45 parts of ammoniacal gas, there is a condensation of nearly the whole of the acid and alkaline gases, and from five to six parts of nitrogen are produced; and the result is dry muriate of ammonia.

Mr. Cruikshank has shown that oxymuriatic acid and hydrogen, when mixed in proportions nearly equal, produce a matter almost entirely condensible by water; and MM. Gay Lussac and Thenard have stated that this matter is common muriatic acid gas, and that no water is deposited in the operation. I have made a number of experiments on the action of oxymuriatic acid gas and hydrogen. When these bodies were mixed in equal volumes over water, and introduced into an exhausted vessel and fired by the electric spark, there was always a deposition of a slight vapour, and a condensation of from $\frac{1}{10}$ to $\frac{1}{40}$ of the volume; but the gas remaining was muriatic acid gas. I have attempted to make the experiment in a manner still more refined, by drying the oxymuriatic acid and the hydrogen by introducing them into vessels containing muriate of lime, and by suffering them to combine at common temperatures; but I have never been able to avoid a slight condensation; though, in proportion as the gases were free from oxygen or water, this condensation diminished.

I mixed together sulphuretted hydrogen in a high degree of purity and oxymuriatic acid gas, both dried, in equal volumes: in this instance the condensation was not $\frac{1}{40}$; sulphur, which seemed to contain a little oxymuriatic acid, was formed on the sides of the vessel; no vapour was deposited; and the residual gas contained about $\frac{19}{20}$ of muriatic acid gas, and the remainder was inflammable.

MM. Gay Lussac and Thenard have proved by a copious collection of instances, that in the usual cases where oxygen is procured from oxymuriatic acid, water is always present, and muriatic acid gas is formed: now, as it is shown that oxymuriatic acid gas is converted into muriatic acid gas by combining with hydrogen, it is scarcely possible to avoid the conclusion, that the oxygen is derived from the decomposition of water, and, consequently, that the idea of the existence of water in muriatic acid gas is hypothetical, depending upon an assumption which has not yet been proved—the existence of oxygen in oxymuriatic acid gas.

MM. Gay Lussac and Thenard indeed have stated an experiment, which they consider as proving that muriatic acid gas contains one quarter of its weight of combined water. They passed this gas over litharge, and obtained so much water; but it is obvious that in this case they formed the same compound as that produced by the action of oxymuriatic acid on lead; and in this process the muriatic acid must lose its hydrogen, and the lead its oxygen; which of course would form water: these able chemists, indeed, from the conclusion of their memoir, seem aware that such an explanation my be given, for they say that the oxymuriatic acid *may be* considered as a simple body.

I have repeated those experiments which led me first to suspect the existence of combined water in muriatic acid, with considerable care; I find that, when mercury is made to act upon one in volume of muriatic acid gas, by Voltaic electricity, all the acid disappears, calomel is formed, and about ·5 of hydrogen evolved.

With potassium, in experiments made over very dry mercury, the quantity of hydrogen is always from nine to eleven, the volume of the muriatic acid gas used being 20.

And in some experiments made very carefully by my brother Mr. John Davy, on the decomposition of muriatic acid gas, by heated tin and zinc, hydrogen equal to about half its volume was disengaged, and metallic muriates, the same as those produced by the combustion of tin and zinc in oxymuriatic gas, resulted.

It is evident from this series of observations, that Scheele's view (though obscured by terms derived from a vague and unfounded general theory) of the nature of the oxymuriatic and muriatic acids may be considered as an expression of facts; whilst the view adopted by the French school of chemistry, and which, till it is minutely examined, appears so beautiful and satisfactory, rests, in the present state of our knowledge, upon hypothetical grounds.

When oxymuriatic acid is acted upon by nearly an equal volume of hydrogen, a combination takes place between them, and muriatic acid gas results. When muriatic acid gas is acted on by mercury, or any other metal, the oxymuriatic acid is attracted from the hydrogen, by the stronger affinity of the metal; and an oxymuriate, exactly similar to that formed by combustion, is produced.

The action of water upon those compounds, which have been usually considered as muriates, or as dry muriates, but which are properly combinations of oxymuriatic acid with inflammable bases, may be easily explained, according to these views of the subject. When water is added in certain quantities to Libavius's liquor, a solid crystallized mass is obtained, from which oxide of tin and muriate of ammonia can be procured by ammonia. In this case, oxygen may be conceived to be supplied to the tin, and hydrogen to the oxymuriatic acid.

The compound formed by burning phosphorus in oxymuriatic acid is in a similar relation to water: if that substance be added to it, it is resolved into two powerful acids; oxygen, it may be supposed, is furnished to the phosphorus to form phosphoric acid, hydrogen to the oxymuriatic acid to form common muriatic acid gas.

None of the combinations of the oxymuriatic acid with inflammable bodies can be decomposed by dry acids; and this seems to be the test which distinguishes the oxymuriatic combinations from the muriates, though they have hitherto been confounded together. Muriate of potash for instance, if M. Berthollet's estimation of its composition approaches towards accuracy, when ignited, is a compound of oxymuriatic acid with potassium: muriate of ammonia is a compound of muriatic acid gas and ammonia; and when acted on by potassium, is is decompounded: the oxymuriatic acid may be conceived to combine with the potassium to form muriate of potash, and the ammonia and hydrogen are set free.

The vivid combustion of bodies in oxymuriatic acid gas, at first view, appears a reason why oxygen should be admitted in it; but heat and light are merely results of the intense agency of combination. Sulphur and metals, alkaline earths and acids, become ignited during their mutual agency; and such an effect might be expected in an operation

tion so rapid, as that of oxymuriatic acid upon metals and inflammable bodies.

It may be said, that a strong argument in favour of the hypothesis, that oxymuriatic acid consists of an acid basis united to oxygen, exists in the general analogy of the compounds of oxymuriatic acid, and metals, to the common neutral salts: but this analogy, when strictly investigated, will be found to be very indistinct; and even allowing it, it may be applied with as much force to support an opposite doctrine, namely, that the neutral salts are compounds of bases with water, and the metals of bases with hydrogen; and that, in the case of the action of oxymuriatic acid and metals, the metal furnishes hydrogen to form muriatic acid, and a basis to produce the neutral combination.

That the quantity of hydrogen evolved during the decomposition of muriatic acid gas by metals, is the same that would be produced during the decomposition of water by the same bodies, appears, at first view, an evidence in favour of the existence of water in muriatic acid gas; but as there is only one known combination of hydrogen with oxymuriatic acid, one quantity must always be separated. Hydrogen is disengaged from its oxymuriatic combination, by a metal, in the same manner as one metal is disengaged by another from similar combinations; and of all inflammable bodies that form compounds of this kind, except perhaps phosphorus and sulphur, hydrogen is that which seems to adhere to oxymuriatic acid with the lesat force.

I have caused strong explosions from an electrical jar to pass through oxymuriatic gas, by means of points of platina, for several hours in succession; but it seemed not to undergo the slightest change.

I electrized the oxymuriates of phosphorus and sulphur for some hours, by the power of the Voltaic apparatus of 1000 double plates: no gas separated, but a minute quantity of hydrogen, which I am inclined to attribute to the presence of moisture in the apparatus employed; for I once obtained hydrogen from Libavius's liquor by a similar operation: but I have ascertained that this was owing to the decomposition of water adhering to the mercury; and in some late experiments made with 2000 double plates, in which the discharge was from platina wires, and in which the mercury used for confining the liquor was carefully boiled, there was no production of any permanent elastic matter.

As there are no experimental evidences of the existence of oxygen in oxymuriatic acid gas, a natural question arises concerning the nature of these compounds, in which the muriatic acid has been supposed to exist, combined with much more oxygen than oxymuriatic acid, in the state in which it has been named, by Mr. Chenevix, hyperoxygenized muriatic acid.

Can the oxymuriatic *acid* combine either with oxygen or hydrogen, and form with each of them an acid compound; of which that with hydrogen has the strongest, and that with oxygen the weakest affinity for bases? for the able chemist to whom I have just referred, conceives that hyperoxymuriates are decomposed by muriatic acid. Or, is hyperoxymuriatic acid the basis of all this class of bodies, the most simple form of this species of matter?

The phænomena of the composition and decomposition of the hyperoxymuriates may be explained on either of these suppositions; but they are mere suppositions unsupported by experiment.

I have endeavoured to obtain the neutralizing acid, which has been imagined to be hyperoxygenized, from hyperoxymuriate of potash, by various modes, but uniformly without success. By distilling the salt with dry boracic acid, though a little oxymuriatic acid is generated, yet oxygen is the chief gaseous product, and a muriate of potash not decomposable is produced.

The distillation of the orange-coloured fluid, produced by dissolving hyperoxymuriate of potash in sulphuric acid, affords only oxygen in great excess, and oxymuriatic acid.

When solutions of muriates, or muriatic acid are electrized in the Voltaic circuit, oxymuriatic acid is evolved at the positive surface, and hydrogen at the negative surface. When a solution of oxymuriatic acid in water is electrized, oxymuriatic acid and oxygen appear* at the positive surface, and hydrogen at the negative surface; facts which are certainly unfavourable to the idea of the existence of hyperoxygenized muriatic acid, whether it be imagined a compound of oxymuriatic acid with oxygen, or the basis of oxymuriatic acid.

If the facts respecting the hyperoxymuriate of potash, indeed, be closely reasoned upon, it must be regarded as nothing more than as a triple compound of oxymuriatic acid, potassium, and oxygen. We have no right to

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* The quantity of oxymuriatic acid in the aqueous solution is so small, that the principal products must be referred to the decomposition of water. This happens in other instances; the water only is decomposed in dilute solutions of nitric and sulphuric acids.

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properties and powers of combination; but differing from them, in being for the most part decomposable by water? On this idea muriatic acid may be considered as having hydrogen for its basis, and oxymuriatic acid for its acidifying principle. And the phosphoric sublimate as having phosphorus for its basis, and oxymuriatic acid for its acidifying matter. And Libavius's liquor, and the compounds of arsenic with oxymuriatic acid, may be regarded as analogous bodies. The combinations of oxymuriatic acid with lead, silver, mercury, potassium, and sodium, in this view would be considered as a class of bodies related more to oxides than acids, in their powers of attraction.

It is needless to take up the time of this learned society by dwelling upon the imperfection of the modern nomenclature of these substances. It is in many cases connected with false ideas of their nature and composition; and, in a more advanced state of the inquiry, it will be necessary for the progress of science, that it should undergo material alterations.

[To be continued.]

LXVI. *Of the Bogs in Ireland.*

The first Report of the Commissioners appointed by Parliament to inquire into the nature and extent of the several bogs in Ireland, and the practicability of draining and cultivating them, has just made its appearance. It consists of seven folio pages, and an Appendix containing, 1. Instructions of the Commissioners to their Engineers—3 pages: 2. Names of the Engineers, Surveyors, Clerks, and other Officers appointed and employed by the Commissioners; with their Salaries and Rewards—1 page: 3. Account of all Sums of Money paid by or under the Authority of the Commissioners—1 page: 4. Report of Mr. Richard Griffith, jun. Civil Engineer, on the Practicability of draining and improving a Part of the Bog of Allen—41 pages. It is accompanied with a Map of Part of the Bog of Allen; transverse Sections of Lullymore Bog; a Section of a subterraneous River in Lullymore Bog; and a Section of a Turf Bank in Timahoe Bog.

The commissioners, after some preliminary observations, state, that in forming their opinions on the points connected with their inquiry, they derived their principal assistance from the Great Ordnance Survey of Ireland, executed by General Vallancey, the Chairman of their Board, it being the only map which defines either the situation or

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"Most of the bogs which lie to the eastward of the Shannon, and which occupy a considerable portion of the King's county and county of Kildare, are generally known by the name of the Bog of Allen: it must not however be supposed that this name is applied to any one great morass; on the contrary, the bogs to which it is applied are perfectly distinct from each other, often separated by high ridges of dry country and inclining towards different rivers, as their natural directions for drainage, so intersected by dry and cultivated land, that it may be affirmed generally there is no spot of these bogs (to the eastward of the Shannon) so much as two Irish miles distant from the upland and cultivated districts.

"With this first and general view of the subject, we had no hesitation in selecting at once the whole of the eastern portion of the great district above referred to, as the object of our first inquiries, forming in itself one whole, whose parts had more or less connexion with each other, lying in the centre of Ireland, in the immediate vicinity of some of the richest and best cultivated counties; intersected also by the two great lines of navigation the Grand and the Royal Canals, and presenting in common apprehension very considerable obstacles to improvement; the overcoming of which would in itself demonstrate the practicability of the improvement of the bogs of Ireland in most other cases.

"We were further induced to form this selection on the general principles of beginning at the end of the great division above referred to, which lies nearest to the capital, and proceeding gradually to its termination at the Western Ocean; not however considering ourselves precluded from making occasional exceptions, where particular circumstances might appear to require it.

"The proportion which the bogs in this district bear to the entire of the bogs of Ireland, appeared to us a further inducement; and we are the more disposed to mention this, as we find that by some we have been thought to have embarked in the first instance on too great a scale: on this we shall merely observe, that having two years allotted to us for the duration of our commission, we undertook at once rather less than one third of our task, in the supposition that it would require about eight months for its execution.

"Having determined to give in charge the whole of this district, it became the next object of our consideration, on what principle we should subdivide it into the smaller districts, referred to in the first article of our instructions, for the purpose of being assigned to separate engineers.

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with those usual in Great Britain, and considering the hardships attendant on this peculiar service. The appointment of the engineers we necessarily hold in our hands, and select them under the obligation of our oaths; the appointment of the surveyors we commit entirely to the engineers, holding the latter responsible for the qualifications of the persons they employ.

" We account with every engineer once a week, and he makes his return to us upon his oath.

" To give an idea of the scale and nature of our expenditure, we subjoin, as the second and third articles of our Appendix, copies of accounts already called for by your honourable house.

" Owing to the winter season having set in, almost immediately after the appointment of the engineers, and which was particularly unfavourable to the execution of the survey, we have as yet received but one of their reports, although they are most of them, we believe, in a state of considerable forwardness.

" This report we have determined on laying at once before your honourable house, considering it as sufficient in itself, to enable the public to form a pretty accurate opinion of the degree of information which may be expected from the execution of our commission; and feeling also, that if we deferred it any longer, we should have no other opportunity before the opening of the next session: we have accordingly subjoined it as the fourth article of the Appendix to this Report.

" The district reported on contains 36,430 English acres of bog, and forms the eastern extremity of the Bog of Allen. The map furnished to us by Mr. Griffith is on a scale of four inches to an Irish mile, and is accompanied by sections of the bog of nearly 200 miles in extent.

" As these maps and sections could not be engraved without enormous expense, we have subjoined to this report a map executed on a scale as much reduced as is consistent with clearness, and which scale we propose to apply universally in the different maps which in the execution of the commission it will become our duty to furnish to your honourable house; and this map we have accompanied with three lines of sections of the bog, to serve at once as specimens of the manner in which the sections are executed, and to convey a clearer view than could be expressed in words, of the internal structure of a great bog; a view, we believe, materially different from any of those generally received.

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ing in the degree of blackness and compactness proportionate to its depth: near the bottom of the bog it forms a black mass, which when dry has a strong resemblance to pitch or bituminous coal, and having a conchoidal fracture in every direction, with a black shining lustre, and susceptible of receiving a considerable polish.

" We have requested Mr. Griffith to make a chemical analysis of these different strata, which he has done in the laboratory of the Dublin Society, and an account of which, with the section above alluded to, forms the Appendix to his Report. Immediately below this lower stratum, there is generally found a thin stratum of yellow or blue clay, varying in thickness from one to six feet; in some places the peat rests on a thinner stratum of yellowish white marl, containing on an average about 60 per cent. of calcareous matter: this stratum of clay in this district universally rests on a solid mass of clay and limestone gravel mixed together, and extending to an unknown depth.

" We should further consider the peat moss as partaking in its general nature of the property of sponge completely saturated with water, and giving rise to different streams and rivers for the discharge of the surplus waters which it receives from rain or snow: these streams in this district almost universally have worn their channels through the substance of the bog down to the clay or limestone gravel underneath, dividing the bog into distinct masses, and presenting in themselves the most proper situations for the main drains, and which, with the assistance of art, may be rendered effectual for that purpose.

" Such is the internal structure of the bogs in this district.

" Viewing them externally, they present surfaces by no means level, but with planes of inclination amply sufficient for their drainage: the highest summit of any part of the bogs in this district, is 298 feet above the level of the sea, taken at an ordinary spring-tide in the Bay of Dublin; while the lowest point any where on their surface is 84 feet lower than the highest, and therefore 214 feet above the level of the sea.

" It requires a mere inspection of the map and sections, to be convinced that there is no part of these bogs from which the water may not be discharged into rivers in their immediate vicinity, and with falls adequate to their drainage; and we observe, that in the instance of the Bog of Timahoe, a part of its water is discharged into the sea at Drogheda, and another part below Waterford."

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of moss in the last stage of decomposition, and decayed branches of heath.

Colour, ... reddish-brown:
Specific gravity .. ·895.

" 1,440 grains of this substance yielded but 20 grains of white ashes, which are found to be composed of vegetable matter.

" No. 2. .. .. 3 feet thick.

" The mass here is very open-grained and fibrous; the moss is usually so perfect, that the different species are easily discernible to the botanist: the sphagnum palustre is observed greatly to predominate.

Colour, ... light reddish-brown:
Specific gravity .. ·356.

" 1,440 grains of this substance yielded but 12 of white ashes, of similar composition to No. 1.

" No. 3. .. .. 5 feet thick.

" Mass open-grained and fibrous; varieties of moss visible, but not so perfect as in No. 2: used as turf, but burns badly, on account of the openness of its texture, and its containing no empyreumatic oil.

Colour, ... pale yellowish-brown:
Specific gravity .. ·408.

" 1,440 grains of the dried peat yielded but 11 grains of white ashes, of similar composition to the foregoing.

" No. 4. .. .. 8½ feet thick.

" Mass tolerably compact, but still fibrous; when used as turf, it burns tolerably well.

Colour, ... deep reddish-brown:
Specific gravity .. ·871.

" 1,440 grains of the dried peat yielded 12 grains of yellowish white ashes, composed of vegetable matter, with a tinge of oxide of iron.

" No. 5. .. .. 3 feet thick.

" Mass compact; fibres of moss rarely discernible*; numerous twigs, and small branches of birch, alder, and fir-trees, are observable amongst the peat in this part of the turf bank. Upon near inspection it was found, that all the branches and twigs were quite hollow; the wood being decayed had disappeared, leaving the bark perfect. This division of the turf bank, when used as turf, burns pleasantly, but quickly.

Colour, ... blackish brown:
Specific gravity .. 1·030.

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* Branches are not found contained in the body of the Bog generally, and even at the edges not universally.

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smell.
Colour,
“ Fracture earthy
tre, when first brok
Specific gr
“ 1,440 grains yield
Of water, contai
could not be se
A very compact c
Gaseous product

“ 500 grains of this charcoal yielded of deep reddish-brown ashes 50 grains, which are chiefly composed of oxide of iron.

“ No. 8. .. .. 4 feet thick.

“ Mass very compact; contains no vegetable remains; is seldom used as turf, owing to the unpleasant smell it gives out when ignited.

Colour, ... black.

“ Fracture conchoidal in every direction; lustre shining; exhibits a strong resemblance to pitch or pitch coal; and is susceptible of a high degree of polish.

Specific gravity .. .. 1·236.

“ Analysis.—1,440 grains yielded, of

	Grains.
volatile empyreumatic oil	124
Water, containing oil that could not be separated	532
Charcoal very compact, internal lustre strongly glistening	566
Gaseous product, which burned with a bright light, but unpleasant smell	168
	1,440

“ 500 grains of this charcoal yielded 96 grains of brick-red ashes, which are found to be chiefly composed of oxide of iron.

“ No. 9. .. .. 3 feet thick.

“ Marle; colour, yellowish-white; does not adhere to the tongue.

“ 100 parts contain :

	Grains.
Carbonate of lime	64
Silex	24
Alumine	12
	100

“ No. 10. .. .. 4 feet thick.

“ Yellowish-blue clay; adheres strongly to the tongue.

“ 100 parts are found to contain :

Alumine	72
Carbonate of lime	6
Silex, coloured by oxide of iron	22
	100

“ Being very much pressed for time in making the foregoing analysis, I have been obliged to attend merely to the most useful results. I hope, however, in a future Report,

to be able to lay before
analysis, containing a
sition of the ashes cont
exact composition of th

LXVII. *On purifying nomelers.*

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Sir, After all the
to decrease friction in
found to answer this
long been known that
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of a better quality, tha
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About the year 1799
parate from olive oil s
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thick mucilaginous ma
This mucilage is an o
but lighter than water.
has been taken is exce
but after it has been
common oil exposed to

About ten years ago
Mr. Barraud, requesti
March 1802 he gave

" I have," says Mr.
meter, in which the
which having performe
India, is vibrating as f
it went out with to a f

Since that time Mr.
me for more of this oil
time-keepers; but to b
ing it, I wrote to him
long experience, and th
interesting answer:—

"*To Mr. Walker.*

"London, 13th October, 1810.

"Dear sir,—It is, I believe, upwards of ten years since you first favoured me with some of your purified oil, which I have ever since constantly applied to my chronometers; and on their return from a long voyage I have always found your oil in good condition, much better indeed than any which I had before been able to obtain; nor has the superior quality of yours been confined to my own observation.

"The late Mr. John Brockbank was complaining to me, some years ago, of the bad state in which he found the oil in his chronometers on their return from India, many of which had failed in consequence, although the oil he used was the best he could obtain. I then mentioned the success which had attended yours, and at his request furnished him with a small quantity, which he applied to his chronometers, and afterwards very gratefully acknowledged the advantage he had derived from its use; having found, on the return of his chronometers from India, your oil in excellent condition, and deemed it far superior in quality, for such purposes, to any he had before been able to procure.

"I have presented one of the last phials, which you favoured me with, to Mr. Vulliamy of Pall Mall, who purposes to give it a trial; but I hope you will be induced, by what has been already ascertained, to make your discovery known.

"I am, * * *

"P. P. Barraud."

Pure oil, such as I have at different times sent to Mr. Barraud, may be obtained by attending to the following directions.

Put a quantity of the best olive oil into a phial with two or three times as much water, so that the phial may be about half full. Shake the phial briskly for a little time, turn the cork downwards, and let most part of the water flow out between the side of the cork and the neck of the phial. Thus the oil must be washed five or six times.—After the last quantity of water has been drawn off, what remains is a mixture of water, oil, and mucilage. To separate these from each other, put the phial into hot water for three or four minutes, and most part of the water will fall to the bottom, which must be drawn off as before.

The oil must then be poured into a smaller phial, which, being nearly full, must be well corked, set in a cool place, and suffered to stand undisturbed for three or four months, or until all the water shall have subsided, with the mucilage on the top of it, and the oil, perfectly transparent, swimming upon the top of the mucilage. When *time* has thus completed the operation, the pure oil must be poured off into very small phials, and kept in a cool place, well corked, to preserve it from the air.

Lynn, Nov. 13, 1810. E. WALKER.

LXVIII. *A further Set of Fifteen Corollaries, to the* Musical Theorems *in Page* 39, *by means of which, the Temperaments of any one of the Concords being given, all the other Temperaments and all the Wolves can be calculated with the greatest facility. By Mr.* JOHN FAREY.

To Mr. Tilloch.

SIR, THE six musical *Theorems* which you did me the favour to print in your 147th Number having given much satisfaction to several of my musical acquaintances, I am induced to send you 15 other *Corollaries* in addition to the 12 inserted at page 44: they are naturally divisible into three classes, viz.

1st. When the temperament of the *fifth* is given.

Corollary 13. The temperament of the IIId is $= 11\Sigma + \mathrm{m} - 4 \times$ temperament of the Vth.

By theorem 3, $\frac{11s-4r}{s}\Sigma + \frac{u-4t}{u}\mathrm{m}$ is the temperament of the IIId, and $\frac{4r}{s}\Sigma + \frac{4t}{u}\mathrm{m}$ is 4 times the temperament of V, by theorem 1: the sum of these is $11\Sigma + \mathrm{m}$, whence the truth of the corollary is manifest: as indeed it is, by merely abbrevating and ordering theorem 3.

Corollary 14. $11\Sigma + \mathrm{m} - 3 \times$ temperament of V $=$ temperament of VI.

Here $\frac{11s}{s}\Sigma + \frac{u}{u}\mathrm{m} - \frac{3r}{s}\Sigma - \frac{3t}{u}\mathrm{m} = \frac{11s-3r}{s}\Sigma + \frac{u-3t}{u}\mathrm{m}$, as in theorem 6;—and so of all the following.

Corollary 15. $11 \times$ temperament of V $- 12\Sigma - \mathrm{m}$ $=$ Vth wolf.

Corollary 16. $8 \times$ temperament of $V - \Sigma =$ IIId wolf.

Corollary 17. $9 \times$ temperament of $V - \Sigma =$ VIth wolf..

2d. When the temperament of the *major third* is given.

Corollary 18. $\frac{1}{4} \times$ temperament of $III - 2\frac{3}{4}\Sigma - \frac{1}{4}m =$ temperament of V.

Corollary 19. $\frac{3}{4} \times$ temperament of $III + 2\frac{3}{4}\Sigma + \frac{1}{4}m =$ temperament of VI.

Corollary 20. $18\frac{1}{4}\Sigma + 1\frac{3}{4}m - \frac{11}{4} \times$ temperament of $III =$ Vth wolf.

Corollary 21. $21\Sigma + 2m - 2 \times$ temperament of $III =$ IIId wolf.

Corollary 22. $23\frac{3}{4}\Sigma + 2\frac{1}{4}m - \frac{9}{4} \times$ temperament of $III =$ VIth wolf.

3d. When the temperament of the *major sixth* is given.

Corollary 23. $3\frac{2}{3}\Sigma + \frac{1}{3}m - \frac{1}{3} \times$ temperament of $VI =$ temperament of V.

Corollary 24. $\frac{4}{3} \times$ temperament of $VI - 3\frac{2}{3}\Sigma - \frac{1}{3}m =$ temperament of III.

Corollary 25. $\frac{11}{3} \times$ temperament of $VI - 28\frac{1}{3}\Sigma - 2\frac{2}{3}m =$ Vth wolf.

Corollary 26. $28\frac{1}{3}\Sigma + 2\frac{2}{3}m - \frac{8}{3} \times$ temperament of $VI =$ IIId wolf.

Corollary 27. $32\Sigma + 3m - 3 \times$ temperament of $VI =$ VIth wolf.

In order to prevent mistakes in the use of Corollary 9, page 43, it may be proper to remark, that the expression there given, is for the *diesis* between ♯C and ♭D, ♯F and ♭G, and ♯G and ♭A, besides those enumerated; but not between ♯E and ♭F or ♭C and ♯C, where the half tones fall, which have a different value, for which I will give a theorem on some future occasion.

Also page 45, line 5, after "Σ only," add,—see the other equalities of wolves in Scholia 3 and 4.

In Scholium 6, after $\frac{1}{11}$c, $\frac{7}{11}$c, and $\frac{8}{11}$c, add, in each case, —*very nearly*.

At the end of Scholium 11, page 52, add,—The *sharp* and *flat* are here, by Cor. 10, each equal to $36\Sigma + f + 3m$, as in Mr. Marsh's Theory of Harmonics, p. 16.

I am, sir,
Your obedient humble servant,

Westminster, Nov. 15, 1810. JOHN FAREY.

LXIX. *On*

LXIX. *On the Barom*

T

Sir, In consequence
that a short rationale, o
in the weather, and tl
connected with them,
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Water exists in the
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it is then merely suspe
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Hence it follows, th
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the latter case, which ha
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water, now much reduc
combination.

The particular or more immediate indication of the weather which is coming, arises from the alteration which is *taking place* in the density of the atmosphere, and which the barometer exhibits by the *rising or sinking state* of the mercury; the weather becoming comparatively *clearer* as the atmosphere is becoming denser, and *duller* as the atmosphere is becoming rarer*.

Hence, if the barometer were as portable and as convenient for reference as a watch, we should seldom be at a loss to know, at least for short intervals, what kind of weather was coming†.

The ordinary range of the barometer in this climate is from RAIN to FAIR; rising however, occasionally, as high as SETTLED FAIR; and sometimes, though very rarely, as high as VERY DRY: and sinking, occasionally, as low as MUCH RAIN; and sometimes, though very rarely, as low as STORMY.

It is scarcely necessary to observe that north and east winds, in consequence of passing to us from a colder climate, and over land, bring a denser, colder, and dryer atmosphere; and south and west winds, coming to us from a warmer climate, and over the sea, bring a rarer, warmer, and damper atmosphere; and moreover, that the capacity of air for retaining water in a state of chemical combination is *increased* by coming from a colder to a warmer temperature; and *diminished*, by coming from a warmer to a colder temperature.

It must be equally apparent, that the greater or less elevation of the clouds depends upon their own degree of density, and that of the atmosphere which supports them.

With regard to the immediate causes of the direction and changes of the wind in this climate, I consider them as involved in too much obscurity and uncertainty to say any thing satisfactorily about them; and with respect to electricity, which though doubtless a powerful agent in meteorological effects, I consider it rather as a matter of curious speculation than of practical utility.

I have therefore only to add, that by a due consideration of the causes enumerated above, connected with the more obvious effects of the sun's varying influence in raising and dispelling vapours, we may, I think, account pretty satisfactorily for the various vicissitudes of weather, which mark the different seasons throughout the year; and, by the relation of the barometer to those causes, be enabled to foresee, with a considerably greater degree of certainty than is commonly supposed, the different changes of weather which are, at all times, about to take place.

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* The difference that might be supposed to arise in the height of the barometer from the effects of different degrees of heat on the atmosphere, may in observations of this nature be entirely disregarded, these effects being very nearly equalized by the expansion and contraction of the mercury in the barometer, from the same cause.

† As the atmosphere is almost constantly varying in its degree of density; so is the barometer, which is an accurate measure of its density, as constantly varying in its altitude, and should therefore be frequently referred to.

I am, sir,
Your obedient servant,
RD. WALKER.

Queen-street, Oxford,
Nov. 17th, 1810.

LXX. *Reflections on some Mineralogical Systems.* By R. CHENEVIX, *Esq. F.R.S. and M.R.I.A., &c. Translated entire from the French, with Notes by the Translator.*

[Continued from p. 303.]

SUPPOSED DEFECTS OF THE CRYSTALLOGRAPHICAL SYSTEM.

M. HAUY has been reproached for his principle of specification and his definition of the species: one alleging that, according to him, muriated soda and sulphated lead are of the same species; another, that the two indications of the species are often in contradiction, and that the same form of molecule does not always accompany the same chemical composition, and *vice versâ*. Some do not like the octaëdron for a primitive form, because, to preserve unity of form in the integral molecule, it is necessary to suppose empty spaces; others, in short, reproach him with the difficulty of finding all the directions of the cleavage (or the construction of the cleft), and the system also with a want of generalization; and finally, as a dernier resource, that we must return to the system of external characters.

Will it be believed that I have heard the first of these objections made by a celebrated philosopher, a professor who draws around him from all parts of the world the zealous lovers of mineralogy, and who repeats it at least once a year in his public lectures? that I have seen it printed and published in a work which passes for one of the best on the systems of this professor? It is of German origin, and proceeds, no doubt, from the circumstance that they have not yet learned to count even to two on their fingers; a great misfortune for a philosopher. Identity of form in the integral molecule,—this is one condition in order that two minerals should be of the same species; and hitherto muriated soda and sulphated lead are in this state. Identity of chemical composition,—here are two conditions, if I have rightly calculated; and muriated soda and sulphated lead are not of the same species.

M. Haüy himself has answered the second objection in a manner which leaves nothing to be added. It is true, that with the same form of integral molecule we have a different chemical composition; but let us observe under what circumstances.

There are three geometrical figures which perform the office of integral molecule: Admirable simplicity of nature, that with such slender means can compose forms in an infinite number! These figures consist of those with four sides, the least number possible to contain a solid; those with five, and those with six; all are the most simple. But they are all susceptible of an infinite variety in the dimensions of their sides and in the inclination of the faces which terminate them, although all have a fixed term of regularity towards which they tend. It is but in these terms, which in this respect are the limits, that we find the identity of the physical with a diversity in the chemical molecule. The repetition of regular forms which are the limits of others, such as the cube and regular tetraëdron in the different species, appears to me to prove nothing against the system of M. Haüy; on the contrary, it gives us cause to admire the mechanism of nature, which delights in multiplying its severe and rigorous features amidst the variety in which it indulges. The most regular forms are also the most simple; and I take it for an axiom, that it is but in approaching simplicity that we approach nature.

Let us examine if the reverse of this be equally true; that is to say, if with the same chemical composition we find a difference in the physical molecule. There is only one case well ascertained; it is that of carbonated lime and arragonite. Chemists the most celebrated in the art of analysis, Messrs. Vauquelin, Thenard, and Klaproth, have found no difference between these two minerals; and I acknowledge that I have repeated the analysis of six different specimens of arragonite, comparatively with the carbonated lime, and I am convinced of their identity of composition. M. Haüy found a difference in the form of their integral molecules. How admirable is this science. to which,—in the first years of its existence, and before that time has impressed it with those marks of rigour which continued observations invariably effect,—only one objection can be made against it

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those who profess another mineralogical belief than that of M. Haüy. It is ranked in the same species as simple carbonated lime, although it often contains but one-third carbonate of lime, and the remainder iron or manganese: evident marks also indicate an additional structure of the cleavage in the direction of the great diagonal, which leads to solids of two or more forms.

We have observed that there exist vacuums between the molecules of bodies, in which foreign matter may lodge. It may therefore happen that particles of iron or oxided manganese are deposited in the vacuums of carbonated lime: when this is the case, we have the mineral in question. But it does not thence follow that the integral molecule should change its form, as the oxided iron does not enter it, and as it, in fact, undergoes no change in its chemical composition. One of the indispensable properties of the molecule of minerals being not to vary but by two indications at once, then whatever enters not the integral molecule, although it forms a part of the mineral, should not change it: this law is constant, whether it relates to the quantity of extraneous substances, or to their nature, their form, or their tendency to crystallization. But as foreign matter can interpose itself between the molecules with which it is surrounded or enveloped, it may influence their relative dispositions; whence will result secondary forms, which shall differ according as the spaces between the molecules are empty, or more or less filled. The passage of light may also be obstructed by the interposition of opaque matter, and the colour must participate in that of the interposing substances. The molecules being enveloped may be further removed from immediate contact, and thus offer a greater facility of separation: the junctures also may

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This supposed improvement, therefore, of Haüy's arrangement, by subdividing and transposing the varieties of this subspecies, exists more in names than characters; for even those of specific gravity and of attracting the magnet depend solely on the preponderance of iron over the manganese; and sometimes the quantity of manganese is very considerably greater than that of the iron, without any difference in exterior character. To change, torture, or reject a system for such a trifling and merely apparent anomaly, would betray more ignorance of the diversity of nature than scientific experience; to attempt to modulate an otherwise complete system to such things, would be to pay more attention to exceptions than to rules, and evince nothing but the foppery of minuteness, which never can exist in minds expanded by true science. Mr. Chenevix's chemical explanation, indeed, of this phænomenon is perfectly sufficient, and more satisfactory than M. Brogniart's scheme of minute division, which may be sometimes very right and sometimes very wrong. His epithet *brunissante*, or browning, adopted from the Germans, is equally as applicable to his sparry iron as to this carbonated lime; that of Haüy, although long, is correct and intelligible,—ferro-manganesiferous carbonated lime.—Trans.

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work was finished, that of nature was not; and it may still present some specimen which shall reveal its secret. If we find that carbonated lime may be divided more than once, it thence results only that we have taken for an integral molecule that which we have been able to observe, but not that which really exists; precisely as chemistry declared that the emerald was composed of silica, alumine, iron and lime, until it was discovered that what was considered only as alumine contained also glucine, and that the iron was combined with chromium. But for this chemistry did not lose its importance. The integral molecule likewise may be found different from what has been believed, if what I have here observed should prove true; yet the general system of the molecule is unshaken.

A recent analysis, however, appears to have excited much interest in this point. Some of this pretended ferriferous carbonated lime has been found, in which there exists scarcely a trace of lime*. I readily believe it, but shall not for that renounce the method. In M. Haüy's collection there is a mineral which at one end is ferriferous carbonated lime, excessively yellow, containing iron, striated and dividing in rhombs. In extending from this end the colour fades, and the other characters which distinguish the common from the ferriferous carbonated lime become weaker till they finally disappear. Whoever ex-

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* The Wernerians, however, cannot consistently avail themselves of this defect, as the mineral still retains the same *external* characters; it is the chemists only who are entitled or qualified to decide on it, and they will not be very precipitate in pronouncing a sentence, since Mr. Davy has proved that even one per cent. of oxygen can produce effects on the external character of substances, which would serve the Wernerians not merely for a specific but even a generic difference. Should they object to the introduction of lime in the name of this mineral, they must recollect their own *holzstein*, woodstone, or petrified wood, which they have thus denominated, and made a particular species in flint genus, although they will not pretend that it contains any vegetable or woody matter; only that, like the mineral under consideration, it owes its form to that substance. Mr. Jameson, indeed, makes an apology for considering a petrification (not petrefaction, as he erroneously writes it, and which the learned Dr. Kidd applies to *incrustations*), " a particular fossil species," by alleging " that woodstone differing in its external characters from all other fossils, the justness of the Wernerian method is evident." Upon this principle he should have divided his species, as the colours, and even specific gravity of petrified oak, ash, &c. are very different. He adds, that " it receives a good polish, and serves for the same purposes as agate." I have examined many specimens of petrified wood in various countries, but have never been so fortunate as to find any that could be substituted for agate, or was susceptible of a polish even equal to coarse marble Surely the professor cannot have noticed such characters merely to make Werner's fine chemical theory of the solution and infiltration of agate less fanciful, or give an example of transition from wood to " petrifaction agate, which is *wood* penetrated with *several* of the fossils that constitute agate? "—Trans.

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we can safely say that no combination has taken place between these two substances, whatever may be the difficulty experienced in separating them. If, on the contrary, these two molecules lose the form peculiar to them, as tartrate of potash and tartaric acid, to effect another conjointly, we should conclude that they have combined to form but one. By afterwards adding some soda as a neutralizer, we may learn the true state of these principles, with respect to each other, in consulting crystallotomy to know if molecules of tartrate of potash and also those of tartrate of soda are found, or molecules of another form, which will be that of a tartrate with a double base.

The great quantity of foreign matter which has been found interposed between the physical molecules, without effecting any change in their form, has shaken the faith of many persons. The *gres* (sandstone) of Fontainbleau (quartziferous carbonated lime, Haüy), in which the law is the same as in the pure calcareous spar which sometimes accompanies it, is an example. But the degree of tendency to a regular form may be much greater in one mineral than in another, and surmount all the obstacles which the mixture of heterogeneous species could oppose to it. This is what we see in our laboratories; it is what the *gres* of Fontainbleau shows us in that of nature. The power of becoming symmetrical may vary in crystallizable substances, like the capacity of saturation in salifiable bases.

As to the octaëdron considered as a primitive form, it has been observed that sections by planes parallel to its faces, divide it again into eight tetraëdrons and six octaëdrons, and this in succession to the ultimate point; so that, to maintain the unity of the integral molecule, we must suppress in the mind one of these two figures. In order to adhere to the most simple, as that which appears to have the most just title to a place in nature, the preference has been given to the tetraëdron, in supposing that between the molecules there are empty octaëdral spaces. Geometry has found that this hypothesis embraces one third of the quantity of matter in a mineral which has this primitive figure, and that the two other thirds are empty. But this takes place precisely in the species of the calcareous genus which has the greatest specific gravity, while nothing is said of the vacuums in the other species. Here, it is said, there is a contradiction.

If we were still ignorant of all that we have learned respecting the density of bodies, this objection would be

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mitting in the molecule of fluated lime the necessary density to obviate this objection, we do not commit any indiscretion.

Now let us take the contrary hypothesis, and say that the molecules of all bodies have the same density, so that the variation of specific gravity in them all depends solely on the vacuums which exist between them. If in any body whatever the quantity of vacuum be zero, we have an absolute plenum; and the heaviest body which we know should be that which would present us with this state of things. Platina furnishes us with an example, while this collection of hypotheses contains the fact most favourable to the objection.

Chabaneau has found the specific gravity of platina to be 2·400, others 2·300, some 2·200, and I have observed it beyond 2·1; that of fluated lime is 3·191. Let us simplify the expression of these relative weights, and we shall have, in an equal volume, the quantity of matter in platina, in fluated lime, and in water :: 1 : $\frac{1}{7}$: $\frac{1}{21}$. There is then $\frac{6}{7}$ of vacuum in fluated lime, or $\frac{18}{21}$, while the hypothesis of M. Haüy notices only $\frac{14}{21}$. There remain, then, $\frac{4}{21}$ more than are wanting to combat the objection. There are also necessarily vacuums in other calcareous species. In taking platina, as above, for an example of an absolute plenum, we shall have, according to the data which the specific gravity of this body furnishes, $\frac{183}{210}$ of vacuum in carbonated lime; $\frac{178}{210}$ in phosphated as well as in fluated, and $\frac{177}{210}$ in sulphated lime. The hypothesis of M. Haüy, instead of being incompatible with what observation teaches respecting the density of fluated lime, is not sufficient to account for its lightness; and the $\frac{40}{210}$ of which it takes no notice, as well as the vacuums in other species which are passed over in silence, are so many particular cases of a verity demonstrated in physics: its great notoriety doubtless made M. Haüy believe that it was unnecessary to anticipate this objection by answering it at first. Instead, therefore, of accusing him of having carried off too much matter from fluated lime as a mineralogist, we should rather consult him as an examiner of nature (*physicien*), on the fate of nearly $\frac{4}{21}$ of which he is silent. It is difficult to conceive how any one could permit himself to make such objections, especially when we *consider the source whence they sprung.*

The divisibility of carbonated lime before mentioned, by supersections which yield molecules of two different forms, and also what we have just observed respecting the

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telescope, nor the chronometer, for they also are very difficult to execute. Let us content ourselves with dressing, sleeping, and eating, convinced that without the pendulum and the 'telescope the stars will continue their course, and bring back the hours of sleep and the restoration of our powers.

The last objection to which I shall pay any attention is that which says, briefly, " We must abandon the system of M. Haüy for that of the external characters, as the integral molecule cannot be observed in all minerals." One of the great advantages of the system of M. Haüy, one of its principal beauties, is to follow nature, and to speak as she does. Where she has finished her work in the highest manner of which it is susceptible, M. Haüy does the same; and if she produces a mineral endowed with all the characters which, according to us, compose the most perfect state, it is classed and defined as such. If she has been sometimes less rigorous in impressing her mark of perfection, the system follows the same course; while the method of external characters renders équally the honours of rigorous classification to sapphire and to the alumina of Halle. To say that we should make no use of an excellent system because cases occur where it is unavailable, is to say to a patient, Lie not on a feather bed; for, if you are deprived of it, you will be reduced to the necessity of sleeping on a board. It is to tell a man in health not to take nourishment, for if the provisions become deficient he could no longer eat.

WERNERIAN TRANSITION OR PASSAGE.

Before terminating these considerations on the species, there is a mineralogical being of which it is necessary to say a few words. It is a being which is neither of this nor that species, but belongs to all. It is not sapphire, for instance, but it resembles it; it is not ruby, but it would be perhaps if it were not something else. It is so constituted that, with a real and material existence, it lives by borrowing, as to its modifications, and puts on the characters of others. It is a hermaphrodite mineral; an infant with two fathers that both disown it; that the other kingdoms of nature reject as a monster; but that mild and easy mineralogy has received into its bosom, and called it *passage*.

There are two manners of conceiving the existence of this interesting refuse of the organized kingdoms.

Let us first suppose a mineral less hard, less brilliant, less

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* As this fact has been
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it is Nature which resumes her rights, and puts a limit to a race of monsters.

The other circumstance which gave birth to the brilliant idea of *passage*, is that which takes place when a mineral is an aggregate of two simple species: such are the heliotrope, composed of quartz and green earth; and prase, composed of quartz and green schorl (*rayonnante*) [actinolite of Kirwan and Jameson; amphibole* actinote of Haüy and Brogniart]. It is at first a very great and irreparable error to consider mixtures as species, the essence of which is simplicity.

[To be continued.]

LXXI. *Notices respecting New Books.*

THE Philosophical Transactions for the Year 1810, Part II, has just made its appearance. Its contents are:—Supplement to the First and Second Part of the Paper of Experiments for investigating the Cause of Coloured Concentric Rings between Object Glasses, and other Appearances of a similar Nature. By William Herschel, LL.D. F.R.S.—On the Parts of Trees primarily impaired by Age. In a Letter from T. A. Knight, Esq. F.R.S. to the Rt. Hon. Sir Joseph Banks, Bart. K.B. P.R.S.—On the Gizzards of Grazing Birds. By Everard Home, Esq. F.R.S.—Observations on atmospherical Refraction as it affects astronomical Observations; in a Letter from S. Groombridge, Esq. to the Rev. Nevil Maskelyne, D.D. F.R.S. Astronomer

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* The union of amphibole (hornblende) with actinote (actynolite) is another fact highly honourable to the study of crystallography. Haüy announced his opinion of their identity, which was fully confirmed by M. Laugier's analysis in the *An. d'Hist. Nat.* vol. v. p. 73.

Amphibole	*contains*	*Actinote*
Silica	42·02	50·00
Oxide of iron	22·69	11·00
Magnesia	10·90	19·25
Lime	9·80	9·75
Alumina	7·69	0·75
Oxide of manganese	1·15	0·50
Chromium	0·00	3·00
Potash	0·00	0·50
Water and loss	5·75	5·25
	100·00	100·00

The object of this analysis being to compare the nature and proportions of the constituent principles of amphibole and actinote, between which crystallography had found a perfect analogy, the comparative result was such, that it appeared necessary to blend them together under the same species of mineral, the latter presenting only some new varieties of colour of the former.—TRANS.

Royal. Communicate
of a Letter from th
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Dublin, to the Rev. N
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Mode of Breeding of
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By Everard Home, Es
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Acid, its Nature and
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F.R.S.E.—Observatio
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Mr. Myers, of the
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LXXII. *Proc*

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view to illustrate the nature, properties, and combinations of oxymuriatic gas, and its relations to inflammable bodies, as compared with those of oxygen. He also offers some general views and conclusions concerning the chemical powers of different species of matter, and the proportions in which they combine. And lastly, he concludes his paper by some observations on the impropriety of the present nomenclature, in reference to the oxymuriatic gas and its combinations; and proposes some concise modes of distinguishing these novel bodies.

Mr. Davy made some previous experiments on the combinations of potassium and sodium with oxygen; and of potash and soda with water, from which he concludes that those metals when burnt in oxygen gas are at their highest state of oxygenation—and at their lowest, when in the state of potash and soda. He also found that ignited potash contains about 16 per cent. of water, and ignited soda 22·9 per cent.

The spontaneous inflammation of the metals of the fixed alkalies in oxymuriatic gas, affords a proof of the intensity of their attractions. In these operations, no water is separated, but mere binary combinations formed; the same as those produced by igniting muriate of potash and soda. Similar compounds are formed when dry potash and soda are heated in oxymuriatic gas, and oxygen is evolved.

Mr. Davy mentions a simple mode by which pure sodium may be obtained. It is by mixing common salt which has been ignited to redness, with potassium, and exposing the whole to a red heat in a glass tube or retort; for every two parts of potassium employed, one part of sodium is obtained.

As the muriates of lime, barytes and strontites remain unaltered by any simple attractions, even at a white heat, Mr. Davy conceived that these compounds consist merely of the metallic bases of the earths in union with oxymuriatic gas, and the experiments he has made confirm this conclusion. Thus when lime, barytes, &c. were heated in oxymuriatic gas, oxygen was expelled, and substances exactly similar to the dry muriates were formed.

In operating on the metals, Mr. Davy employed green glass retorts holding from three to six cubical inches of gas, they were furnished with stop-cocks. The metal was first introduced into the retort, it was then exhausted and filled with oxymuriatic gas, the heat of a spirit lamp was employed in the processes. The products from arsenic, antimony, and bismuth, were the butters of arsenic, antimony, and bismuth; and on the addition of water, the white oxides

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LXXIII. *I*

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The difficulty which has attended the opening and closing valves of sufficient water-way, having presented great obstacles to a regularity of movement,—this objection is surmounted in this instance by the invention of a new valve, which admits apertures of large size, and is opened and closed with any required velocity, and is applicable to the passage either of water or steam.

To Mr. Tilloch.

Sir,—If you will please to communicate the following fact through the medium of your very intelligent Magazine, some of your readers will probably favour the public with their opinions upon the subject.

On my passage from Rio de Janiero to this place, on board the ship Favorite, Capt. Atkinson, on July the 14th, being in latitude 31° 56′ and longitude per account 39° 30′, at six A. M., when below, felt a very singular sensation which lasted near a minute. All below ran upon deck, feeling the ship shake as if she was passing over a wreck, rubbing very hard; or as if some very heavy body was rolled from one end of the ship to the other. To the officer and people on deck the sensation was as if the ship was going over a bar, touching, but not stopping; this lasted nearly the space of a minute. Some ran for the lead, which was hove in the shortest time possible,—no bottom: others sounded the pump,—no difference;—each looked astonished and panic-struck. The sea was smooth, a gentle breeze westerly, all sail set; the ship was loaded with coffee. At about seven A. M., an hour after, the same sensation was felt, less sensible and of shorter duration.—Could it arise from electricity?

Any of your readers favouring the public with their opinion, will much oblige

Your humble servant,
A Passenger.

METEOR SEEN IN HOLLAND.

Wall (on the Meuse), Sept. 22.

On the 19th of this month, between the hours of five and six in the evening, a luminous meteor appeared to the south, and about the distance of a quarter of a league from the small commune of Brezeau: persons who attentively examined it assert that it was nearly a quarter of an hour in collecting floating over the place where it was first seen;

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To Mr. Tilloch.

Sir,—Should the following idea be considered by you as an improvement in writing and printing numbers consisting of many digits, its insertion in your publication will confer an honour on, Sir,

Your very humble servant,

Spitalfields, 19th Nov. 1810. A Rhirtalp.

When a number such as 69,470,600,078,406,300,097 presents itself, though pointed in periods of three figures, the manner of expressing it in words does not immediately occur to the mind. The mode I would beg leave to offer as an improvement is, besides pointing it in periods of three figures, to place one accent over the seventh figure, or millions; two accents over the 13th figure, or billions; and so on, increasing the accents at every myriad, thus:—$6\overset{\prime\prime\prime}{9},470,60\overset{\prime\prime}{0},078,40\overset{\prime}{6},300,097$,—by which we can perceive at once that the two first figures denote trillions, without the usual mode of reckoning according to the Numeration Table.

EARTHQUAKE.

Extract of a letter, dated St. Michael (Azores), August 24.—"One of those dreadful phænomena never witnessed in your country has plunged many here in unspeakable wretchedness and affliction, and continues to occasion great terror to all the inhabitants of this island. On the 11th of August, at ten P. M. slight shocks of an earthquake were felt at intervals of a few minutes for four hours. During this time the inhabitants, under the influence of alarm for their personal safety as well as property, were running to and fro in the greatest distress. Between two and three a dreadful rocking was experienced throughout the whole island; several houses, unable to resist its violence, were thrown down, and many others were greatly damaged; and such persons as sought safety in the open air were dashed to the ground. Hitherto the calamity had been confined in its effects, and though great injury had been sustained, we had to congratulate ourselves on the loss of few lives; but we were yet to witness a most dreadful spectacle. On the 12th at mid-day, a hollow-rumbling sound was heard, the clouds gathered, and the wind was hushed into silence; the rocking returned, and in a few minutes after the village of Gozas, situated on a plain, comprising 22 houses, was swallowed up, and in the spot where it stood a lake of boiling water gushed forth. Many of the unfortunate inhabitants, who had previously retired to the elevated ground, beheld the sight with a degree of horror

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Mr. Ed. Browne Mr.
Mr. Mathias Mr.
Mr. Milward Mr.

Mr. Field Mr. Coat
Mr. Upton Mr. Seat

Right Hon. Marquis T
O

Right Hon. Sir J. Banks, Bart. K.B. President of the Royal Society;
Isaac Hawkins Browne, Esq. M.P. F.R.S.
James Ware, Esq.

SECRETARY,
Mr. William Chamberlaine.

SOLICITOR,
Okey Belfour, Esq.

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Jame Vere, Esq.
Sir William Watson
Okey Belfour, Esq.
William Morgan, Esq.
Charles Chevalier, Esq.
Sir Frederick Baker, Bart.

LIST OF PATENTS FOR NEW INVENTIONS.

To Edmund Griffith, of the city of Bristol, Esq. for certain improvements in the manufacture of soap for the purpose of washing with sea-water, with hard water, and with other waters.—Oct. 8, 1810.

To Richard Woodman, of Hammersmith, in the county of Middlesex, boot- and shoe-maker, for his method of manufacturing all kinds of boots, shoes, and other articles.—Oct. 8.

To Edward Manley, of Uffculme, in the county of Devon, clerk, for his apparatus for writing.—Oct. 8.

To John Fraser, collector of natural history, now of Sloane-Square, in the county of Middlesex, for his discovery of certain vegetables, and a way of preparing the same, so as they may be usefully applied in the manufacturing of hats and bonnets, chair-bottoms, and baskets, and for other articles or purposes.—Oct. 15.

To John Wheatley, of Greenwich, in the county of Kent, coach-builder, for his improved axle-tree for wheels of carriages, and also improved wrought- or cast-iron boxes and cast-iron stocks to receive the spokes of the wheels.—Oct. 15.

To Thomas Mann, of Bradford, in the county of York, stuff-merchant, for certain improvements in the construction of artificial legs.—Oct. 31.

METEORO-

By

Days of Month.	The
	8 o'Clock, Morning.
Oct. 27	44
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29	38
30	36
31	33
Nov. 1	44
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26	43

N.B. The

present instruments of surgery. They should likewise observe, with a professional eye, the various mechanical improvements which are daily taking place. Might not the circular saw be introduced in some operations? Small circular saws, cutters, or wheels with toothed edges of different sizes and thickness, might perhaps be used with effect in insulating and removing the depressed angular pieces of bone which occur in fractures of the skull. When the trephine is inadmissible, a circular cutter applied to the edge of the fracture, might, if used with proper precaution, cut away the bone with safety, and make a space sufficient to admit the elevator. Such a cutter might be turned by the hand, as great velocity would be dangerous. A method could easily be contrived to apply such an instrument with the requisite steadiness to the part.

4th. The centre point of the trephine necessarily protrudes beyond its teeth; in consequence of which, when the inexperienced operator neglects too long to remove it, the most serious effects are sure to follow. Might not this be easily prevented by having a shoulder, as mechanics term it, to surround the point, just so far down from the extremity of the point, as to permit the saw to fix itself, and no more?

5th. Would not a contrivance be useful, in trepanning the skull, to fix the head in the most favourable posture?

6th. The best shape for the points of one description of piercing instruments has, I think, never yet been exactly ascertained; and it is certainly a question of considerable importance. I mean those piercing instruments, where breadth of instrument is requisite immediately on insertion; for, as to common needles, and other small instruments merely for piercing, it is evident the more acute we make their points, the better. In some instruments, however, where a point is merely necessary for their insertion, when that point is much prolonged beyond the *efficient* part of the instrument, it becomes injurious: What point will suit such an instrument best? Is it well ascertained that the drill or shear point is the most advantageous? If the point formed an acute angle, sloped to one side, would it not answer as well? What is the proper angle for such a point? Mechanics pierce brass, copper, and steel with drills of different shapes: May not there be an appropriate point for piercing animal membranes? The French discovered by experiments (fatal experiments!) that the descending blade of the guillotine cut best when sloped to a certain angle. However confident we may be in our opinion, to experiment we should always have recourse, when possible; and in satisfying our present doubts, it is very possible. An apparatus might easily be contrived for the purpose. All that is requisite is, a piece of brass, about four inches long and two inches square, supported by two or more feet, and perforated longitudinally, to admit a thick steel pin, which pin is to be fitted to the perforation in the brass, so as to move very freely, and to exceed the brass about two inches or more in length. On the upper end of this pin, I would have a small flat piece of brass fixed, capable of holding weights; and in the lower end, a hole made, into which the different shaped points on which we are desirous to make experiments are to be fixed, similarly to the shifting feet of common compasses. Immediately below this pin I would place a species of drum, consisting of a small box, with bladder or other elastic substance stretched over it to imitate the animal tunics. Now it is evident, that by placing different weights on the upper end of the pin, until the point we are experimenting on pierce the stretched bladder, we may be able exactly to appreciate the comparative advantage of the different kinds of point.

7th. The operation of couching frequently fails, from the cataract or opake crystalline lens being of a soft consistency; which the couching needle, instead of depressing, passes through and divides. If the broad part which is, I believe, frequently used in depression, were made concave so as to fit the convex edge of the lens, might it not in some degree remedy this evil?

8th. Considering that the majority of calculi in the bladder are more or less spherical, it appears to me that the forceps now used in lithotomy is not of the most advantageous construction. By each side of the beak of the usual forceps being concave, with an oval hole at the bottom of the concavity, similar to the forceps once used in extracting polypi, the edges of which hole, and the sides of the concavity, might have teeth, Would it not be more likely to lay hold of even an irregularly spherical calculus than the forceps at present used, with toothed beaks and flat sides? and which seems better contrived for crushing a soft calculus to pieces, than for holding it fast and withdrawing it whole.

9th. Grown bold by practice, I shall now venture, as my last suggestion, to propose another kind of forceps for extracting the stone. Let us suppose a forceps with the beaks formed of two narrow elliptical rims, jointed so as in some degree, by the pressure upon a calculus, to conform themselves to its size. To the edges of these rims I would at-

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potassium* about 40·5; and from an experiment which I have detailed in the last Bakerian lecture, on the combustion of potassium in muriatic acid gas, oxymuriatic acid will be represented by 32·9, and muriatic acid gas, of course, by 33·9; and this estimation agrees with the specific gravity of oxymuriatic acid gas, and muriatic acid gas. From my experiments, 100 cubical inches of oxymuriatic acid gas weigh, the reductions being made for the mean temperature and pressure, 74·5 grains; whereas by estimation they should weigh 74·6. Muriatic acid gas I find weighs, under like circumstances, in the quantity of 100 cubic inches, 39 grains; by estimation it should weigh 38·4 grains.

It is easy from these data, knowing the composition of any dry muriate, to ascertain the quantity of oxide or of acid it would furnish by the action of water, and consequently the quantity of oxygen with which the inflammable matter will combine†.

In considering the dry muriates, as compounds of oxymuriatic acid and inflammable bodies; the argument that I have used in the last Bakerian lecture, to show that potassium does not form hydrate of potash by combustion, is considerably strengthened; for from the quantity of oxymuriatic acid the metal requires to produce a muriate, it seems to be shown that it is the simplest known form of the alkaline matter. This I think approaches to an experimentum crucis. Potash made by alcohol, and that has

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* Supposing potash to contain nearly 15·6 per cent. of oxygen.

† I have stated in the last Bakerian lecture, that during the decomposition of the amalgam from ammonia, one in volume of hydrogen to two of ammonia is evolved: it is remarkable, that whatever theory of the nature of this extraordinary compound be adopted, there will be a happy coincidence as to definite proportions. If it be supposed that the hydrogen arises from the decomposition of water; then the oxygen that must be assumed to exist in ammonia, will be exactly sufficient to neutralize the hydrogen, in an equal volume of muriatic acid; or if it be said that ammonium is a compound of two of ammonia and one of hydrogen in volume, then equal volumes of muriatic acid gas and ammonia will produce the same compound as oxymuriatic acid and ammonium, supposing they could be immediately combined. I once thought that the phænomena of metallization might be explained according to a modified phlogistic theory, by supposing three different classes of metallic bodies: First, The metal of ammonia, in which hydrogen was so loosely combinned as to be separable with great ease, and which, in consequence of the small affinity of the basis for water, it had little tendency to combine with oxygen. The second, the metals of the alkalies and alkaline earths, in which the hydrogen was more firmly combined, but in combustion, forming water capable of being separated from the basis. And, thirdly, the metals of the earths and common metals, in which the hydrogen was more intimately combined; producing by union with oxygen, water not separable by any new attractions. The phænomena of the action of potassium and sodium upon muriatic acid, referred to in the text, seem however to overturn these speculations so far as they concern the metals from the fixed alkalies.

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As iron has a strong affinity for oxymuriatic acid, I attempted to procure soda by passing steam over a mixture of iron filings, and muriate of soda intensely heated: and in this way I succeeded in decomposing some of the salt: hydrogen came over: a little hydrate of soda was formed; and muriate of iron was produced.

It does not seem improbable, supposing the views that have beeen developed accurate, that, by complex affinities, even potassium and sodium in their metallic form may be procured from their oxymuriatic combinations: for this purpose the oxymuriatic acid should be attracted by one substance, and the alkaline metals by another; and such bodies should be selected for the experiment, as would produce compounds differing considerably in degree of volatility.

I cannot conclude the subject of the application of these doctrines, without asking permission to direct the attention of the Society to some of the theoretical relations of the facts noticed in the preceding pages.

That a body principally composed of oxymuriatic acid and ammonia, two substances which have been generally conceived incapable of existing together, should be so difficult of decomposition, as to be scarcely affected by any of the agents of chemistry, is a phænomenon of a perfectly new kind. Three bodies, two of which are permanent gases, and the other of which is considerably volatile, form in this instance a substance neither fusible nor volatile, at a white heat. It could not have been expected that ammonia would remain fixed at such a temperature; but that it should remain fixed in combination with oxymuriatic acid, would have appeared incredible, according to all the existing analogies of chemistry. The experiments on which these conclusions are founded, are, however, uniform in their results: and it is easy to repeat them. They seem to show, that the common chemical proposition, that complexity of composition is uniformly connected with facility of decomposition, is not well founded. The compound of oxymuriatic acid, phosphorus, and ammonia, resembles an oxide, such as silex, or that of columbium in its general chemical characters, and is as refractory when treated by common re-agents; and except by the effects of combustion, or the agency of fused potash, its nature could not be detected by any of the usual methods of analysis. Is it not likely, reasoning from these circumstances, that many of the substances, now supposed to be elementary, may be reduced into simpler forms of matter? and that an intense attrac-

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that by employing crystallized native sulphur, which had been recently sublimed in nitrogen, I should avoid the presence of any foreign matter, yet I am inclined to believe that this is not the case; for by subliming some similar sulphur in nitrogen, I find that litmus paper placed in the upper part of the retort is slightly reddened.

When potassium is made to unite with sulphur, if the retort employed is not lined with sulphur, some of the potassium is destroyed by acting upon the glass; and when large quantities of sulphur are used, it is very difficult to decompose the whole of the sulphuret of potassium by an acid; sulphuretted hydrogen likewise is soluble in muriatic acid: and this circumstance led me to under-rate the quantity of sulphuretted hydrogen given off in experiments of this kind *.

In acting upon sulphuretted hydrogen by potassium in my early experiments, I used large quantities of the gas and of the metal; and in these cases I have reason to believe that the violence of the combustion occasioned the decomposition of a considerable quantity of the gas; and, in consequence, led mé to form erroneous conclusions concerning the nature of this curious operation.

In all late experiments in which sulphur or sulphuretted hydrogen was concerned, I have used muriatic acid saturated with sulphuretted hydrogen over mercury. I have employed sulphur distilled from iron pyrites in vacuo, which did not in the slightest degree affect litmus paper, and I have combined it with potassium in retorts of green-glass, or plate-glass lined with sulphur and filled with very pure nitrogen or hydrogen. In making potassium act upon sulphuretted hydrogen, I have employed the gas only in the quantities of from one to three cubical inches, and have made the combination in narrow curved tubes of green-glass over dry mercury. With all these precautions, and after having made a great number of experiments, I am not able to gain perfectly uniform results. Yet there is a sufficient correspondence between them to enable me to form conclusions, which I may venture to say cannot be far from the truth.

When one grain of potassium, which would give by the

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* This circumstance has been pointed out by MM. Gay Lussac and Thenard, in a paper printed in the *Journal de Physique* for December, in which these gentlemen endeavour to show that, whether potassium has been acted upon by large or small quantities of sulphur, and under all circumstances, it evolves a quantity of gas exactly equal to that which it produces by the action of water. I have been able to gain no results so precise on this subject. I have in another place (the same journal in which their memoir has appeared) offered some observations on their inquiries.

action of water about one cubical inch and $\frac{1}{10}$ of hydrogen is made to act upon about half a grain of sulphur, some sulphur sublimes during the combination, which always takes place with heat and light, and from $\frac{1}{14}$ to $\frac{1}{10}$ of a cubical inch of sulphuretted hydrogen is evolved. The compound acted on by muriatic acid, saturated with sulphuretted hydrogen, affords from $\frac{9}{10}$ to $\frac{11}{10}$ of a cubical inch of pure sulphuretted hydrogen.

When more sulphur is used so as to be from twice to ten times the weight of the potassium, the quantity of sulphuretted hydrogen evolved by the action of the acid, is from $\frac{7}{10}$ to $\frac{9}{10}$; but if heat be applied to the combination, so as to drive off the superfluous sulphur, the quantity of gas collected is very little inferior to that produced from the combination in which a small proportion of sulphur is used; and I am inclined to believe, from the phænomena presented in a great number of experiments, that sulphur and potassium, when heated together under common circumstances, combine only in one proportion, in which the metal is to the sulphur nearly as three to one in weight; and in which the quantities are such that the compound burns into neutral sulphate of potash.

When a grain of potassium is made to act upon about 1·1 cubical inches of sulphuretted hydrogen, all the hydrogen is set free, and a sulphuret of potassium containing one-fourth of sulphur is formed, exactly the same as that produced by the immediate combination of sulphur and the metal.

When sulphuretted hydrogen is employed in larger quantities, there is an absorption of this gas, and a volume is taken up about equal to the quantity of hydrogen disengaged, and a compound of sulphuretted hydrogen and sulphuret of potash is formed, which gives sulphuretted hydrogen by the action of an acid, nearly double in quantity to that given by the sulphuret of potassium.

From a number of experiments I am inclined to believe that potassium and phosphorus, in whatever quantities they are heated together, combine only in one proportion, a grain of potassium requiring about $\frac{3}{8}$ of a grain of phosphorus to form a phosphuret; which when acted upon by muriatic acid, produces from $\frac{9}{10}$ to $\frac{10}{10}$ of a cubical inch of phosphuretted hydrogen.

Half a grain of potassium decomposes nearly three cubical inches of phosphuretted hydrogen, and sets free rather more than four cubical inches of hydrogen; and the phosphuret formed seems to be of the same kind as that produced by direct combination of the metal with phosphorus.

If, according to Mr. Dalton's ideas of proportion, the quantity in which sulphur enters into its combinations were to be deduced from its union with potassium, in which it seems to form about one-fourth the weight of the compound, the number representing it would be 13·5. I have lately weighed sulphuretted hydrogen and sulphureous acid gas, with great care: the specific gravity of the first at mean temperature and pressure, from my experiments, is 10645, which differs very little from the estimation of Mr. Kirwan: that of sulphureous acid gas I find is 20967. Sulphuretted hydrogen, as I have shown, contains an equal volume of hydrogen; and on this datum the number representing sulphur is 13·4. I have never been able to burn sulphur in oxygen without forming sulphuric acid in small quantities; but in several experiments I have obtained from 92 to 98 parts of sulphureous acid from 100 of oxygen in volume; from which I am inclined to believe, that sulphureous acid consists of sulphur dissolved in an equal volume of oxygen; which would give the number as 13·7 * nearly, considering the acid gas as containing one proportion of sulphur, and two of oxygen; and these estimations do not differ from each other materially.

I have made several experiments on the combustion of phosphorus in oxygen gas. From the most accurate, I am inclined to conclude that 25 of phosphorus absorb in combustion about 34 of oxygen in weight: and considering phosphoric acid as composed of three proportions of oxygen and one of phosphorus, the number representing phosphorus will be about 16·5, which is not very remote from the number that may be deduced from the composition of phosphuret of potassium.

The numbers which represent the proportions in which sulphur and phosphorus unite with other bodies, are such, as do not exclude the existence of combined portions of oxygen and hydrogen in their constitution; but it may be questioned, whether the opinion which I formed, that the

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* The estimation from the composition of sulphuretted hydrogen, must be considered as most accurate, and that from the formation of the sulphuret of potassium as least accurate: for it was only by combining sulphur and potassium in small proportions, and ascertaining in what cases uncombined sulphur could be distilled from the compound, that I gained my conclusions concerning the composition of the sulphuret of potassium.

In the last Bakerian lecture, I have estimated the specific gravity of sulphuretted hydrogen at 35 grains the 100 cubical inches, which was not far from the mean between the estimations of Mr. Kirwan and Mr. Thenard. According to this last experiment, sulphuretted hydrogen is composed of one proportion of hydrogen, represented by 1, and one of sulphur represented by 13·4.

inflammable gas disengaged from them by electricity, is necessary to the peculiar form in which these bodies exist, is not erroneous. Phosphorus, as I have stated in the last Bakerian lecture, is capable of forming a solid hydruret: and a part of the sulphur distilled from iron pyrites is usually of a soft consistence, and emits the smell of sulphuretted hydrogen, and probably contains that body. It is not unlikely, that in all cases, phosphorus and sulphur contain small quantities of the hydrurets of phosphorus and sulphur; and the production of a minute portion of sulphuric acid in the slow combustion of sulphur, is probably connected with the production of water. Though the pure oxides of sulphur and phosphorus have never been obtained, yet from the doctrine of definite proportions, these bodies ought, under certain circumstances, to be formed. And I am inclined to believe, that they sometimes exist in minute quantities in common phosphorus and sulphur, and with hydrogen give to them their variable properties.

The colours of different specimens of phosphorus, as well as of sulphur, differ considerably; the red colour of phosphorus as it is commonly prepared, is probably owing to a slight mixture of oxide. Common roll sulphur is of a very pale yellow, the Sicilian sulphur of an orange colour, and the sulphur distilled from iron pyrites in vacuo, which arose in the last period of the process, of a pale yellowish-green colour. All the late experiments that I have made, as well as my former researches, induce me to suspect a notable proportion of oxygen in Sicilian sulphur, which is probably owing to the presence of oxide of sulphur, which may give rise to sulphuric acid in distillation, or to sulphuric acid itself.

Conceiving that, if definite proportions of oxygen and hydrogen existed in sulphur and phosphorus, they ought to be manifested in the agency of oxymuriatic acid gas on these bodies, I made some experiments on the results of these operations. In the first trial, on the combination of sulphur with oxymuriatic acid gas, I employed five grains of roll sulphur, and admitted the gas into the exhausted retort, from a vessel in which it had been in contact with warm water: in this case more than a half a cubical inch of oxygen gas, and nearly two cubical inches of muriatic acid gas, were produced. Suspecting in this instance, that aqueous vapour had been decomposed, I employed cold water in the next experiment, and dried the gas by muriate of lime: in this case, though Sicilian sulphur was used, no oxygen gas was evolved; and not a half a cubical inch of muriatic

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I inquired if it was the common or the white Polar bear [*Ursus maritimus*], and was answered, "It may be the *passage* from one to the other." (*Es mag wohl ein übergang seyn.*) The mineral kingdom, in appropriating this unfortunate bear, has rendered it the subject of an absurdity.

With the word passage we may associate two other favourites in the same class; *modification* and *tendence*[*]. Formerly manganese was considered as a modification of iron; nickel, cobalt, lime, magnesia, the earths and alkalis, almost all were modifications. It might have been said that in modifying nature produced all. When a man is afraid of saying he *does not know*, he speaks of modification. But philosophy, in appropriating to itself the sciences, has banished this fear; and, in fact, what are all these pretended modifications, but modifications of our ignorance?

In the same cabinet of petrifications I saw a disciple of the transcendent philosophy who admired each specimen, was enraptured with a lichen, and in ecstasy before a fish. "You believe," said he, "that these are real impressions of animals and plants. No; they are *tendencies* in nature to form them; *tendencies* to organization,—trials." Conducting him gently near a beautiful piece of Florence marble, "Behold," said I, "a *tendency* in nature to build ruins." I also demonstrated to him, by graphical granite, that nature had a *tendency* to write!

The philosopher supported his opinions on the circumstance that among the petrifications we find natural species which no longer exist: now, it is contrary to the system of dualism that a species should be extinguished, as then the sum of all the quantities in the universe would be no longer equal to zero. I observed to him that these species might be concealed for the moment in caverns. Yet he occupies a distinguished place in the mines of Germany, and will soon appear before the world in the character of an author. How little honourable are these dreams, of which transcendentalism is so proud, even to human weakness!

SYSTEMATIC PRINCIPLES OF CLASSIFICATION.

Besides the division into species, five or six other general ones are admitted in mineralogy (Emmerling, p. 27, vol. i. 2d edition; and Brochant, vol. i, p. 45), classes, genera, subspecies, &c. There are, it is said, as many classes as fundamental principles, (*grund bestandtheile*) marking, and predominating in, the combination of minerals; the earths,

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[*] To these may be added Mr. Jameson's *English* terms of *flœtz, suite, formation suite, drusy, &c. &c.*—Trans.

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DIAGNOSTIC OR DESCRIPTIVE MINERALOGY

We shall now proceed to the second part of mineralogy; to the means which assist us in the diagnosis or art of knowing minerals. This will comprehend the art of making them known to others, or that of describing them.

M. Werner has divided the diagnostic characters into, 1st, external characters; 2d, chemical or internal characters; 3d, physical characters; and, 4th, empirical characters. Perhaps it may surprise some to see the latter epithet thus confined to only a part of this system.

The preference given by M. Werner to external characters is manifest from what follows: (Brochant, Introduction, p. 30.) He examines these characters corresponding to the five following questions: "What are the characters which always manifest themselves, and in all minerals? These are the external characters and the chemical characters, although the smallness of the specimens often prevents the latter from being discovered. (Is there not here a little contradiction in the characters which *always manifest* themselves, and that *often* cannot be *discovered*?)—What are those which most certainly indicate an essential difference? The chemical characters; nevertheless, the external characters equally indicate the state of aggregation —What are those which we can determine more exactly? The external characters, because the others require delicate operations.—What are the most easily and most promptly found? The external characters, because they immediately strike our senses.—What are those which we can discover without destroying the mineral? The external characters."

M. Werner principally employs but two chemical characters; fusibility by the blow-pipe, and the proof of acids. The physical properties which he mentions are electricity, magnetism and phosphorescence, with their modifications; and even the indication of these characters has no other object but to complete the description of the minerals. In the

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to exculpate him[*]. Among the metals, indeed, he considers it as of great weight, and in this he is supported by chemistry. It is because the colour attracts the sight, that sense which first informs us of the presence of objects in general, that it occupies the first place. The other universal external characters are cohesion, unctuosity, coldness, gravity, smell, and taste. They are called universal, because they belong to all minerals. It must be confessed, however, that among them there are some which are of very little importance, and merit slight attention. But specific gravity is not of this number, and it will not be uninteresting to see the manner in which it is treated.

A good hydrostatic balance or an areometer is all that is necessary to take the specific gravity of a body, and the operation is one of the most easy. Only a little patience, less knowledge, and no reasoning are necessary to succeed. But Werner banishes all exact modes, and says in general, that a body *swims* on water; that it is *light* when, water being 1,000, it does not weigh 2,000; *moderately* heavy, if from 2,000 to 4,000; *heavy*, from 4,000 to 6,000; and *very heavy*, if above 6,000. All that we can say from these for-

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[*] This is candid and liberal, becoming a man of science: But how does Werner's pupil estimate this character? "In giving (says Mr. Jam. vol. i.) an account of the crystallization of a mineral, we mention its fundamental figure or figures, describe their varieties, and arrange them according to their natural alliances. *Colour*, which is a very *important character*, must also be treated in a similar manner: the species and varieties must be correctly determined, and arranged according to their affinities with each other: otherwise, particularly in minerals possessing extensive *suites* of colour, as diamond and sapphire, it would be very difficult to recollect them, and when remembered would not convey to the mind a very *distinct picture* of this *highly interesting* character. (Here the truth has transpired involuntarily.) I have therefore been careful in the descriptions to determine the colours with precision, and to arrange them as much as possible in a natural order. In the treatise of Haüy, the colours are not arranged, and very seldom accurately determined: this is the case, although not in so great a degree, with a more useful work, The Mineralogy of Brochant." It is true, the colours are not arranged by Haüy; as he, like a man of real science, treats them as purely *accidental* characters. But what is the intrinsic value of the arranged "suites of colours?" There are perhaps no two persons living who have identically the same ideas of colours, still less can any two equally find terms to describe their own notions of the matter. It follows then that each individual will have his peculiar "suite of colours," and that this "suite" must be ranked with the "mineralogical instinct" of the Wernerians. I have seen a German, a French, an Italian, a Spanish, a Portuguese, and an English, mineralogist make the experiment together; each described separately his own ideas of the colour of a certain mineral in his native and in all the other languages: the descriptions were then compared, first with respect to the individual and nation, and next with respect to the six languages; and the disparity was such as would make any delicate mind feel ashamed of the system built on such a sandy and indefinite basis. Yet Werner has not hesitated to give his idea of the colour of a mineral as a name to it!—TRANS.

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posed of two bent faces, but they result from an infinite number of planes. The character of simplicity therefore is totally wanting in this choice.

Still, however, there are many more objections to this method. There is scarcely any figure which I have not heard considered in several points of view. The dodecaëdron with rhomboidal faces has been sometimes regarded as a hexaëdral prism, terminated at each extremity by a triëdral pyramid.

The hexaëdron appears entirely useless, as besides this figure there may be two modes of considering all the crystals which belong to it. The cube, for instance, is a hexaëdron, but at the same time it is a quadrangular prism with square faces. The rhomboid is also a hexaëdron and a quadrangular prism, with rhomboidal faces; and every tetraëdral prism terminated by planes as bases is a hexaëdron. Moreover, these figures may be considered as two mutilated triëdral pyramids, united, it is true, base to base, with the edge against the face. Here then is a crystal which belongs to three different species of principal forms; and such is the influence of this character in the specification of minerals, that the same mineral may very well belong to three species in the oryctognostic system.

The table is nothing but a prism extremely shortened. The geometer knows as well as any other what a table is: but I suspect that from Archimedes to Newton; from the first who failed in squaring the circle, till the learned German who told me that he had discovered a fourth dimension in space,—no geometer has treated it as a geometrical figure. This invention is purely mineralogical. But where does the prism finish, and the table commence? Is there a point where a crystal being no longer a prism is not yet a table? I do not see why the table should not be ranked among the imitative forms, as the club, bush, comb, mirror, and other usual instruments.

WERNERIAN PRETENSIONS TO THE DISCOVERY OF THE INTEGRAL MOLECULE.

There is an article in the external characters of Werner, which at first seems to have some resemblance to the form of Haüy's integral molecule: I mean what relates to the lamellated fracture. After having spoken of the perfection, imperfection, &c. of the laminæ, their direction and their form, he speaks of the structure of the laminæ (lamination), or of the cleavage (*durchgang der blatter*), and says that it may be double, triple, quadruple, and sextuple. If it were wished to enter into all the details of this subject, we could find nothing more proper to demonstrate the weakness and futility of the system of external characters; but it is not, in fact, worthy of attention. Let us take only two instances. Mica is given as a mineral which has only a single direction of cleavage; this supposes two faces terminated by planes. But two planes are not sufficient to contain a solid. What then terminates the other faces of mica? This is what Haüy found in discovering other directions of the laminæ, by which he was led to determine the primitive form and integral molecule of mica, which is a right quadrangular prism whose bases are rhombs. Consequently there are three directions of the cleavage, and each of these three has another parallel to it, whence result six parallel faces two and two, or a parallelopiped. According to Werner, hyacinth has but two directions of cleavage. Haüy found its primitive form an octaëdron with isoscele triangles, and consequently its integral molecule a regular tetraëdron, and four directions of cleavage. Werner stopped his researches where his senses abandoned him. Haüy has availed himself of all the means which a profound knowledge of the different branches of the natural sciences has put in his power; and in throwing a strong light on certain minerals, he has rendered sensible the fissures which could not otherwise have been perceived.

Some persons, indeed, have pretended to infer from what Werner says respecting the property of cleavage, that he also knew the form of the integral molecule, but having perceived its futility as a principle of classification, he abandoned the idea. Two notes (p. 28 and 127 of the French translation), in his Treatise on External Characters, have been pointed out as announcing clearly his opinion. I have been able to see nothing in the first, except that animals and vegetables have different parts, which we call organs, and that the separation of those parts destroys the animal or vegetable, while we can divide a mineral into as many small parcels as we please, without its ceasing to be the same mineral. But if we destroy its composition, then the mineral is destroyed. It is not therefore doubtful that their relations consist in their composition. In p. 31, there is the following remarkable but just observation: "The systems of those who have wished to *arrange* fossils by their external characters * have already furnished a proof of the inconvenience

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* I have been assured by an old pupil and relative of Werner, that it was not originally his intention to form any system of mineralogy on external characters,

LXXVII. *Memoir on the Diminution of the Obliquity of the Ecliptic, as resulting from ancient Observations. By M.* LAPLACE. *Translated from the* "Connoissance des Tems for 1811" *by* THOMAS FIRMINGER, *Esq.*

To Mr. Tilloch.

SIR, THE variation of the obliquity of the ecliptic having been a phænomenon in astronomy of a nature to engage the most lively interest of those who have made this sublime and useful science the subject of their study, and as its investigation has never been fully developed till the appearance of that profound work the *Mécanique Céleste* of M. Laplace, I have no doubt the following comparison of ancient observations with the deductions derived from his formula will be highly interesting to many of your readers. The article is taken from the *Connoissance des Tems* for the year 1811; and the only apology offered for its translation is the extreme scarcity of that work in this country: it was drawn up by the profound mathematician and philosopher above mentioned, with a view to compare his deductions with the actual state of the system at an interval of time as great as observations of sufficient accuracy would admit; and the coincidence, taking into consideration the imperfect state of science in those ages, is remarkably striking. It presents to us not only one of the most undeniable proofs of the Newtonian principle of gravitation, a fabric on which the whole of the *Mécanique Céleste* is founded, but furnishes the historian with facts which give additional credit to the faithfulness of the narration. On this as well as on other occasions we have a right to form our opinion from analogy. We see the recorded observations agree, as nearly as the state of science in those days will admit, with what theory has assigned to them; and as they do not make a separate history of themselves, but are coupled with the history of the times in which they were made, the known truth of the one gives a satisfaction to the mind in appreciating the value to be affixed to the other. If we look to the account given in the Lunar Tables published by the Board of Longitude in France, we shall find those tables were compiled principally from the astronomical observations made in the Royal Observatory at Greenwich; and not only the epocha, but the present state of diminution in the obliquity of the ecliptic has been in a great measure determined from them: we shall not, therefore, be surprised to find, hereafter, a nearer coincidence in the actual state of the ancient observations, and the deductions drawn from theory, when we possess, as we hope soon to do, the means of settling this epocha and actual state of diminution at the present period. The grand mural circle now making for the Royal Observatory, by Mr. Troughton, will, it is expected, be in readiness for observation early in the ensuing year; and we cannot doubt but that the first object to be determined with it, will be the settling of the above-mentioned data, so essential in the theory and practice of astronomy. The observations of Mr. Pond have clearly tended to show errors in the mural quadrants at Greenwich of considerable magnitude; and whatever accuracy these instruments might originally possess, we have now no hesitation, from a comparison of contemporary observations, not only of Mr. Pond, but of Mr. Groombridge, Dr. Brinkly, &c., to express our satisfaction on this point. Indeed it is not difficult to account for the above-mentioned errors, when we take into consideration the construction of the mural quadrants, and of the observatory in which they are contained. These instruments are freely suspended, in a line nearly with their centre of gravity, by two pins from a large stone pier; the upper and lower parts are therefore very differently affected by every change of temperature in the atmosphere: for instance, if we suppose an increase of heat to be equally diffused over the whole instrument; the upper part (for we must consider the expansion as taking place from the point of suspension) will be less affected than the lower, it having the action of gravity counteracting the force of expansion, whilst the lower part is assisted in its expansion from the same force of gravity, and *vice versâ;* or, in other words, with its own weight, which in these instruments is very great, the whole I think amounting to nearly 1000 pounds each. We shall therefore observe, that it is scarcely possible for these instruments to have retained their original accuracy for but a very short space of time after they were first put up: the change however might be inperceptible for several years; but as we now know that metals when continually acted upon, do not retain their first figure except where the parts are duly balanced, we can easily see how the change of figure must necessarily happen. In running our eye over the observations made by Dr. Bradley with Bird's mural quadrant, we find frequent mention, when observing the stars in the night-time, and particularly when the difference in the temperature of the atmosphere within and without was very considerable,

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brother of Vou-vang, in the town of Loyang. According to tradition, a gnomon of eight feet cast at noon a shadow one foot five inches long at the summer solstice. This shadow is mentioned in the ancient book of Tcheou-li, and in other books, and the authors of the Han consider the observation as incontestable.

" Loyang is the town of Hon-an-fou in Hon-an. According to Father Regis's observation, this town is placed at the latitude of 34° 46′ 15″. Father Demaille observed, together with Father Regis, as well at Cai-fong-fon as at Hang-tcheou.

" A shadow of one foot five inches from a gnomon eight feet long, gives a latitude of near 34° 22′, supposing the declination of the ecliptic 23° 29′. Tcheou-Kong governed the empire for his nephew in the year 1100 before Christ; and it was he that caused the imperial palace to be built at Loyang, which was a second court of Tcheou's empire. Therefore, if we were to admit a declination of 29° 55′ at the time of the observation, the latitude would be 34° 48′ 51″; which is remarkable.

" It was again a tradition, that in the winter solstice Tcheou-Kong observed with the same gnomon a shadow of 13 feet. This tradition is not so certain as the former. This shadow would give a true altitude for the sun's centre of 31° 18′ 42″. The summer shadow gives 79° 7′ 11″;—difference 47° 48′ 49″; half of which, 23° 54′ 24″ 30‴, would be the ecliptic's obliquity; which is worthy of remark. If the calculation of the latitude was made from the shadow at the winter solstice, supposing the declination 23° 29′, it would give a much more northerly latitude than what the altitude in the summer solstice gives."

In vol. ii. p. 21, of his History of Chinese Astronomy, published by Father Sauchet, Father Gaubil attributes the same observation to the authors of the Astronomy of Sfefen in the said town of Loyang. But in the manuscript I have just quoted, he relates what follows, (*Conn. des Tems* 1809, p. 394.)

The authors of Sfefen's Treatise of Astronomy have noticed for Loyang at the two solstices, the shadows observed by Tcheou-Kong, and recorded in the first observation. These authors have given shadows for the other days of the year in the equinoxes. These shadows are so faulty that no dependance can be placed on the observations. The authors no doubt considered Tcheou-Kong's observation as unreformable.

" In several treatises of Chinese astronomy, the shadows in the solstices at Loyang, attributed to Tcheou-Kong, are first set down; after which rules are given to add to, or subtract from, the length of these shadows, according as the places are further north or south than Loyang. What I here mention is clearly explained in some of the said works; but in others, the editors have not been careful to give the rules for the increase or decrease of the shadows observed by Tcheou-Kong, for application to places further north or south; whence it arises that in calendars for Nanking or Hin-tcheou, or other towns, the shadows are given for Loyang only."

From the foregoing, it appears to me that no doubt can be entertained of the quoted observation not wholly belonging to Tcheou-Kong. The learned Freret has calculated this important observation in the third part of his excellent Dissertation on the Certainty and Antiquity of the Chinese Chronology. This is what he says:

"The most ancient observation of the solstices that is known with certainty is prince Tcheou-Kong's, brother to Vou-vang the founder of the dynasty Tcheou. Tcheou-Kong was regent of the empire from the year 1104 to the year 1098. The observation was made in one of these six years. The precise date of the observation for the time of the cycle and moon is not marked, but the place of observation and length of shadows are known. This detail is related in the Tcheou-li, which is a part of the Li-ki or Book of Rites.

"A gnomon was made use of, of eight feet Chinese: at the summer solstice the shadow was one foot five-tenths, and in the winter it was 13 feet; which gives for the obliquity of the ecliptic 23° 54′ 14″; the same quantity nearly as was supposed by the ancient Greek astronomers Pytheas, Eratosthenes, Hipparchus, and Ptolomy.

"The altitude of the pole at Loyang, (place of the observation) as determined by the altitude of the sun above the horizon and by the resulting obliquity of the ecliptic, is found 34° 47′ 33″. Regis and Mailla, by an observation made with accurate instruments, have found it 34° 46′ 15″. By the obliquity of 23,29 as supposed by our modern astronomers, Loyang would be placed at 34° 32′, differing only 15′ 13″; which gives room to presume that the obliquity of the ecliptic must have changed.

"The observation of Tcheou-Kong was made at a time anterior to Solomon's reign, and about the war of Troy. Its exactness proves that observations must have been made in China several centuries back."

Freret's calculations want a slight correction. By rectifying them, and allowing for the refraction and the parallax of the sun supposed to be 8″,7, I find 79° 22′ 39″,6 for the altitude of the superior edge of the sun's disk at the summer solstice, and of 31° 35′ 1″,8 for that of the said edge in the winter solstice. By subtracting the apparent semidiameter of the sun at the two solstices, which I find to be 15′ 47″,7 and 16′ 14″,3 respectively, the corresponding altitudes to the centre will be 79° 6′ 51″,9, and 31° 18′ 47″,5, which gives 23° 54′ 2″,2 for the obliquity of the ecliptic, and 34° 47′ 10″ for the polar altitude; which, being nearly a mean between the three observations of the missionaries, proves the accuracy of Tcheou-Kong's determinations.

Freret by certain and ingenious calculations had in the same dissertation fixed the epocha of Tcheou-Kong's regency between the year 1098 and 1104, before our æra. I shall observe that in this respect he agrees perfectly with Father Gaubil. I shall then suppose that these observations were made in the year 1100 before our æra. I have given in the 3d vol. of my *Méc. Cel.* b. vi. ch. 12, a formula by which the obliquity of the ecliptic may be determined for a very distant period: and t expressing a number of years elapsed since 1750, the value of this obliquity in decimal degrees will be

$$26^\circ{,}0796 - 3676''{,}61 - \cos(t\,43''{,}0446) - 10330{,}4\ \sin(t\,99''{,}1227)$$

whereby $t = -2850$, which gives in decimal degrees the corresponding obliquity of the ecliptic = 26°,51611, or in ordinary degrees = 25° 51′ 53″; which must be increased about 5″, because the obliquity of the ecliptic in 1750 exceeded to that amount the quantity used in the preceding formula: thus 1100 years before our æra, the obliquity of the ecliptic was 23° 51′ 58″,—a result which only differs 2′ 4″ from that given by the observed lengths of the gnomon shadow in the two solstices. A more perfect coincidence cannot be wished for, if allowance be made for the uncertainty attending this sort of observation, owing particularly to the penumbra which renders the shadow ill-defined.

If, together with Father Gaubil, the observation alone of the summer solstice was taken into account, and the polar altitude at Loyang was supposed with him to be 34° 43′ 15″, by subtracting its complement 55° 16′ 45″ from the altitude 79° 6′ 52″ of the sun's centre, determined by the length of the shadow in the summer solstice, the obliquity of the ecliptic would be 23° 50′ 7″. The result of my formula is very nearly a mean between that and the obliquity

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B	448·6063	8·366
Bb	429·3227	68·1522
A	401·2438	7·1628
*	375·	6·9675
G	358·8838	6·6883
*	335·4110	6·2470
F	320·9972	50·9832
E	300·	5·5810
Eb	287·1053	45·5768
D	268·3273	4·9878
*	250·7784	4·6704
C	240.	4·4735

Kong's determination. This difference will appear very small, when we consider the uncertainty of the precise epocha of the observation on which this determination is founded, and above all the uncertainty even of the observations. It would suffice to remove 54 years beyond the 1100dth before our æra, to reduce this difference to nothing, and then the observation would belong to the time of Ou-en-ouang, father of Tcheou-Kong, whom Father Gaubil mentions as having much loved and cultivated astronomy. The Chinese astronomers determined the moment of the solstice, by observing equal lengths of the gnomons' shadows forty or fifty days previous and after the solstice; and from that there may already be some error in Tcheou-Kong's determination. But the greatest error that is to be apprehended in the observation is in the manner of referring the solstice to the stars, in order to which the moment of the passage of such stars as crossed the meridian twelve hours after the moment of the solstice was observed: thus the right ascension of the opposite point to the summer solstice would be determined, and therefore also that of the winter solstice. But for so doing it was necessary to measure an interval of twelve hours. It appears that hour-vessels were used for measuring the time that a vessel was in filling to different heights with the water falling from a higher vessel (Treatise of Chinese Astronomy of Father Gaubil, published by Father Sauchet, Part I. p. 37.) It is easy to perceive how uncertain this manner of measuring time was, and three minutes of time, in an interval of twelve hours, are sufficient to account for the error of Tcheou-Kong's determination. The Chinese astronomers made likewise use of the moon's situation relatively to the stars in the lunar eclipses, to obtain the place of the sun, and therefore that of the winter solstice, at which they fixed the commencement of their year.

We must come down a thousand years, from Tcheou-Kong's epocha, before we find a second observation of the gnomons' shadows made in the solstices in China. Towards the year 104 before our æra, the astronomers Lieou-hiang and Lo-hia-hong observed the length of the shadow of an eight-feet gnomon at the winter and summer solstices. They found it 13 feet one inch four fen, or 13ft,14 at the former, and one foot five inches eight fen, or 1ft,58 at the latter (vol. ii. of Chinese History, published by Father Sauchet, p. 8). This observation is supposed to have been made in the town of Siganfou, then the capital of the empire: but this is an error which Father Gaubil has rectified in the quoted manuscript; in which is read as follows: (*Conn. des Tems*, 1809.)

"Lieou-hiang, father of Lieou-hia, wrote upwards of 50 years before Christ. This author says that an eight-feet gnomon gave the noon shadow in the winter solstice 13 feet one inch four fen, in the summer's it was one foot five inches four fen. Litchun-foung, an astronomer of the dynasty of the Tangs, complains that these shades were improperly applied to Siganfou. Lieou-hiang mentions neither the place nor the time of these observations."

The shade at the summer solstice is not exactly the same as that published in the quoted History of Chinese Astronomy; but I think that this last ought to be preferred, the shadow given in the MS. giving an evidently too considerable obliquity of ecliptic. It is very likely that in the manuscript Father Gaubil may have written, in a mistake, instead of eight fen the same number that he wrote for the winter solstice. Adopting therefore, 1ft,14 and 13ft,58 for the lengths of the shadows at the summer and winter solstices, and allowing for the refraction and the sun's parallax, I find 31° 2′ 23″ and 78° 33′ 41″ for the altitudes of the sun's centre, resulting from these observations. Half of their difference gives 23° 45′ 39″ for the ecliptic's obliquity. If we add it to the complement of 78° 33′ 41″, we shall have for the altitude of the pole 35° 11′ 58″, an altitude very different from that of Siganfou, which the Jesuits have found 34° 16′ 45″. Litchun-foung was therefore right to complain that these noon shadows had been improperly referred to Siganfou.

To compare my formula with this observation, I suppose that $t = -1850$, and then it gives for the obliquity of the ecliptic 23° 43′ 59″,4, and by adding 5″, as we have done for the preceding observation, we shall have 23° 44′ 4″,4, which only differs 1′ 34″,6 from the result of this second observation. These two observations are the only ones before the commencement of our æra, that Father Gaubil has made us acquainted with; and it is to be supposed that this learned missionary could not discover others: the destruction of books by fire, which took place 213 years previous to the Christian æra, having caused the loss of the greatest number of preceding observations.

[To be continued.]

LXXVIII. *Re-*

LXXVIII. *Reply to Mr.* M.*'s Remarks on Mr. Smyth's Comparative Table in* vol. xxxv. p. 488. *By Mr.* SMYTH.

To Mr. Tilloch.

SIR, I SHALL esteem it a favour if you will insert the following answer to the gentleman who signs himself M. in your Magazine for September.

Mr. M. says it is "curious that Mr. S. should presume organ-tuners will continue to tune as their ancestors did before them, till irrefragable arguments are produced to prove the superiority of Kirnberger's temperament." Here, I confess, I stand convicted of inconclusive reasoning. The fact, however, I imagine to be this: an organ, with compound stops, will not admit of the major thirds being tuned sufficiently sharp to ameliorate, in any considerable degree, those greatly tempered chords which are called *wolves;* of which I wish the breed were extinct.

I am glad to find that Mr. M. agrees with me in opinion, that Kirnberger's is one of the *worst* unequal temperaments. Had Mr. M. stated in definite terms his own favourite system, it should have been submitted to examination.

Mr. M. says, "*perhaps* for the organ a good *unequal* temperament is preferable to the *Isotonic.*" I was not ignorant that even for this instrument the Isotonic has had its advocates; and Mr. M. presents to my view the names of Couperin, Marpurg, Rameau, Cavallo, professor Chladni, and many other eminent philosophers. Now, not being a philosopher myself, I take the liberty of asking one plain question, which relates solely to the temperament of the organ:—Can any man living *prove,* that there ever was one organ in Christendom tuned according to the *equal temperament,* in consequence of a peremptory order from any one of these gentlemen, and suffered to *remain* in that state? This is coming to the point.

A person disposed to cavil might raise arithmetical and philosophical doubts whether a *real* equal temperament has ever been heard.

I wish Mr. M. would inform us, and explain precisely, *what the system is* which he tunes so dexterously on his harp, by the *melody alone,* without striking consonances. Had his instrument so tuned been intended for melody alone, this mode of tuning might answer the purpose; but, as each of the strings has various relations to other strings, and a temperament of a diatonic interval, too small to

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Bb	48·2557	40·0412	10·6067	3·2066	2·4007	47·0736	14 7447
A	402·7338	16·4028	9·9734	3·0162	2·2478	15·8794	13·7622
*	378·7240	15·4250	26·3800	2·8758	8·0076	14·9330	29·9746
G	359·3279	14·7447	8·8992	2·6912	2·0140	14·1686	12·2794
*	337·9052	13·7622	23·5368	2·5306	1·8994	13·3226	26·7434
F	320 5991	29·9746	7·9397	2·4006	1·7973	35·2898	10·9559
E	301·4859	12·2794	7·4665	2·2478	1·6901	11·8872	10·3023
Eb	286·0449	26·7434	7·0843	8·0076	1·6033	31·4802	9·7755
D	268·9919	10·9559	6·6613	2·3140	1·5081	10·6067	9·1919
*	352·9553	10·3023	17·6199	1·8944	1·4379	9·9734	20·0206
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LXXIX. *Copy of the Instructions given to their Engineers by the Commissioners appointed to inquire into the Nature and Extent of the several Bogs in Ireland; with further Particulars respecting the Bog of Allen, and its Substrata; accompanied with a transverse Section of Lullymore Bog, reduced from Mr.* Griffith's *Sheet Section. By Mr.* William Farey.

The secretary of the commissioners appointed to inquire into the nature and extent of the several Bogs in Ireland, and the practicability of draining and cultivating them, is directed by the Board to communicate, for the information of the engineers who may be employed, and of the proprietors of bogs, the mode in which they have been advised to proceed.

"1st.—They propose to divide the Bogs of Ireland into districts, and to assign each district in charge to one or more engineers.

"2d.—Each of the engineers is to provide a sufficient number of assistants, for whose qualifications he is to be responsible.

"3d.—The commissioners think it necessary to direct the attention of their engineers to the particular heads of inquiry contained in these instructions; but it is by no means intended to confine their judgement within these limits; on the contrary, where local circumstances point out a preferable mode of proceeding, the commissioners expect that it shall be fully stated, in addition to the information on the different points which they now suggest.

"4th.—They conceive, that the first steps towards the drainage of an extensive bog, should be to ascertain the

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" 9th.—In all cases where the bogs are wholly or partially surrounded by high land, whose natural inclination is to the bog, the engineers are to consider where catch-water drains may be necessary; and as it will generally happen that these catch-water drains may admit of a greater fall than it will be practicable to give to the main drains, care must be taken, where the catch-water drains are to join the main drains after their issue from the bog, that it shall be at such a distance, or on such a level, as to preclude the danger of the water to the catch-water drains, in time of floods, penning back the water of the main drains, so as to overflow the bog. Where the levels will not admit of the waters of the catch-water drains being conducted into the main drains without being subject to this inconvenience, provision must be made for conducting them through separate channels into the river, or other place, where the waters of the bog are to be discharged.

" 10th.—Each of the engineers is to prepare a map of the district assigned to him, distinguishing,

" 1. The extent and boundaries of the bogs which it contains.

" 2. The nature of the soil and country immediately contiguous to each bog, particularly specifying the situations of lime-stone, lime-stone gravel, marle, or other manures.

" 3. The surface of the bog, whether firm black bog, or shaking quagmire.

" 4. The situation of any springs, rivers, or lakes, which appear to occasion the wetness of any of the bogs.

" 5. The course of any rivers, streams, roads, or canals, by which the bog is intersected.

" 6. The drains and other works proposed by the engineers.

" 7. Such lines of new roads as appear most proper for the carriage of manure, for carrying out the future produce of the reclaimed bogs, and for communication with the roads in the vicinity.

" 11. These maps are to be accompanied with sections, delineating the surface and bottom of the bog, and the nature and depth (as far as may be necessary) of the under strata on which it rests.

" 12. The maps and longitudinal sections are to be drawn on a scale of four inches to an Irish mile, and the perpendicular scale of the sections to be $\frac{1}{20}$ inch on the foot.

" 13. They are to be accompanied with index maps, on the scale of one inch to a mile.

" 14. In taking the levels necessary for determining the sections, the engineers are to take care that their assistants proceed, in all cases, so as to cross and correct each other. The engineers are to be responsible for the correctness of the whole.

"15. The main drains are generally to be laid down so as to allow the collateral drains communicating with them, to embrace the greatest extent of surface the nature of the bog will admit of: but where the inequalities of level in its surface, or the outlets of discharge for the waters, present a choice of plans for its drainage, so as to induce any doubt in the mind of the engineer which plan may be most eligible, he is to submit the different plans to the commissioners.

"16. The engineers are to accompany the maps with written reports, containing generally whatever occurs to them on the subject of the drainage of the districts assigned to them, and particularly specifying,

"17. The probable expense of such drains, roads, canals, locks, and other works, as they recommend.

"18. The names of the proprietors who claim any right or interest in the bogs, and to what extent, and in what proportions, as far as they can learn.

"19. Whether any, and what tracts of bog in their districts have already been reclaimed, and what have been the manures used, and the modes pursued, in their amelioration, and what is the nature and the state of the crops which they actually produce.

"20. The probable value of the land when reclaimed, and the mode of culture which may be the best adapted for it, particularly distinguishing those parts that may be best suited for planting.

"21. Where any of the bogs proposed to be drained are at present used for the supply of fuel, the engineer is to report how far the quantity and quality of the fuel is likely to be injured or improved by the works which he recommends.

"22. Where the wetness of the bog appears to be occasioned by a lake on a higher level, the engineer is to report on the practicability and means of draining the lake; and also on the difference of levels in summer and winter of all rivers and lakes connected with the bogs.

"23. Where the bottom of the bog is lower than the river into which it would be convenient to discharge the waters of the drains, the engineer is to report on the practicability of lowering the river sufficiently to receive them.

"24. As in many instances the levels may not admit of the bogs being drained in the usual manner, in such cases the engineer is to take into consideration the propriety of draining it by means of wind-mill pumps or other machinery; the expense of erection, and annual charge of which, he will include in his report.

"25. The engineers are further to consider what situations may best answer for the corn-mills which may become necessary, in consequence of the increased tillage of the reclaimed districts, and how far the water of the drains may be used in working them; and they are particularly to inquire as to the situations and circumstances of such mills already in existence, the supply of whose water may be affected by the projected drainages, and to consider and report, whether it be most expedient to provide reservoirs for their supply, or to purchase the interest of the proprietors.

"26. They are also particularly to report, where any of the proposed works appear likely to diminish the supply of water for the Grand or Royal Canals, or other navigations, or to interfere with their levels or embankments; and in what manner such injuries may be best obviated.

"27. In order to connect the respective surveys with each other, and to enable the commissioners to judge how far these drains may be applied to the purposes of internal navigation, they propose to direct, that the engineers to whom the districts nearest to Dublin may be allotted, shall ascertain with the utmost accuracy, the difference of level between the levels in their maps, and of the platform on the capital of the column erected in the memory of Lord Nelson; and to communicate the difference of level to the engineers who may have districts immediately beyond them, for the purpose of carrying forward the comparison. The commissioners intend afterwards to request the Ballast Office to mark at the Pigeon House Dock the level of high water in an ordinary spring-tide in the Bay of Dublin, so that by determining the difference of level between that and the platform on the column, the difference between the level of the sea and the various levels which are to be taken in pursuance of these instructions, may be correctly ascertained.

"28. To enable the commissioners to complete the connexion of the surveys by trigonometrical observations, if such should hereafter be deemed expedient, the engineers are to have permanent marks at the extremities of the several levels, and to lay down all remarkable objects which are likely to be permanent, such as raths, towers, castles, cairns, hill-tops, market-houses, &c.

"29. The various lines of levels are to be shown on the map by dotted lines.

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dimensions, the engineer must consider how the district may best be subdivided.

" 36. The engineers are in all cases, both in their maps and in their reports, to express the contents of the bogs, both in Irish and English acres, and also to insert in their maps, scales of both Irish and English miles.

" 37. The commissioners not judging it expedient for the present to lay down a meridian for the purpose referred to in the twenty-eighth article of their instructions, consider it sufficient that the engineers should construct their maps upon the magnetic meridian, the north of the magnetic meridian pointing to the top of the map, and the meridian line being parallel to the sides of it.

" 38. The estimates referred to in the seventeenth article of the Instructions, are to include all the expenses, which in the judgement of the engineer will be necessary to reduce the bog to such a state that it shall be ready to receive agricultural improvements. These estimates are however to distinguish the expenses of the different descriptions of works, and of the different classes of drains recommended.

By order of the Board.
B. McCarthy,
Sec^{y} to the Commissioners.

Dublin Society House,
May 16, 1810.

To Mr. Tilloch.

Sir,—Having as an exercise, by direction of my father, reduced Mr. Richard Griffith's large section across the Bog of Lullymore, so as to agree in scale of length and in position nearly, with his map of this bog, printed in the 1st Report to Parliament on the Bogs in Ireland, from which you gave some extracts in your last number, and distinguished therein all the proposed drains, and shown by arrows whether they run northward (up) or southward (down), I take the liberty of sending a copy, and perhaps you may deem the same worth a plate in a future number of your Magazine, in order to explain, as it does (see Plate X.), the uneven surface and variable thickness of the peat in these vast bogs, the uncertain thickness and existence of the alluvial yellowish blue clay, (No. 10. in your last number, p. 371,) on which the peat frequently rests, and the very uneven and undulating form of the great bed of alluvial clayey limestone gravel, of vast thickness, which forms the floor and borders of this and most others of the bogs of this part of Ireland, except in a few places where strata appear, according to the report which Mr. Griffith, jun. has made on the subject; wherein, p. 15 and 16, the tract of land called the Island of Allen is thus described :—

" The surface of the Isle of Allen rises very quickly from the bog on all sides, particularly to the north-west, where it is composed (at least to a considerable depth) of limestone gravel, forming very abrupt hills. In those places where the face of the hills has been opened for the purpose of raising stone and gravel, the mass is composed of rounded limestone, varying in size from two feet in diameter to less than one inch; the largest are not so much rounded as the small, frequently their sharp angles are merely rubbed off; they are usually penetrated by contemporaneous veins of Lydian stone, varying in colour from black to light grey; the colour of the limestone is usually light smoke grey, rarely blueish black; when it is, the fracture is large conchoidal; that of the grey is uneven, approaching to earthy.

" The Lydian stone, when unattached to the limestone, has usually a tendency to a rhomboidal form, sometimes cubical, the edges are more or less rounded, the longitudinal fracture is even, the cross fracture is conchoidal.

" From the strong resemblance that subsists between the rolled limestone and its accompanying substances, and the upper beds of the limetone strata, which extend from the county of Tipperary, through Kilkenny, (where the lower beds are used for marble,) Carlow, Queen's County, King's County, Kildare, Meath, Westmeath, Dublin, &c. &c. there can be no doubt that the least accumulation of limestone gravel, which nearly covers the whole province of Leinster (forming steep ridges of hills frequently above 300 feet high, and sometimes approaching the summits of lofty primitive mountains) did originally form the upper beds of the limetone strata, which when now found in situ are seldom firm, on account of their alternating with thin beds of slate clay, usually much decomposed, and their being traversed by numerous fissures and veins of calcareous spar and Lydian stone.

" It is much more difficult to trace the course of the currents which first removed the limestone from its native bed, and afterwards having rolled the detached masses backwards and forwards, deposited them on the sides of hills, whose base had withstood the action of the waters, or by cross currents and eddies formed independent hills and minor ridges, the deposition of which, together with a subsequent deposition of a bed of clay, varying from one to six feet in thickness, and which almost universally covers the surface of the gravel, by obstructing the course of the waters in a country having naturally but little fall, may, by creating a general stagnation in them, and thereby forming extensive shallow lakes, have caused the growth of the *Sphagnum palustre**, and other aquatic mosses and plants, of which the mass of our bogs is composed.

" This island, though separated from the southern range of hills by a low boggy valley, may on a general view be considered as a continuation of that range.

" Perhaps a short geological description of this ridge, (which, on account of its height and steepness, forms the most prominent and interesting feature in the county,) though apparently foreign to the general object of this report, may (by preventing ignorant people from searching for limestone and other manures where they do not exist) be considered as an useful and necessary appendage.

" Near Ballyteague Castle, in the northern edge of the Island of Allen, stratified limestone makes its appearance at the surface, dipping 20 degrees east of south, at an angle of 5 degrees from the horizon. The stone is principally used for building, as on account of its containing a large proportion (according to my analysis 15 per cent.) of silex, it requires much fuel to burn it into lime.

" The next rock visible crops out about two miles to the southward of Ballyteague, at the base of the Hill of Allen near the village called the Leap of Allen, the intermediate country being covered by hills of limestone gravel; it is a species of conglomerate, composed of rounded quartz pebbles, varying in size from minute sand to six inches in diameter, connected together by a red iron-shot, argillaceous cement; then beds of a deep brick red slate; clay much interspersed with mica is found interstratified with the conglomerate: the dip is 30 degrees east of south at an angle of 7 degrees from the horizon. Southward of this quarry, rises the Hill of Allen, a very steep conical hill about 300 feet high (reckoning from its base); it is composed of an irregular unstratified mass of fine-grained greenstone, the crystal of hornblende and feldspar being very minute; transparent calcareous spar is frequently observable in the mass, rarely large crystals of feldspar are found interspersed; the rock on approaching the summit of the hill becomes more crystalline, detached masses of beautiful porphyretic greenstone thickly studded with large crystals of

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* Bog Moss.

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* Mem. de l'Ac. de Berlin, 1

† Pref. to 1st vol. of Obs.
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‡ See La Lande's Astr. 224
La Place's Mec. Cel. vol. iv. p. 2

by some seconds. In his paper on the subject, which is divided into four parts, he proves, first, that the mean refractions are very nearly the same for the same apparent altitudes throughout the whole extent of the temperate zone; since those which were observed at Paris did not exceed those observed at the Cape of Good Hope but $\frac{1}{40}$ at most. In the second he determines the absolute quantity of the mean refraction for the apparent height of the pole at Paris, and gives the result of his observations with regard to the latitude of Paris and of the Cape of Good Hope. In the third he gives his table of mean refraction, and another of corrections depending upon the state of the barometer and thermometer; concluding with some reflections on its construction and use. In the fourth he compares his new table with the most celebrated of those that had before that time been in use among astronomers; and he then shows how it agrees with the observations of Bradley, Zanotti and Mayer.

But by La Caille's Memoir * it appears, that previous to this time M. Mayer had formed and communicated to him a table of astronomic refractions which he computed by means of an algebraic formula†, the coefficients of which he deduced from his own observations, and took into account the variations relative to those of the barometer and thermometer. He found the alteration of refraction for a depression of 15 lines in the barometer, the same as for a rise of 10 degrees in the thermometer, and the variation for each degree of the latter, according to his table, $\frac{1}{28}$ of the whole mean refraction, which he adapted for 28 inches of the barometer, and 0° of the thermometer‡. This proportion takes place down to 80° of zenith distance. Mayer considered also that the mean refraction is the same for all parts of the earth; and that the only variation which takes place, depends on the changes of the § weight and temperature of the atmosphere.

La Caille, in comparing Mayer's Table with observations,

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* Mem. 1755, p. 555. † Vid. Mayer's Tables, 1770.

‡ French measure and Reaumur's therm.

§ It was perceived that the refraction near the horizon at Paris is sensibly affected by vapours, and the smoke which arises from the city, situated north of the observatory. Exhalations and the moisture of the atmosphere have certainly a considerable influence on it, and so has the situation of the place, being more or less elevated. The neighbourhood of a city, mountains or hills, forests, rivers, or marshy plains likewise affect it much; and La Caille was persuaded that an astronomer never had refractions purely celestial near the horizon, that is, of the nature of those 20° above it; local circumstances producing such considerable differences in them that he did not choose to insert in his table those for altitudes below 6°. Cassini de Thury believed that the refraction and its inequality were greater at Paris under similar altitudes on the south side than on the north, and at 4° he found it 20″ more. *Encycl. Meth.*

found that his correction for the thermometer was a little over-rated; and accordingly, for his new table, altered it to $\frac{1}{47}$ for each degree. And here it may be observed that La Caille did not correct his altitudes above 36° at Paris, and 30° at the Cape; first, because he only noted the barometer and thermometer in the night, when he observed stars below 30° of altitude. Secondly, because, that at 36° of altitude, where the mean refraction is about $1\frac{1}{2}$ minute, the variation which belongs to 10 degrees of the thermometer only amounts to $3\frac{1}{2}$ seconds; a quantity about equal to the limits of the errors of observations made with an instrument of six feet.

The formula given by Euler* appeared also about this time. It took into account the variation of the refraction depending upon the thermometer and barometer, but was certainly too complicated to be generally adopted. He shows however, that in very different hypotheses the refraction will be sufficiently exact, if taken in the inverse ratio of the degrees of heat, when the star or planet is not too near the horizon, but the precise quantity of this ratio was unknown to him.

In this state the refraction stood when Dr. Bradley took the subject into consideration, and began to find its quantity from his own observations. The rule which he adopted, although a very elegant one, he neither lived to complete nor to present to the world; but it was published after his death by Dr. Maskelyne†, and has commonly been used in England up to the present time. He found the mean refraction at 45° of altitude 57″, and, that at all other altitudes it was equal to 57″ multiplied by the tangent of the zenith distance, diminished by three times the refraction. Then supposing the mean state of the atmosphere to be at 29·6 in. of the barometer, and 50° of Fahrenheit's thermometer, he made the true or corrected refraction equal to $57'' \times t,(\text{Z.D.}-3r) \times \frac{\text{barom.}}{29{\cdot}6} \times \frac{400}{350+\text{ther.}}$ where it is to be understood that the mass of air is supposed to increase in bulk $\frac{1}{400}$ for each degree of Fahrenheit's scale.

A variety of experiments have been made at various times to ascertain the increase in bulk of a quantity of air represented by unity for a certain number of degrees of rise of the thermometer. The following is a list of some of them‡:—

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* Mem. de l'Ac. de Berlin, 1754, p. 181.

† Pref. to 1st vol. of Obs. 1765. Phil. Trans. 1764 and 1787, p. 157. Req. Tables, &c.

‡ See La Lande's Astr. 2241. 3d ed. Thomson's Chemistry, vol. i. p. 489. La Place's Mec. Cel. vol. iv. p. 270. Phil. Trans. 1809, &c. &c.

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M. Bonne	1·00 2577$\dot{7}$
Bradley	1·00 25000
Dalton	1·00 20701
De Luc	1·00 2088$\dot{8}$
Fahrenheit	1·00 2577$\dot{7}$
Gay-Lussac	1·00 20868
Groombridge	1·00 21000
Hawksbee	1·00 06933
La Caille	1·00 2222$\dot{2}$
Mayer	1·00 2044$\dot{4}$
Shuckburg	1·00 2222$\dot{2}$
Mean of all except Hawksbee's	1·00 22490

The refraction deduced from Bradley's very neat and simple formula was in a few years adopted by nearly all the astronomers of eminence throughout Europe. The extreme facility with which it might be computed, and the corrections applied, whether from the formula itself or from tables ready calculated for that purpose, was a powerful recommendation in its favour; but its near agreement with observations soon established it.

In 1805, the very ingenious and profound M. de la Place in his *Mécanique Céleste** favoured the world with a chapter on this subject, wherein he has displayed as much sagacious penetration as deep mathematical learning and ability. He begins with considering the trajectory of a ray of light traversing the atmosphere; and by supposing all its layers spheric, and of variable density, according to some function of their height, he deduces a differential formula for the refraction whose integral he then finds; but, he observes, this equation supposes that the refractive forces of the layers of the atmosphere are directly proportional to their density, which is the result of Hawksbee's experiments. Nevertheless, it is possible that this assumption may not be strictly correct, and it would be useful if more experiments were made on the subject. He then finds that the hypothesis of an uniform temperature is erroneous, as well as that of the density decreasing in arithmetic progression, when the height increases in a similar progression; and he says, " the constitution of the atmosphere being comprised between the two limits of a density decreasing in arithmetic progression, and of one decreasing in geometric pre-

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* Vol. iv. page 231.

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" It results from this table," he says, " that the effect of moisture in the air on the refraction is very small; the excess of the refractive power of the aqueous vapour on that of the air being compensated in a great measure by its less density. We may nevertheless attend to it by means of the preceding table, in cases of extreme humidity. Observations of the hygrometer will point out the ratio of the quantity of vapour spread in a given volume of air to the quantity which would produce extreme humidity in this volume. The increase of refraction which corresponds with extreme humidity must then be multiplied by this ratio." He concludes the subject with the following remark:

" If we would take into account the figure of the earth in the theory of refraction, it is to be observed that at the point where the observer is situated, we may always conceive an osculatory circle to the surface of the earth, whose plane passes through the star: now the figure of the atmospheric layers is very nearly the same as that of the earth; the circles, concentric to the circle in question, are therefore osculators likewise of these different figures; and we may determine the refraction of the star by supposing the earth to be spheric, and of a radius equal to this osculatory circle. Thus wee see, 1st, That the refraction always takes place in the vertical plane: 2dly, That it is not the same on all sides of the horizon, since the osculatory circles are not the same in every direction; but we may rest assured, that the error is insensible, when the star is a little elevated. At the horizon, however, differences of some seconds may occur." Thus terminates one of the most masterly chapters on this subject ever written; after which he proceeds to treat of the terrestrial refraction.

Upon these theorems* found by La Place, reduced to rather a more convenient form, and with coefficients differing a little from his, Delambre has computed a set of Tables by means of which the refraction may be found with great facility. They were first published by Puissant † in a work closely connected with this subject, and are well arranged for use. The first of them gives the refraction for every degree of apparent zenith distance down to 80°, and for every 30′ from thence to 90°. They are adapted for 0·71 metre of height of the barometer, and 35° of the centigrade thermometer ‡. Besides the refraction and its difference for

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* Page 271, and page 264 of the Mecan. Cel. vol. iv.

† Traité de Geodesie, at the end, 4to, 1805.

‡ Or, 27·953 English inches, and 95° of Fahrenheit's thermometer.

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with the navy; whilst it is well known, that at this moment a few of our best mathematicians are groaning under insults, degradations, and injuries, as severe as they are unprovoked and undeserved. Had France the tenth part of our naval power, with her present number of scientific men, the whole world must soon be subjected to her dominion: and where so much is at stake, it behoves us, by giving all the encouragement in our power, to place our navy as much above that of other nations in scientific knowledge, as it is in all other high and great qualifications.

The gradual decrease of the study of mathematics in this country has already been publicly noticed*; but its causes, although very evident, have not yet been mentioned. Perhaps, at some future period, this may form the subject of another communication. I have been led into this digression by considering the still more deplorable state if possible of astronomy, which at this moment is scarcely cultivated by half a dozen persons throughout the whole kingdom. But to return.

It has been doubted, notwithstanding what is stated by La Caille, whether the mean refraction of France be the same as that of England,. What gave rise to this was the use of Bradley's Table in the determination lately made there of the obliquity of the ecliptic, wherein a difference of some seconds was found, between the result obtained from observations made in the winter solstice, and that from others made in the summer. This doubt has not yet been satisfactorily removed: but the very accurate astronomic circles lately made by our English artists, who are undoubtedly the best in the world, will, it is presumed, with good assistance from theory, not only soon decide this question, but furnish us with such observations as will determine the refraction to a second, till we approach near the horizon.

A very material step has lately been made towards this, by the publication of Mr. Groombridge's valuable paper on Refraction†, wherein he has determined, by a great number of very accurate observations, both the quantity of mean refraction at 45°, and the coefficients for correcting it on account of the state of the atmosphere. The former of these he makes 58″·1192 by a mean of a great number of observations, when the barometer is at 29·6 inches, and the thermometer 45° of Fahrenheit without, or 49° within, which he considers as the mean state of the atmosphere*. With respect to the latter, he finds the multiplier for all degrees below 49° within, to be ·0024; and above 49° within, ·0023 †: but for those above or below 45° without, he finds it ·0021. Instead, however, of the number 3, which Dr. Bradley had adopted for his coefficient of r, Mr. Groombridge finds that 3·3625 agrees better with observations: consequently his numbers and coefficients will give us the following four equations.

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* Edinburgh Review of La Place's *Mecanique Céleste.*

† Philosophical Transactions for 1810.

1st. For the thermometer *within*, and *below* 49°, putting

$$d = 49 - h.$$

$$\text{Refr}''. = 58''\cdot1192 \times t,(Z - 3\cdot3625r) \times \frac{b}{29\cdot6} \times (1 + \cdot0024d).$$

2dly. For the thermometer *within* and *above* 49°, put

$$d = h - 49.$$

$$\text{Refr}''. = 58''\cdot1192 \times t,(Z - 3\cdot3625r) \times \frac{b}{29\cdot6} \times (1 - \cdot0023d).$$

3dly. For the thermometer *without* and *below* 45°, put

$$d = 45 - h.$$

$$\text{Refr}''. = 58''\cdot1192 \times t,(Z - 3\cdot3625r) \times \frac{b}{29\cdot6} \times (1 + \cdot0021d).$$

4thly. For the thermometer *without* and *above* 45°, put

$$d = h - 45.$$

$$\text{Refr}''. = 58''\cdot1192 \times t,(Z - 3\cdot3625r) \times \frac{b}{29\cdot6} \times (1 - \cdot0021d).$$

Or, in logarithms:

1st. Thermometer *within* and *below* 49°, put $d = 49° - h°$, then:

$$\text{L. tan.}\left(Z - \frac{26\cdot9}{8}r\right) + \text{L.}b + \text{L.}(10000 + 24d) + 6\cdot29303 = \text{L. Ref.}''$$

---

[*] Mr. Kirwan states that the mean temperature of any place is equal to 84—51s^2, latitude. According to this the mean temperature of Blackheath would be 52°·8, but its exposed situation may possibly be the cause of this small difference.—*Estimate of the Temperature of different Climates*, or *Dalton's Meteorological Observations*, page 120.

[†] Mr. Dalton found likewise that the expansion of air from 55° to 133½, or for the first 78½°, was 167 parts, whilst the expansion from 133½° to 212°, or for the next 78½°, was only 158 parts, or nine less than the former. So that it appears there is a difference between the expansion of air for the high degrees and that for the low ones.—*Manchester Memoirs*, vol. v.; or *Thomson's Chemistry*, vol. i. p. 490, edit. of 1810.

2d. Thermometer *within* and *above* 49°, put $d = h^\circ - 49^\circ$, then:

$$\text{L.}\ t,\left(Z - \frac{26 \cdot 9}{8} r\right) + \text{L.}\ b + \text{L.}\ (10000 - 23d) + 6 \cdot 29303 = \text{L. Ref.}''$$

3d. Thermometer *without* and *below* 45°, put $d = 45^\circ - h^\circ$, then:

$$\text{L.}\ t,\left(Z - \frac{26 \cdot 9}{8} r\right) + \text{L.}\ b + \text{L.}(10000 + 21d) + 6 \cdot 29303 = \text{L Ref.}''$$

4thly. Thermometer *without* and *above* 45°, put $d = h^\circ - 45^\circ$, then:

$$\text{L.}\ t,\left(Z - \frac{26 \cdot 9}{8} r\right) + \text{L.}\ b + \text{L.}(10000 - 21d) + 6 \cdot 29303 = \text{L. Ref.}''$$

But as it appears more simple to avoid the two numbers 49 and 45; and reckon the state of the thermometer from zero, we may reduce the whole of these equations to that temperature, and then find other multipliers for the number of degrees above that point; which is easily done as follows: Put R = the refraction at any given temperature; τ = the degrees of that temperature, ϱ = the refraction at zero, n = the multiplier for the state R, and ν = that for zero: then we have

$$R + R\tau n = R\ (1 + \tau n) = \varrho;\ \text{whence}\ R = \frac{\varrho}{1 + \tau n};\ \text{but}$$

on the contrary, $\varrho - \varrho\tau\nu = R = \varrho\ (1 - \tau\nu)$; consequently, $\varrho(1 - \tau\nu) = \frac{\varrho}{1 + \tau n}$, from which we get $\nu = \frac{n}{\tau n + 1}$: then, by substituting Mr. Groombridge's multipliers for n, we get the new multipliers for reducing the refraction from zero to any other temperature. By this we also obtain the advantage of having only three equations instead of four; which, putting h = height of thermometer above zero, will now be as follows:

1st. From zero to 49°, *within.*

$$58'' \cdot 1192 \times 1 \cdot 1176\ \text{tan.}\left(Z - \frac{269}{80} r\right) \times \frac{b}{29 \cdot 6} \times (1 - h \cdot 002147) = \text{R.}''$$

2dly. For all degrees above 49 *within.*

$$58'' \cdot 1192 \times 1 \cdot 127 \times t,\left(Z - \frac{269}{80} r\right) \times \frac{b}{29 \cdot 6} \times (1 - h \cdot 002067) = \text{R.}''$$

3d. For the thermomet

$$58''\cdot1192 \times 1\cdot0$$

$$(1 - h\ \cdot00191$$

Or, in logarithms:

1st. From zero to 49° *u*

$$\text{L. } t, \Big(Z - \frac{269}{80}$$

$$\cdot002147) = \text{L}$$

2dly. Above 49° *withi*

$$\text{L. } t, \Big(Z - \frac{269}{80}$$

$$\cdot002067) = \text{L}$$

3dly. For the thermome

$$\text{L. } t, \Big(Z - \frac{269}{80}$$

$$\cdot001919) = \text{L.}$$

In these equations, some little arithmetic tr last part of each expres easiest done by a table of the nine digits: by m number into any other adding that of its sever be done by multiplying

pression $3\cdot3625\, r = \frac{3 \times}{8}$ quire contracted multipli

The last may easily be simple method pointed well known that $\cos.^2 =$ $x = 1 - hn$, and com two expressions, we hav

$$c^2, A = r^2 -$$

consequently $s.\ A = \surd$ corresponding will be ec rule. To the logarithm divide the sum by 2. Logarithmic Sines, and t responding to it, which is readily done at the same opening of the book. Double this logarithmic cosine, and it is the logarithm required of $1 - hn$*.

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* Trigonometry, 2d edit. 4to.

But in a letter dated 17th September 1810, which I had the honour to receive from Mr. Groombridge, he informs me that he has calculated on the data before mentioned a Table of Refraction for every 10′ down to 70° of zenith distance; for every 5′ from thence down to 86°; for each 4′ thence to 88°; each 3′ thence to 89°; and for every 2′ from thence to 90° 18′: together with an auxiliary Table for the correction depending on the difference of the barometer and thermometer from the mean state. He has also contrived some very simple methods of performing with great facility whatever arithmetic operations may be requisite in using them. Every sincere lover of the science will no doubt join with me in requesting that these tables and methods may form the subject of another communication to the world, whereby it is presumed the mode of finding the refraction will be made extremely easy, and an important service rendered to astronomy.

As an appendix to what has been said on the refraction, I shall take the liberty of adding the following method of finding the sun's parallax, which is rather more accurate than as it is usually given in the Tables. *Add together the logarithmic sine of the sun's zenith distance, the logarithmic distance for the given day taken from page* iii *Nautical Almanack, and the constant number* 0·94151 : *their sum, rejecting the tens in the index, will be the logarithm of the sun's parallax in seconds.*

It must however be observed, that I have taken the sun's mean horizontal parallax at 8″·74: for it was found† by observations made at the Cape of Good Hope, as well as others made by M. Pingré and Mr. Short, 8″·8; by M. du Sejour, 8″·84; by M. Lexell and M. de la Lande, 8″·6. The mean of these six determinations is 8″·74, which is the quantity we have adopted above.

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* When the thermometer is below zero, this expression will of course become $1 + hn$, in which case it may, as above, be compared with $1 + t^2 = ſ^2$. but instances are extremely rare where any necessity for this occurs in England.

† La Lande's Astronomy, art. 1725, 3d edit.

LXXXI. *De-*

ascertaining the density of the strata of the atmosphere, if any doubts remained as to the nature of the air, the proportions of its constituent parts, and the law which its dilatation follows by the elevation of temperature: but at present, as we have precise information on these subjects, and as the uncertainties which may remain on the indications of the hygrometer are much smaller than those which ought to result from the methods mentioned; it is more expeditious and more certain to adhere to the barometer, combining its indication with that of the thermometer and hygrometer.

The case is not the same with the manometer destined to determine the changes which take place in the elasticity of a quantity of air contained in a vase. Saussure directed towards this object the apparatus to which he gave the name of manometer, and by means of which he made some most important observations*: it is simply a barometer, the bulb of which is contained in a bell-glass which is hermetically closed, and into which we may introduce the substances which may affect the elasticity of the air, by an aperture in the neck of the bell-glass, but by establishing, at that instant, the communication between the internal and external air.

While the communication with the external air is interrupted, the barometer is insensible to the variations of the atmosphere, and it undergoes no change in its elevation except by the increase or diminution of the elasticity.

This is the manometer, the applications of which I wished to extend, and which I endeavoured to adapt to the observation of the phænomena which take place during vegetation, and generally those which animal and vegetable substances present, during life or after death, relative to the atmosphere with which they are surrounded.

In the first place we perceive that the barometer which performs the functions of the manometer, indicates the quantities of gas which are disengaged or absorbed in a given time; and as it is easy to ascertain a change even of one thousandth part in the height of the barometer, we may determine a change of one thousandth part in the quantity of the contained air, by the absorption or extrication of a gas.

But in order to make this estimate, there must be a thermometer suspended internally to indicate the same temperature with that of the first observation: or if the temperature

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* *Essais sur l'Hygrometrie*, p. 109.

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M. Theodore de Saussure
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* *Journal de Physique*, to

by the liquid contained in the apparatus, may be determined by precipitating this acid by lime water, or by water of barytes, either from the whole or from a part of the liquid: after that, we introduce the precipitate into a flask, adapt the tube of a funnel to it, through which we pour a quantity of dilute sulphuric acid; and by the loss of weight which takes place, we ascertain the quantity of carbonic acid which was dissolved in the liquid, and which is disengaged from the carbonate.—We may, by the processes which I have indicated, ascertain in a volume of air eqnal to that of a kilogramme of water, and contained in a manometer which has this dimension, the change which would be produced by the volume of one gramme of water; the production of a quantity of carbonic acid which does not exceed a centigramme in weight; and a variation in the proportions of the oxygen and azote which does not exceed a centieme: this is a precision which would seem to be sufficient for all the determinations which we would wish to establish.

We have besides the advantage of being able to repeat and compare the tests at different times, without interrupting the experiment, and to vary several of its circumstances: I have constructed manometers of different dimensions, in order to apply them to different objects.

Hitherto I have made but a small number of observations with this instrument, and I have not pursued them with the care which they require; but my chief object in this publication is to induce those to employ it who are occupied with experiments of this nature, and who have more leisure and perseverance than I have. I shall describe some early attempts.

M. Theodore de Saussure, to whom we are indebted for some learned and laborious researches upon vegetation, has shown, that in most of the cases where we suppose that the oxygen gas was absorbed by a vegetable or animal substance, there is simply formed a combination of the carbon of these substances with the oxygen of the atmosphere; that the volume of the gas did not diminish, except on account of the absorption of the carbonic acid by water; and that at the same time water was produced by the combination of the oxygen and hydrogen which existed in the substance; so that, although the residue had been deprived of a part of its carbon by the action of the oxygen gas, it is nevertheless found more carbonized afterwards, because it has been stripped of a greater proportion of hydrogen and oxygen than of carbon*.

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* *Recherches Chimiques sur la Végétation.*

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Then this solution give
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an olive-coloured precipitate with the solution of highly oxidated iron, instead of a blackish blue precipitate: a fawn-red precipitate with the muriate of copper, instead of a blue precipitate.

Hence we see that, in the application to the arts, we may obtain from the campeachy a different colour, according to the kind of vessel in which we make the decoction: that the action of the air, at least when it is continued too long, changes its nature and decomposes it: so that the decoction kept by the name of campeachy juice may be spoiled, if we allow it to undergo the action of the air without some precaution.

Results were obtained different from the two foregoing, when we submitted gall-nuts to the test of the manometer, with the view of examining what passed in the development of the gallic acid: a portion of the oxygen of the air is transformed into carbonic acid by means of the carbon of the substance; but another portion also is liberated, the two elements of which it had furnished; and lastly, there is a considerable absorption of azote; a circumstance which requires ulterior observations.

Explanation of the Plate which represents the Manometer, and of the Method of using it.

Fig. 1 and 2. Vertical and horizontal projections of a cylindrical manometer formed by a vessel A with a large aperture, the neck of which has a copper rim B. The inside of this rim forms a screw for receiving the plate of copper E, which serves to close the manometer: it rests on a round pad of leather so arranged that when the lid is screwed down upon it, the vessel is very closely shut. G, G, buttons on which are fixed the notches of the key represented flat in R, and seen directly in S; this key serves to keep the vessel steady, while we turn and fasten the lid with the other key T; the square head of which embraces the button of the same form, which we see at E in the two projections.

a, *a*, *a*, three hooks fixed in the lid from which we may suspend a thermometer, a hygrometer, &c. D, a socket in which we fix with hard mastic a barometer with a syphon: as it would be difficult to give it, in this socket, a situation exactly vertical, and besides, as the inclination of the screw in the lid may remove it from this position, in order to give more precision to its indications, we rest the manometer on a rim of wood, having three screws in it *k*, *k*, *k*, which we move until the tube of the barometer be very vertical; which we may easily judge by means of the plummet I F, which is to be adjusted successively in two positions which form a right angle with each other. This plummet is attached to a moveable brass scale H, to which we give only $0^m.04$ to $0^m.05$ of extent. This scale embraces, by means of two rings *b b*, not shut, the barometrical tube: it may thus be placed at any height on the barometer, and preserve the position which is given to it. It is used to determine the quantity which the height of the column of mercury has varied in the course of an experiment: if this quantity exceeds the limits of this scale, which is not very probable, it may be shifted so as to measure at several times the whole variation observed. The absolute height of the mercury is taken at the commencement of the experiment on a barometer, and we fix one of the extremities of the scale H at the summit of the mercury at this moment. The small branch of the syphon is furnished with a scale, in order also to observe the difference of the height of the mercury from the commencement to the end of the experiment. When the experiments require it, we give to the tube a length which exceeds much that of the common barometers, and it may be augmented sufficiently to indicate a pressure double that of the atmosphere.

The plate E has at C a stop-cock intended to give issue to the air of the apparatus when we wish to examine it; and this stop-cock is adjusted in such a manner that we may repeat these experiments as often as we judge it necessary in the course of an experiment, without fearing to change the nature, or even the state of compression, of the air of the manometer. For this effect, the stop-cock has above its collet at L (figures 1, 3, 4, and 5) two nut-screws, one internal and one external. On the latter is mounted a copper salver M, which we fill with distilled water: the glass tube N, graduated and furnished with a copper socket at O, is adjusted upon the internal screw, after having been also filled with distilled water: the extremity of its screw is furnished with a round of leather, which we compress. On opening the stop-cock the water of the tube is displaced by the air, which escapes from the manometer, and when we perceive that a sufficient quantity has entered into the tube, we shut the stop-cock. Upon unscrewing the tube, the volume of the air which has entered generally changes, and occupies a smaller or larger space, in proportion as it underwent in the manometer a pressure weaker or stronger than that of the atmosphere. But we remove the tube by plunging the finger into the water of the bason, and closing with its extremity the orifice of the tube, and we do not measure the air until after we have determined with the usual precautions the temperature and pressure to which it is exposed.

We must only introduce in this manner into the manometer, a liquid, which most commonly does not disturb the results, and the influence of which we can always ascertain: if we were afraid, however, that it would interrupt the experiment, we might receive it into a vessel disposed for this purpose in the inside of the manometer.

Fig. 3 shows the various pieces just described, ready to be adjusted: fig. 5 is a section of these same pieces all adjusted.

We ought to take care, in the construction of this apparatus, to give the hole of the key of the stop-cock a diameter sufficiently large to admit of the easy flowing of the water of the tube, and it ought not to be less than twelve millimetres. In order that the air contained in this hole may be in the same circumstances with that which occupies the whole capacity of the manometer, we leave the stop-cock open during the experiments, as seen in fig. 1 and 2; we intercept the communication with the external air by means of a copper stopper Q (fig. 1 and 4) which has the same screw with the mounting of the divided tube, and which is also furnished with a rim of leather. In order to close it properly, it has at its surface a square cavity which is seen at *p*, into which we insert the stalk *r* of the same form which is at the extremity of the handle of the key T. We then only close the stop-cock at the moment when we wish to extract the air from the manometer.

LXXXII. *On the Barometer. By* RICHARD WALKER, *Esq.*

To Mr. Tilloch.

SIR, CONSIDERING that I may not have been sufficiently explicit in my last paper, respecting the effects of the *difference of temperature* on the weather, I have been induced to offer the following remarks on that part of the subject.

A *warm temperature* of the air, at any degree of density of the atmosphere, will retain a greater portion of water in a state of *chemical combination*, than a *cold temperature* of the air at a similar degree of density of the atmosphere. Hence we may account for the almost constant *dry state* of the lower stratum of the atmosphere during the summer

MER SEASON, and the almost constant *moist state* of the lower stratum of the atmosphere during the WINTER SEASON; the air, however, being sometimes sufficiently dense, as in the clear weather which accompanies a freezing atmosphere, to retain the water in a state of *chemical combination*, notwithstanding the diminution of temperature.

The same circumstance accounts likewise for the different states, with respect to *moisture and dryness*, of the middle seasons, viz. SPRING AND AUTUMN, accordingly as these participate in their nature more or less of either of the former seasons; observing that, *cæteris paribus*, there is more rain and misty weather during AUTUMN than SPRING, in consequence of the greater quantity of water which has been raised into the atmosphere during the SUMMER than the WINTER SEASON.

All the circumstances I have had occasion to mention, depending upon the *greater or less density*, and the *higher and lower degrees of temperature* of the atmosphere, are exemplified by the two following familiar experiments:

In the first instance, by means of pumping out of a glass receiver (containing air apparently dry and perfectly transparent) a certain portion of the air it contains, when the air being rarefied deposits a certain portion of the water it originally contained in *chemical combination* in a cloudy vapour, which, upon re-admission of the air, is re-absorbed; and in the second instance, by abstracting heat from a glass vessel containing atmospherical air, and again restoring the heat. The latter circumstance is likewise instanced, *naturally*, by what commonly happens in the course of a hot summer's day, particularly when the ground has become very moist by previous rain; the vapour *ascending* visibly in the morning, disappearing during the middle of the day, and *descending* visibly again in the evening*.

The variations of temperature in the atmosphere *independent of those which proceed from the direct influence of the sun*, arise from the conversion of water into vapour, which produces *cold*; and the condensation of vapour into water, which produces *heat*. Hence it commonly follows, that in proportion as the barometer *rises*, the thermometer *sinks*, and *vice versâ*, throughout the year; the direct influence of the sun in *clear weather* being abstracted†.

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* At Lima, in Peru, it never rains; the moisture raised in the day time being restored again at night in the state of mist.

† In summer, during fair weather, the nights, or rather the mornings before sun-rise, are cold, approaching even to frost.

Thunder frequently follows a considerable duration of dry hot weather, both these circumstances being favourable to the collection and insulation of electric matter.

The extraordinary *elevation* of the barometer which sometimes happens, is said to arise from two currents of air, from opposite directions, meeting and accumulating over a particular spot; and the extraordinary *depression* of the barometer, from the circumstance of two currents of air setting out from any particular spot: in either case a commotion of the air is necessarily produced, whilst the equilibrium is restoring.

That the atmosphere, as well as the sea, is affected periodically in a small degree by the attraction of the moon, is well ascertained; but it does not appear that the weather is in the least influenced by any mechanical effect of the moon.

I was first led to the remark noticed in a former paper, respecting the difference of the weather during the *increase* and during the *wane* of the moon, by observing that eclipses of the moon were much seldomer obscured by a clouded atmosphere than eclipses of the sun; and subsequent observations of a general nature have somewhat confirmed me in the same opinion.

P. S. I omitted to mention, in my paper on the measurements of heights by the barometer, (Phil. Mag. for Oct. 1810, p. 278) that when the lower station in the barometer is *below* what is provided for in Table 2, p. 279, as is sometimes the case in *different gradations* of heights, the most accurate method will be, first to calculate the whole height, *assuming* 30 *inches of the barometer for the lower station;* and in like manner calculate the *lower portion only*, and then subtract the latter product from the former.

Oxford, Dec. 15, 1810. RD. WALKER.

LXXXIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

On Dec. 6th, the reading of Mr. Davy's Bakerian Lecture was continued, and on the 13th concluded. In this part of the lecture Mr. Davy detailed a number of experiments, which he regarded as showing that when any metallic oxide is converted into the substance improperly called a muriate, but which is a binary combination of oxymuriatic gas and a metal, the oxygen produced is exactly that which fered to die. Professor Davy suggested to the author, that the cold air thrown into the lungs (which produced the usual change in the colour of the blood) might contribute to this effect; and accordingly an experiment was made to obviate such consequence by means of a ligature: when it appeared, that in an hour and forty minutes the body in which the circulation was artificially continued after dividing the spinal marrow, was only one degree colder than that which died immediately. Mr. B.'s experiments seem to militate against the doctrine of the vitality of the blood; but they do little towards illustrating the fact, that tortoises can live and walk about long after having been deprived entirely of the brain, and even part of the spinal marrow.

On the evening of the 20th, part of a letter from Dr. Parry, of Bath, was read, on certain nervous affections; as convulsions, tremulous motions, and sudden startings or pulsations of what is vulgarly called the life blood; after which the society adjourned till January 10.

ROYAL SOCIETY OF EDINBURGH.

On Monday the 5th of November, the Royal Society of Edinburgh met for the first time in their new apartments in George-street, when Dr. Thomas Thomson read two papers, giving the account of the analyses of two new minerals from Greenland. To one of them he has given the name of allanite, and to the other sodalite. In the first he discovered a considerable portion of cerium, and in one analysis he detected a quantity of a metallic oxide perfectly new in its properties, for which he proposed the name of junonium. The other mineral, according to his investigation, affords 23 per cent. of soda and three of muriatic acid. By an analysis of Mr. Ekeberg, the same constituents were yielded in the proportions of 25 per cent. and six per cent.

At the next meeting on the 19th, a short communication was read, respecting a singular water-spout observed at Ramsgate.

On the 3d inst. a paper by Dr. Brewster was read, being a new demonstration of the fundamental properties of the lever.—Also a communication by Sir George Mackenzie, Bart. relative to the hot springs of Iceland; when Sir George exhibited some beautiful drawings and part of a series of magnificent specimens from that country, which he proposes to deposit in the cabinet of the Society: and at the last meeting, on the 17th, Sir George began a description of the minerals of Iceland, when he exhibited specimens from the district called the Guldbringe Syssel.

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METEOROLOGICAL TABLE,

By Mr. Carey, of the Strand,

For December 1810.

Days of Month.	Thermometer.			Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
	8 o'Clock, Morning.	Noon.	11 o'Clock, Night.			
Nov. 27	41	47°	42°	29·20	12	Fair
28	40	46	39	·01	21	Fair
29	36	43	37	28·98	0	Rain
30	35	42	34	29·25	17	Fair
Dec. 1	34	38	36	·50	12	Fair
2	32	36	31	·90	10	Fair
3	30	37	40	·85	0	Rain
4	42	44	44	·90	8	Fair
5	45	49	47	·89	10	Fair
6	47	50	44	·46	0	Rain
7	44	47	36	·30	6	Fair
8	37	42	30	·61	0	Rain
9	29	35	31	·87	10	Fair
10	35	40	36	·23	0	Rain
11	36	38	30	·80	5	Fair
12	35	46	42	·30	0	Rain
13	46	52	47	·85	10	Fair
14	48	48	43	·50	5	Stormy
15	40	46	36	·86	15	Fair
16	37	43	35	30·30	10	Cloudy
17	41	49	47	·08	7	Cloudy
18	48	46	45	29·40	0	Rain
19	41	40	36	·51	9	Fair
20	37	44	43	·62	0	Rain
21	43	46	42	·38	10	Fair
22	41	46	52	·60	9	Cloudy
23	52	47	43	·42	8	Fair
24	42	43	41	·45	4	Rain
25	49	49	46	·05	4	Stormy, and in the evening many vivid flashes of lightning.
26	44	47			23	Fair

N.B. The Barometer's height is taken at one o'clock.

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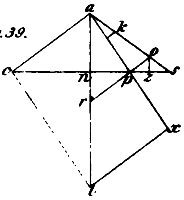

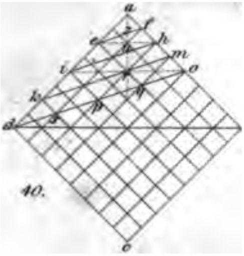

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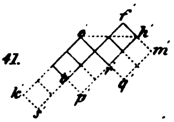

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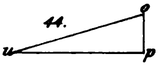

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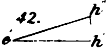

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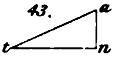

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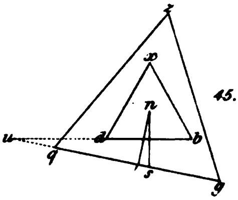

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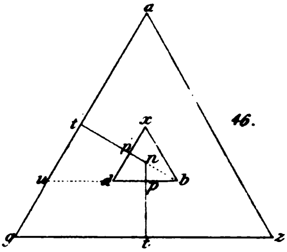

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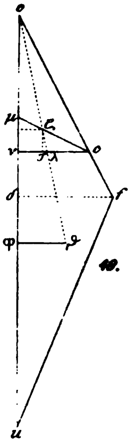

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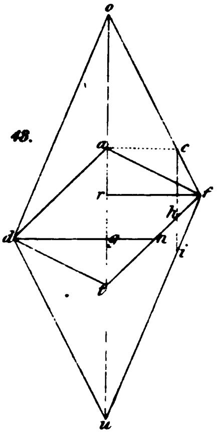

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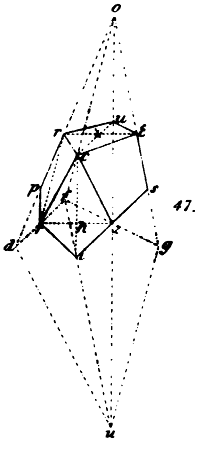

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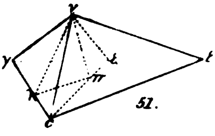

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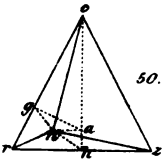

Lowry sculp.

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HAÜY PLATE XII.

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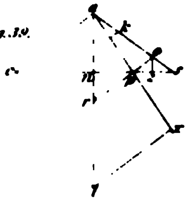

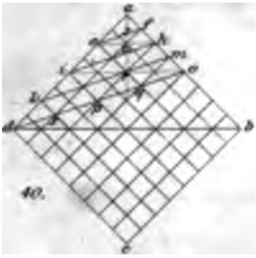

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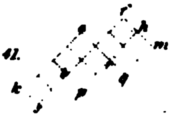

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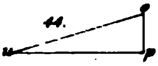

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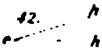

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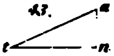

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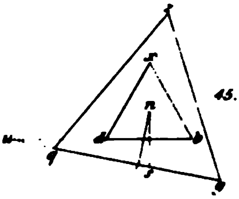

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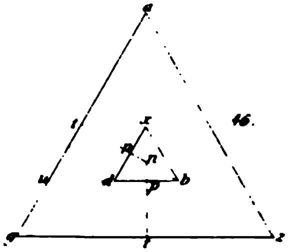

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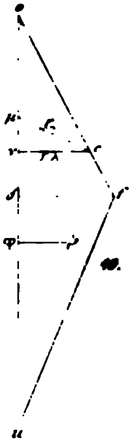

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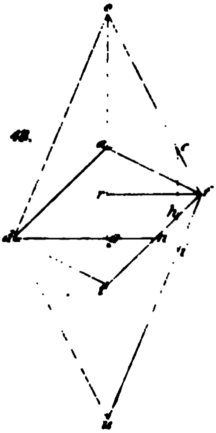

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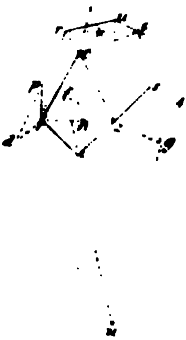

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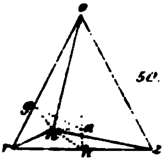

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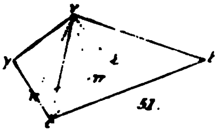

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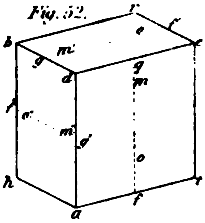

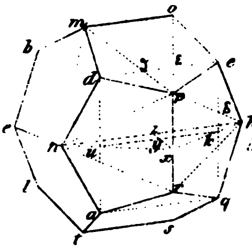

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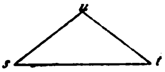

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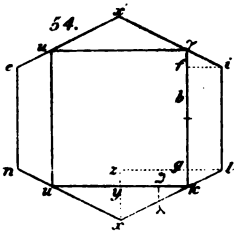

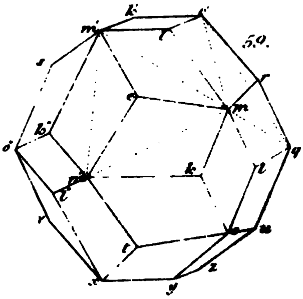

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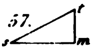

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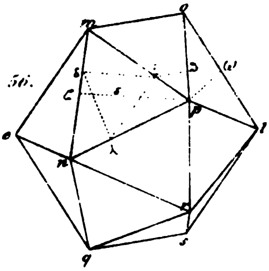

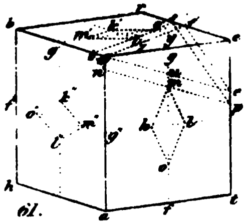

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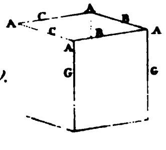

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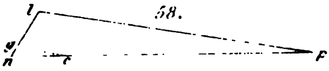

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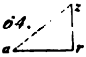

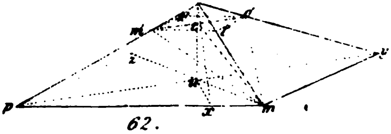

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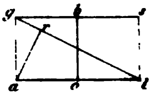

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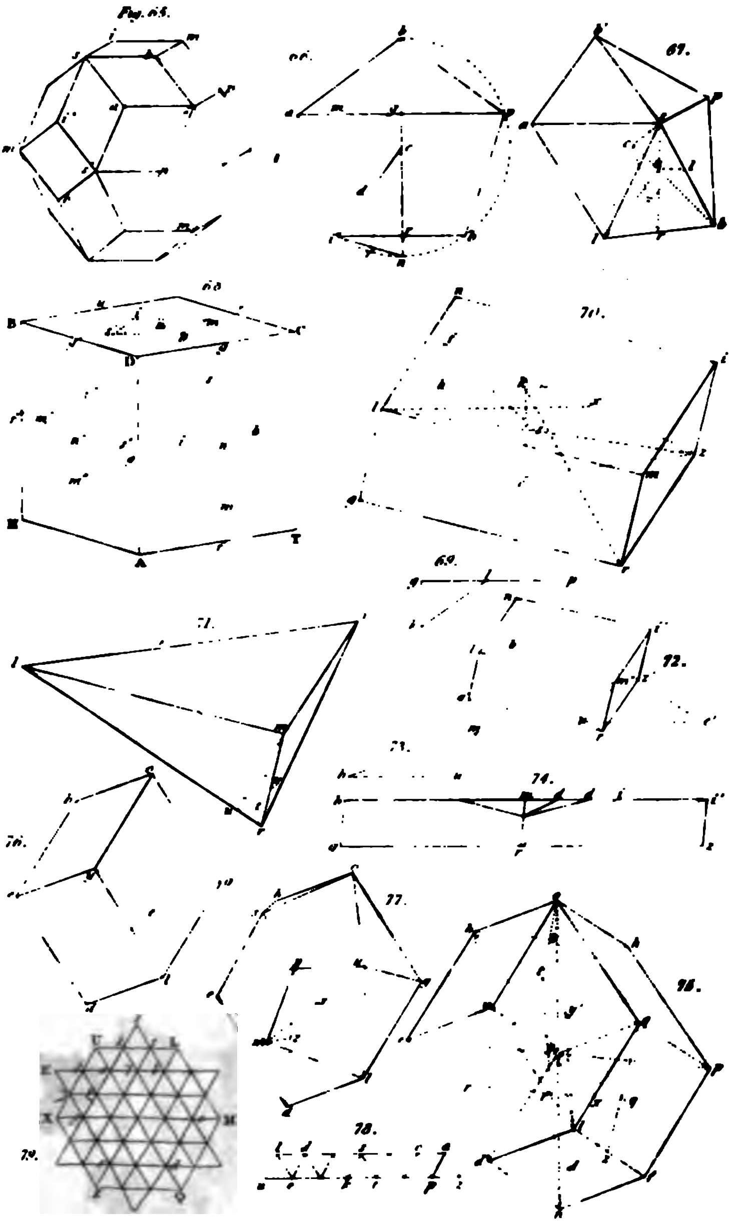
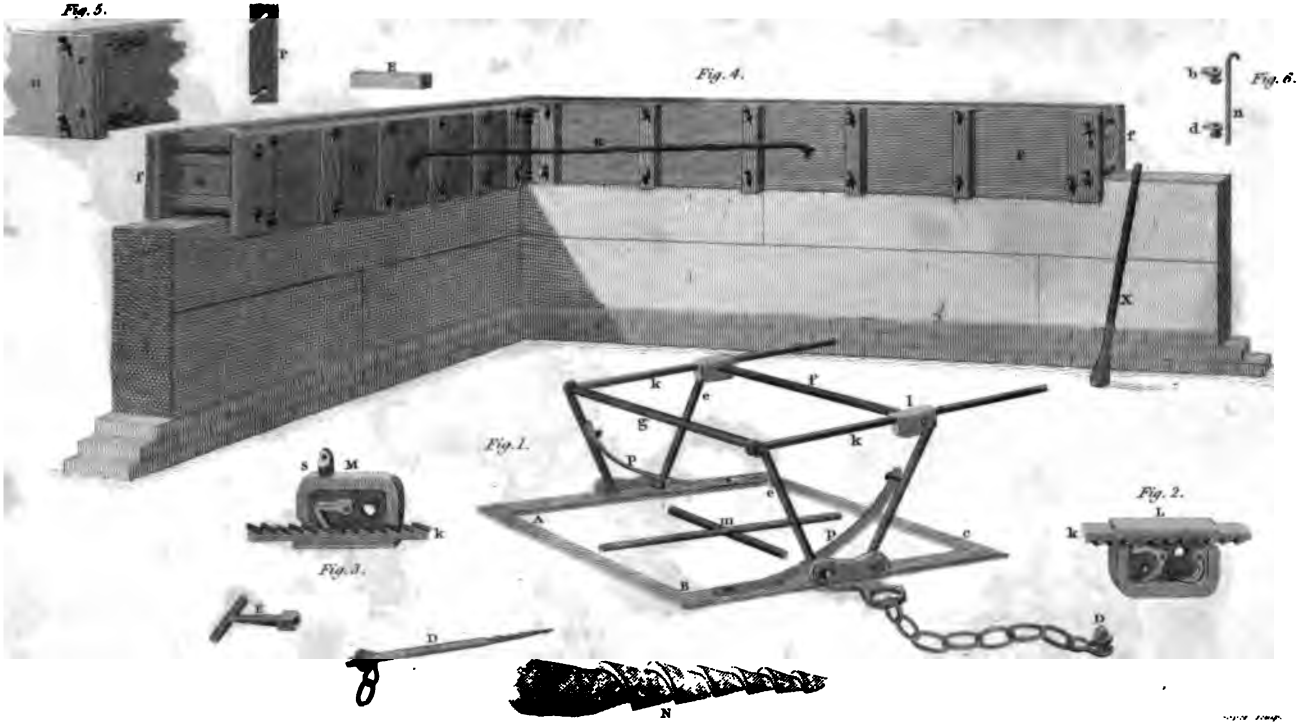


Lowry sculp

Mr. Salmon's method of Building Pisé or Earthen Walls _ & his Man Trap.



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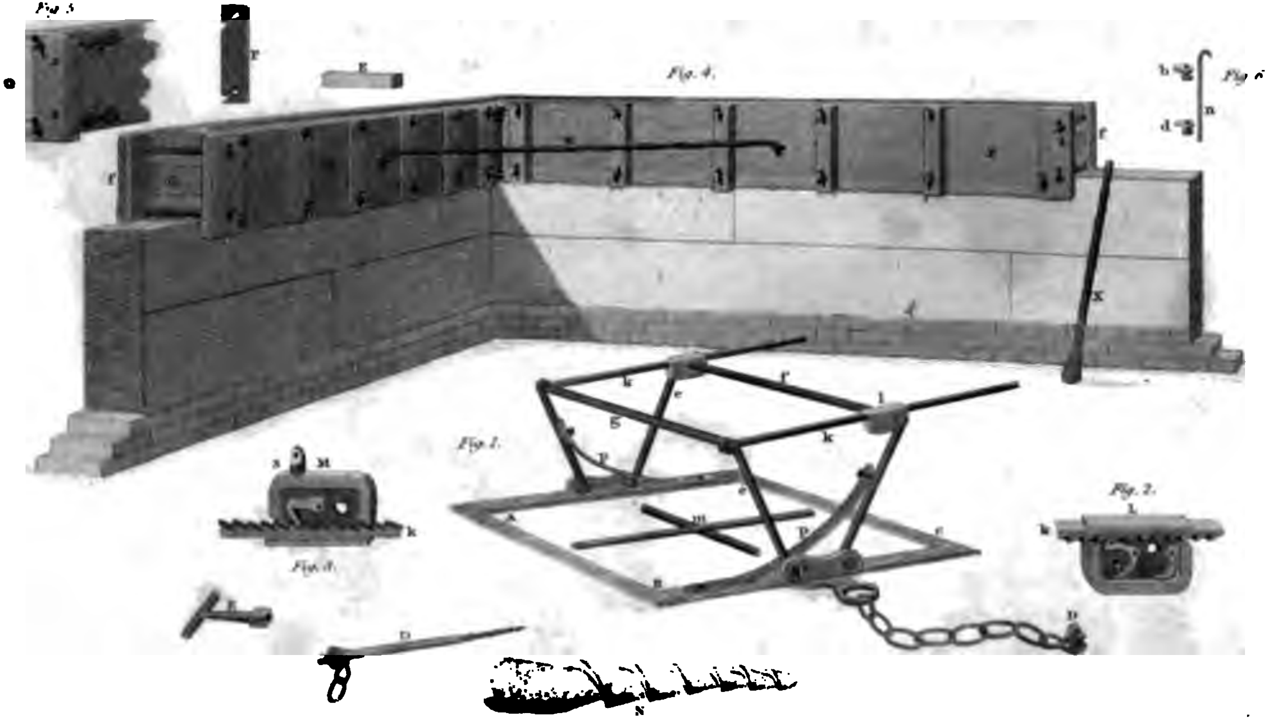

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PLATE XIV.

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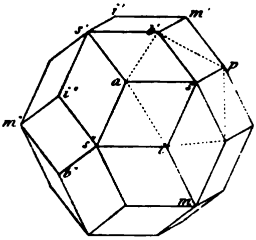

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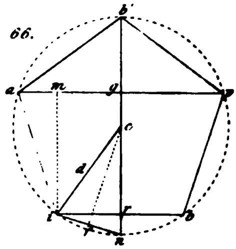

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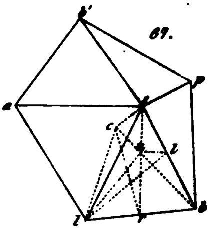

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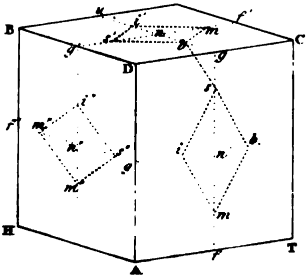

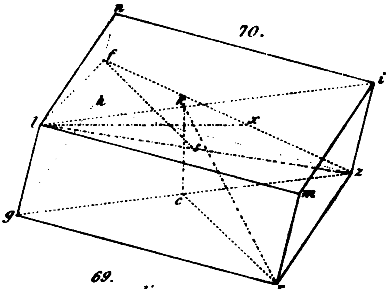

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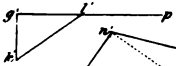

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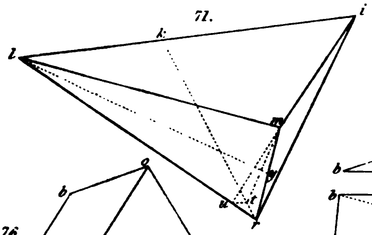

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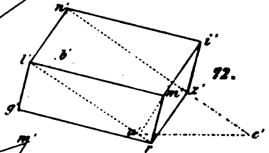

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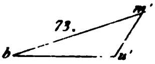

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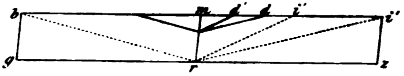

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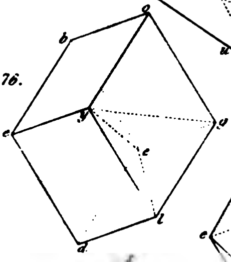

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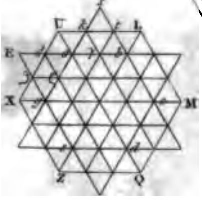

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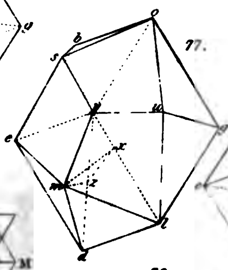

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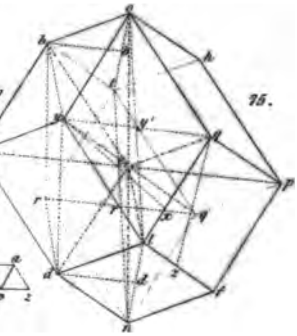

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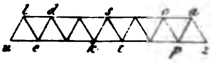
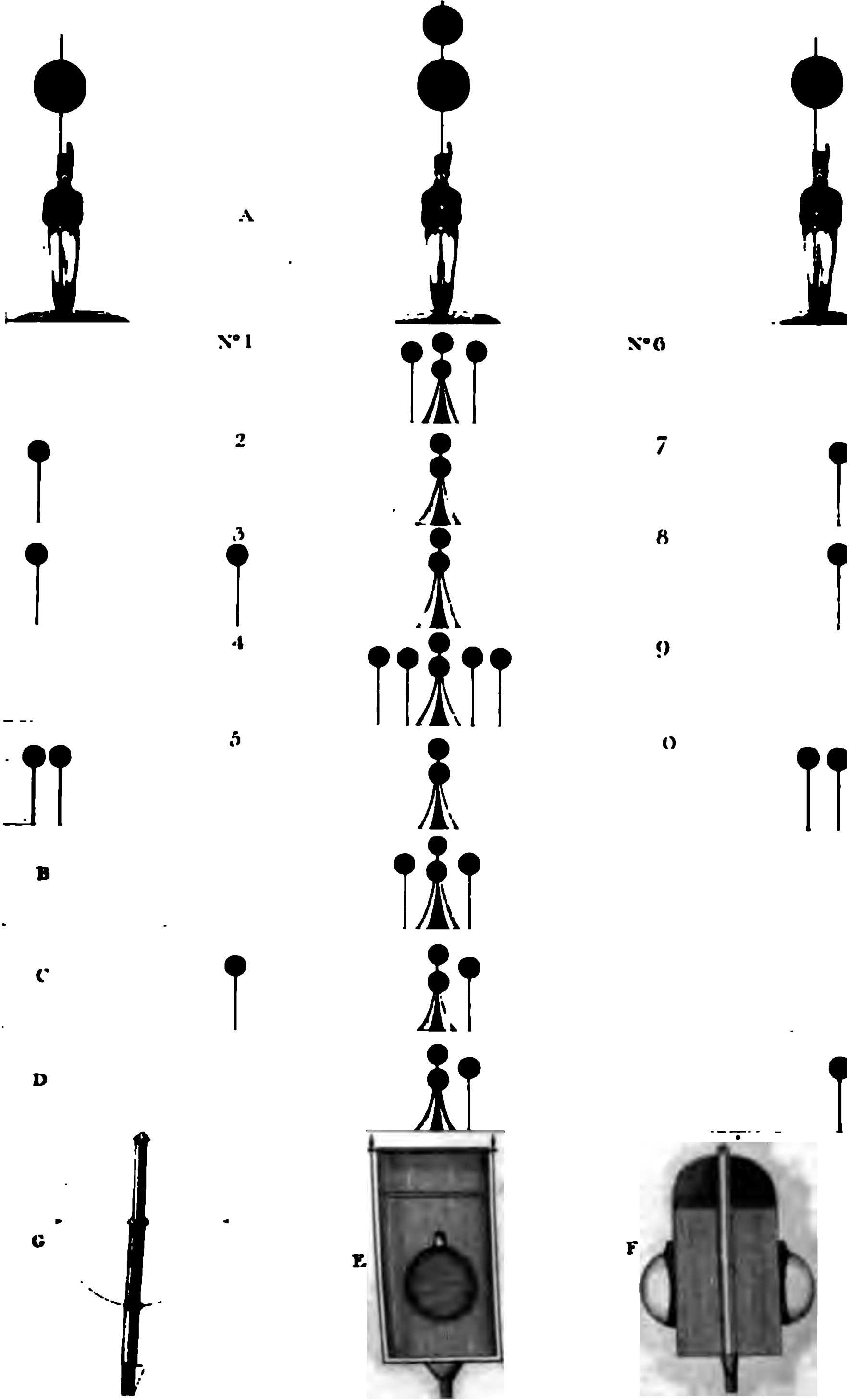


Lowry sculp.

CAMP TELEGRAPH.



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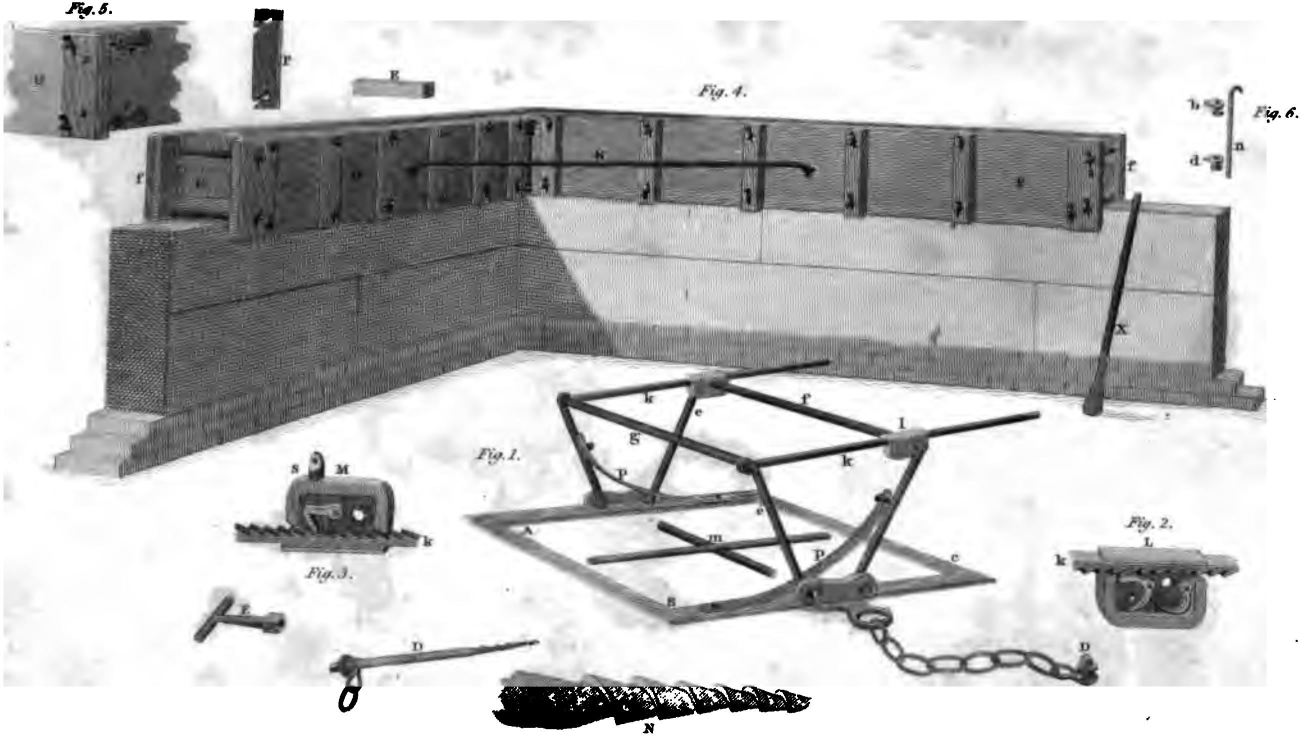
Mr. Salmon's method of Building Pisé or Earthen Walls — & his Man Trap.



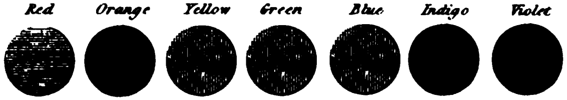


Fig. 1.

Fig. 2.

Fig. 3.

Fig. 4.

Fig. 6.

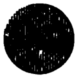
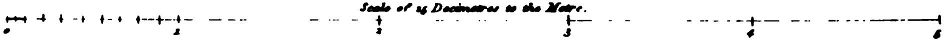
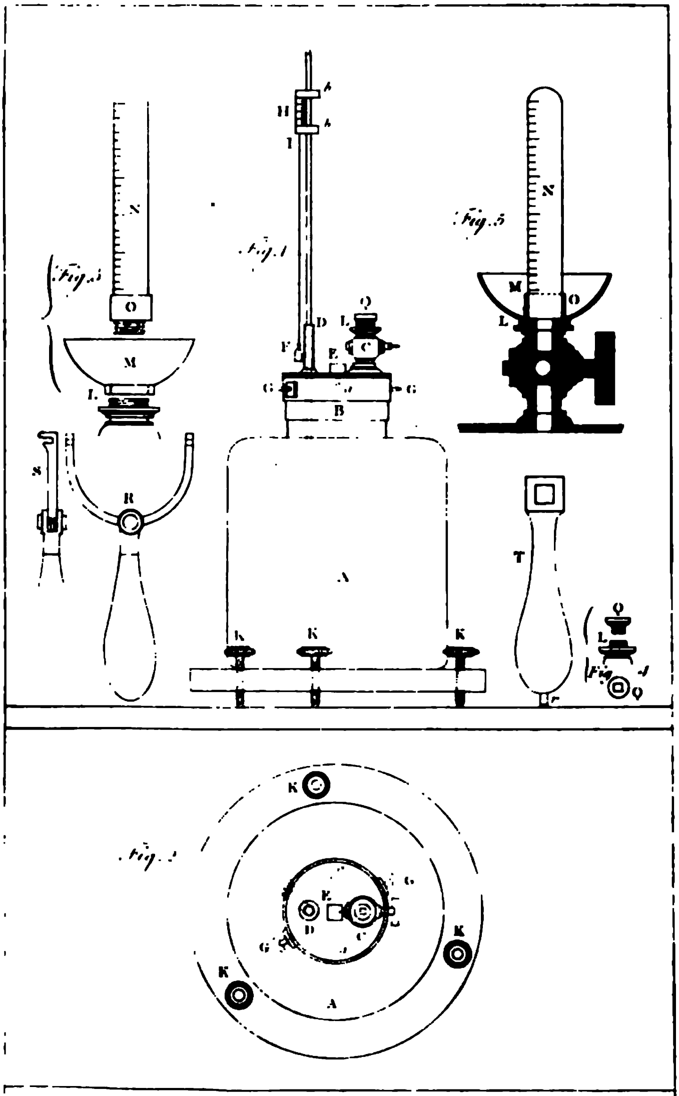


Fig. 5.

Scale of 1/5 Decimetres to the Metre.



Cooper sculp.



Scale ½ Decimeter to the Metre

[illegible] Decimeters.

[illegible] Inches

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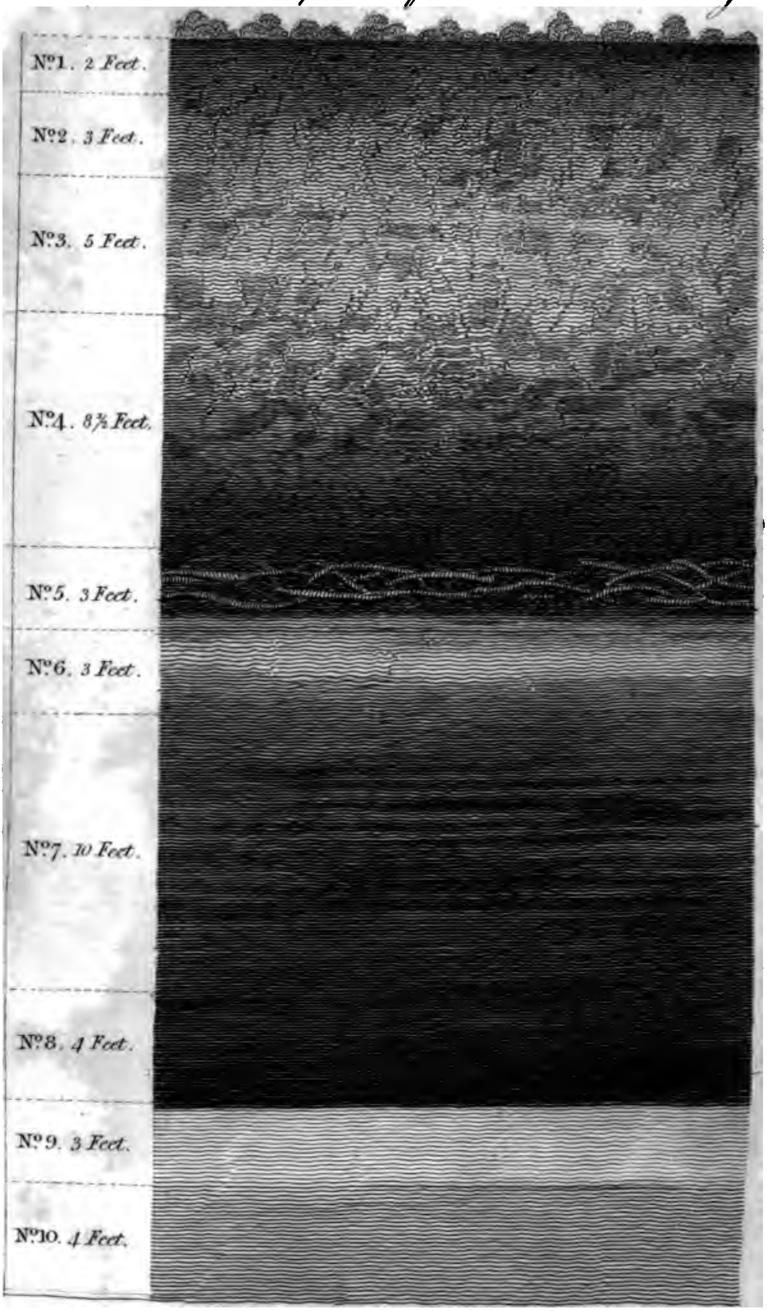
Section of a Turf-bank in Timahoe Bog.



No 11. Clay mixed with limestone Gravel.
Depth unknown.

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